AN EXAMINATION OF GLOBAL CONVENTIONALISM

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ABSTRACT

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A number of issues are addressed in order to evaluate the case for a significant form of global conventionalism (GC). A variety of positions which have been labelled conventionalist - the Duhem thesis, local conventionalism, trivial semantic conventionalism (TSC), and the weltanschauungen view - are characterized and their relations to GC are explored. The relations between GC on the one hand and instrumentalism, realism, and the model-theoretic approach, on the other, are also analyzed. The attempt is made to produce a more explicit and more rigorous formulation of the claims inherent in GC. GC is initially characterized, less precisely, as the doctrine which asserts that there may exist alternative theories which handle exactly the same data but which are genuinely distinct. A formal concept of intertranslatability is subsequently introduced and defended as a criterion of theory synonymy. GC is apparently not supported on any general logical grounds. Furthermore, there exists no pair of theories instantiating GC. It is unlikely that such a pair of theories could be constructed. A brief assessment of the major arguments for conventionalism which have been constructed with reference to theories that convey a physical geometry (the context where GC is most plausible) is presented. These arguments actually appear to support nothing stronger than TSC. The conclusion

drawn is that no sound argument for GC has ever been presented. The popularity of the doctrine apparently owes a great deal to certain conceptual confusions operative in the literature. These are discussed.

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PREFACE

It is our view that the literature devoted to the topic of global conventionalism betrays some serious conceptual confusions. An indispensible preliminary step in assaying the doctrine thus involves the clarification of the precise claims advanced by the doctrine. Hence, the first three chapters of this thesis are devoted largely to the task of conceptual clarification, a task which proceeds on several fronts.

In Chapter I, a number of other doctrines and conjectures which have been tagged with the label of conventionalism are characterized, and their relations to the global variety are analyzed. Global conventionalism is characterized at the outset as the doctrine which asserts that there may exist alternative theories which handle exactly the same data, but which are genuinely distinct. Admittedly, this account is itself an inadequate one. In particular, the notion of "distinctness" requires further elaboration. However, the reader will be asked to forebear and to content himself, for a time, with this first approximation. It is adequate for our purposes in Chapter I and Chapter II.

The other forms of conventionalism which are identified are: trivial semantic conventionalism, local conventionalism, the Duhem thesis, and the weltanschauungen analysis of scientific theories. Of these, perhaps the most important to consider are the Duhem thesis and trivial semantic conventionalism. The conclusion drawn in Chapter I is that the four additional varieties of "conventionalism" referred to are quite distinct from the global brand. This contradicts a prevalent notion that there exists a close conceptual or logical relationship between the Duhem thesis and global conventionalism. This association assumes two distinct forms. In some instances, the doctrines are simply conflated: the Duhem thesis is taken to represent an assertion of global conventionalism. In other places it is merely maintained that the Duhem thesis affords some support for global conventionalism.[1] According to the analysis put forward in Chapter I, both forms of association are misconceived.

An even more serious confusion involves the failure to distinguish adequately between global conventionalism and trivial semantic conventionalism. In Chapter III, a purported "refutation" of global conventionalism is discussed. In this particular case the doctrine which is represented as global conventionalism is apparently none other than trivial semantic conventionalism. In other instances the confusion of trivial semantic conventionalism with global conventionalism has lead to the mistaken evaluation of the latter as true but entirely trivial.

With respect to the weltanschauungen analysis of science, the term "conventionalism" appears rather misleading. However, we argue that it is possible to formulate a version of global conventionalism within the constraints imposed by this point of view. The relevant restriction is the denial of the observational-theoretical dichotomy.

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[1] cf. John Losee, A Historical Introduction to the Philosophy of Science, (Oxford: Oxford University Press, 1980). "The Conventionalist point of view received further support from Pierre Duhem's analysis of the disconfirmation of hypotheses.", p. 165.

Nonetheless, a basic premise of this thesis is that the distinction does stand, and we proceed to locate the potential for conventionalism in the theoretic portion of our theories.

In Chapter II, we attempt to clarify the concept of global conventionalism along another dimension. Here we explore the relations between global conventionalism, on the one hand, and instrumentalism, scientific realism, and the model-theoretic approach to the analysis of the cognitive status of theories, on the other. The traditional association of global conventionalism with instrumentalism is rejected, and we argue that global conventionalism is neutral with respect to these other points of view. The possibility that global conventionalism may actually be a semantic, rather than an epistemological, claim is raised. The appropriateness of formal approaches to the issue of conventionalism - both a semantic approach and a purely syntactic approach - is discussed.

Chapter III represents an attempt to crystalize the concept of global conventionalism in a precise and rigorous formulation. To this end we invoke certain formal concepts of model theory. The major task undertaken in this chapter is that of providing a rigorous account of what is intended by the assertion that two theories are "distinct". Four different formal criteria of theory synonymy are formulated and subjected to critical scrutiny. A formal criterion of strict inter-translatability is selected as the appropriate one and is subsequently defended against recent criticisms. A complete and rigorous formulation of the doctrine of global conventionalism incorporating this account of theory synonymy is presented. In light of this formulation, and the arguments which lead up to it, the doctrine of global conventionalism, while not refuted, appears highly implausible. It is certainly not supported on any general logical grounds. Nor, we maintain, has any pair of theories which instantiates the doctrine ever been constructed.

In Chapter IV, we proceed to examine the basic arguments adduced in support of conventionalism with regard to theories of space and time, or spacetime. This is the area in which the doctrine enjoys its greatest degree of plausibility. We argue that these arguments ultimately support nothing stronger than trivial semantic conventionalism.

CHAPTER I

A TAXONOMY OF THE VARIOUS SPECIES OF "CONVENTIONALISM"

The term conventionalism is used variously in the philosophy of science. The collection of doctrines and conjectures which have acquired the label constitutes a somewhat mixed bag. It contains a cluster of closely related assertions. But it also numbers several claims which are significantly disparate - to the point where the fact that they are identified with the same label is somewhat misleading. This is a circumstance which is to be explained largely on the basis that different varieties of "conventionalism" have been advanced as responses to distinct (though not necessarily unrelated) queries. These questions include the following: (1) Can theories be conclusively refuted? (2) Can isolated hypotheses be falsified? (3) What is the cognitive status of theories? (4) What is the cognitive status of a particular theory or type of theory (say, a theory of space and time or of spacetime)? (5) What is the cognitive status of a particular component or element in some specific theory? (6) What is the nature of scientific progress?

We are not for arguing that these questions have no bearing on one another. In a comprehensive and consistent philosophy of science they generally do and, indeed, must. But the situation is thus: the discipline has been more or less preoccupied with one or another of these issues at separate moments in its history. Furthermore, even regardless of this historical dimension, the reflections of different individuals have tended to form the focal points for the debates centering on these various matters. So it is hardly surprising that several theses have been advanced in the literature which all display features which warrant application of the conventionalist badge, and yet which are sufficiently distinct that the denial of one may not necessarily entail the denial of another. Given the ambiguities inherent in current usage of the term, it will be useful to provide a catalogue of the major forms of conventionalism, and to comment briefly on each.

1. Global Conventionalism

The type of conventionalism which interests us and which this thesis concerns may be called "universal" or "global" conventionalism.[1] In brief, it is the doctrine which asserts that theories are conventions. By this characterization it is intended that they are merely constructs and that their formulation is consequently arbitrary in a sense which is significant from an epistemological standpoint.

A popular dictionary of modern thought defines this type of conventionalism as the doctrine which "holds that scientific theories are not mere summaries of passively received experience but are free

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[1] I have appropriated the terminology from Paul Horwich's article: "How to Choose between Empirically Indistinguishable Theories," Journal of Philosophy, vol. 79, no. 2 (February, 1982), pp. 62-77. His characterization of the doctrine, however, resembles trivial semantic conventionalism more closely than it does the account I shall develop.

creations of the mind for the simplest and most convenient interpretation of nature."[2] With the incorporation of the expression "free creations" this characterization may convey something of the essential spirit of conventionalism, and it may be acceptable as a first approximation. However, it is altogether too vague to serve adequately as a working definition for a meaningful discussion of the subject. For example, that scientific theories are not "mere summaries of passively received experience" is a notion so widely accepted as to be virtually axiomatic. But this existing orthodoxy stops well short of the radical claims endorsed by conventionalism. Moreover, the precise nature and boundaries of the "freedom" which is alleged to be inherent in the process of elaborating a theory is left unclarified. Clearly, if we are to proceed to the task of evaluating claims made by conventionalism then a sharper characterization is required, one which spells out these claims more explicitly. A few preliminary remarks by way of sketching in the outline of a more precise account are called for, although we intend to leave the detailed formulation of this account to the proceeding sections of this thesis.

Global conventionalism, it has already been remarked, asserts that scientific theories are of an essentially arbitrary character. The doctrine maintains that theories as a whole or as systematic entities are merely constructs and that as such they are arbitrary in an

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[2]Quinton, A. "Conventionalism," in The Fontana Dictionary of Modern Thought. Alan Bullock & Oliver Stallybrass (eds.), (London: Collins, 1977).

epistemologically significant sense. The notion that theories are constructs in a radical sense predates the modern formulation of global conventionalism. Presumably it climaxed first with Kant's analysis of the categorical framework presupposed by Newtonian physics. But Kant, of course, expressly denied that this framework of pivotal concepts which he had identified was arbitrary. Indeed, the whole point of his attempted "transcedental deduction" of the categories was to establish this framework as the only possible one, as uniquely necessary to the scientific enterprise. Conventionalism, in contrast, carries things a step further than Kant's brand of a priorism by insisting that theories be construed as arbitrary constructs.

This point of divergence cannot be overemphasized. It entails that on the conventionalist view one must allow in principle for the existence of alternative theories, theories which will be such that there can be no cognitive or methodological rationale for choosing any particular alternative over its competitors. Otherwise there is no sense to the notion that theories are arbitrary conventions. The way to broach the issue of conventionalism is patently via the question of the existence, or the possible existence (if none are in evidence), of such alternative theories. This much seems obvious, but nonetheless it is a matter which has not been addressed in a fully responsible manner in the literature devoted to the subject of conventionalism - as we shall see later.

In brief, the alternative theories alluded to must satisfy two principal requirements. First, they must yield exactly the same observational consequences or, in the popular jargon, must "save the same phenomena". So the only grounds for indicating a preference for one particular alternative would necessarily have to be non-cognitive ones. Such considerations might be aesthetic or pragmatic. Some alternative theory might be rather more elegant than its available counterparts, or it might contain a more efficient algorithm. But the point is that no such advantage would allow us to infer the cognitive superiority of a given theory from among a set of conventional alternatives - at least certainly not directly. Unless this first requirement is satisfied by alternative theories one can make no sense of the notion that they are conventional, that their status is, in an essential and important sense, arbitrary. It should be remarked that this condition must be satisfied rigorously. Approximate or even virtual empirical equivalence will not suffice to sustain conventionalism.

The second requirement which must be met by these theories is that they must be genuinely distinct. There must be no translation procedure, no mechanical operation, available for transforming a theory into one of its putative alternatives. If such an apparatus exists, then the alleged alternative theories are not alternatives in any epistemologically significant sense, and their existence does not substantiate the claims advanced by universal conventionalism. They are, in such case, not truly distinct theories at all, but rather merely semantic or syntactic variants of a single theory.

Much more needs to be said about these requirements. The central notions of "distinctness" and of "observational equivalence" need to be discussed more thoroughly as they are somewhat problematic. However, as we indicated previously, we prefer to defer further consideration of these topics. For the moment, we shall procede to complete this brief classification of the remaining varieties of conventionalism, and to summarize their relations to the global brand.

2. Trivial Semantic Conventionalism

There is, first of all, the variant called trivial semantic conventionalism (TSC). This form of conventionalism rests upon the circumstance that our assignment of meaning to our linguistic signs or symbols involves an element of definition, stipulation or convention. It is based, in other words, on the obvious fact that we can connect our concepts with whatever linguistic signs we choose. According to this version, our physical theories - and, in fact, all forms of discourse - are conventional in the rather uninteresting sense that their linguistic form or mode of expression might have been different from what it is. In that case, the sentences of two theories which are trivial semantic variations of the same theory may be different. And if the same sentences occur in both versions, they may have different truth values. But this does not mean that they are truly distinct or that they are arbitrary in any important sense. To invoke a venerable philosophical distinction, in such a situation the sentences of the theory, its overt linguistic expression, may have been altered; but it will still comprise the same propositions.

For our purposes, the notion of TSC is an important one. One of the points we shall try to secure later is that certain arguments which purport to establish the significantly conventional status of a particular class of theories, namely theories of space and time or of spacetime (theories which incorporate a physical geometry), really do not ground anything stronger than TSC. TSC itself would appear to be unassailable, but of course it is utterly trite.

3. Local Conventionalism

Another type of conventionalism may be called "localized" conventionalism.[3] This is the version which asserts that "certain assumptions are made in empirical science for purposes of convenience only."[4] There is little difficulty with the notion that scientific theories contain some definitions, some elements which have a conventional status. There are numerous examples of such assumptions: the use of the infinitessimal calculus rather than the calculus of finite differences; the practice of employing the real number system rather than the rational numbers; the decision to treat physical fields as mathematical fields; the technique of calculating the mass of an irregular solid by integration, which assumes that the solid is actually continuous - to name but a few that come readily to mind. All of these are clear cut cases of components or dimensions of physical theory which are incorporated by stipulation.

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[3]The terminology, again, is borrowed from Horwich, op. cit.

[4]Suppes, Patrick, An Introduction to Logic (Princeton: A. Van Nostrand Co., 1957), p. 295.

Within the context of the investigation of the conceptual foundations of a particular theory, the identification of specific elements which are of a stipulative or definitional status is a legitimate and worthwhile philosophical exercise. However, it should be understood that, in general, no conclusion concerning the cognitive status of scientific theories as a whole follows from the admission that they invariably contain a certain number of specific conventional elements. In particular, it certainly does not follow from the circumstance that a theory contains a certain number of definitions that the entire theory is simply a stipulation. In other words, local conventionalism does not entail the universal variety. However, this general point aside, it must still be acknowledged that the demonstration of the conventional status of any significant and intuitively non-conventional elements of science may, even if it cannot amount to a proof of universal conventionalism, add to the plausibility of that doctrine.

There is an important difference between local and universal conventionalism regarding the strength of the arguments which can be adduced in support of their respective claims that is worth remarking. The types of claims made in the former context are, in principle at least, resolvable in a conclusive manner. This stems from the circumstance that while a propitious convention can, for example, provide for a more efficient algorithm, it cannot create any new content that will be endowed with factual status or significance. Following this principle of the non-creativity of definition, the acid test of the definitional status of a claim is simply whether it contributes any further factual content to the system of which it is a part.

It is especially important to bear in mind that appearances can be misleading. For example, the classical postulate "f = ma" has, at first blush, the semblance of a definition. However, it is rather a theoretical principle which, in conjunction with the remainder of the body of theory of which it is a part, carries factual significance. Superficial appearances must be discounted and the acid test of definitional status brought into play. When the test is applied, it becomes apparent that this principle is by no means a mere definition.

Now, of course, theories do not always oblige us by wearing their definitions on their sleeves, as it were, and it is sometimes considerably more difficult to establish the definitional status of some element in a theory than the example of the cases cited above may suggest. Perhaps a good illustration of the degree of difficulty that may be encountered in trying to fathom the status of a particular claim asserted by a theory is that provided by the literature regarding the alleged conventionality of distant simultaneity in the Special Theory of Relativity (henceforth, STR).

Here the spectre of conventionality arises in the following way. Suppose we wish to determine whether events occuring at two points, A and B, which are spatially separated, happen to be simultaneous. We require synchronized clocks at the two locations for this task. The usual method of synchronization consists in this. A light ray is emitted from source A at time t_1. This signal is received at B at time t_2 and is immediately reflected back to A where it arrives at t_3. On the assumption that the light signals travelling paths AB and BA have the same velocity, $\underline{c}$, we adjust our clock at B so that $t_2 = t_1 + \frac{1}{2}(t_3 - t_1)$. The clocks at A and B are then said to be synchronized.

The conventionalists, chiefly in the persons of Reichenbach and Grunbaum, have argued that the determination of synchrony in this manner is essentially a convention. Their reasoning runs as follows. Standard synchronization would assume a factual status only if the isotropic character of the round trip velocity ABA were a matter of fact. However, to compare the velocities of the one-way signals AB and BA we require previously synchronized clocks. Hence any attempt to measure experimentally the one-way velocity of a light ray presupposes the method of coordinating our instruments outlined above, a method which already proceeds on the assumption of the isotropy of light.

On the basis of this predicament the conventionalist argues that any attempt to establish simultaneity by the standard method is necessarily circular, and that the only way to escape this circularity is _via_ a conventional stipulation of the principle of isotropy, a stipulation which of course strips the principle of any factual significance. To bolster his position the conventionalist then proceeds to argue that there are any number of alternative conventions which, epistemically speaking, are on an equal footing with standard synchronization. In fact, any definition of the form $t_2 = t_1 + \varepsilon(t_3 - t_1)$, where the parameter ε is on the open unit interval, will suffice. There exist infinitely many such definitions and, according to the conventionalist, they are empirically indistinguishable.

Conventionalists do not all speak with the same voice and Grunbaum tries to distinguish his particular brand of conventionalism from Reichenbach's by asserting that his is based on ontological considerations as distinct from any principle of verification. It is, he emphasizes, a matter of fact that ε represents an upper limit on signal propogation, and hence it is a matter of fact that there is no actual or objective relation of simultaneity in the universe, at least in the universe described by STR.[5]

The conventionalist view is supported by the findings of J.A. Winnie.[6] Winnie set himself the task of developing the kinematics of STR in a framework which treats ε as a variable. The ultimate result of this exercise was a generalized form of the Lorentz transformation. Winnie concluded from his enterprise that a theory which is empirically equivalent to STR may be constructed, a theory which posits ε as a variable.

The ensuing literature is too bulky to comment on exhaustively, nor is any detailed reportage necessary for our purposes. However, a few comments on general direction and tendencies which have appeared in the debate will be useful in pressing our previous point.

Arguments against the conventional status of simultaneity in STR revolve, appropriately, around the possibility of specifying a method

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[5] A. Grunbaum, "Relativity Theory, Philosophical Significance of," Encyclopedia of Philosophy, vol. 7.

[6] S. Winnie, "Special Relativity Without One-Way Velocity Assumptions", Philosophy of Science, 37 (1970).

of measuring the velocity of a one-way light signal which is independent of standard synchronization and non-circular. One proposal in this vein which has been much discussed is the method of slow clock transport, an approach which has been argued extensively by Bowman and Ellis.[7] This approach exploits a well known factual prediction of STR concerning the behaviour of moving clocks. According to STR, a moving clock is retarded relative to one located in a stationary frame. Just how slow the moving clock runs is a function of its velocity and the distance it travels.

Suppose, then, that we have two clocks, C_a and C_b, located at points P_a and P_b, respectively, in an inertial frame. A third clock C_t may be locally synchronized with C_a and transported to P_b at some specific velocity, v. Assume the departure time is 0. Then let t represent the arrival time as recorded by C_b, and let t' stand for the arrival time as recorded by C_t (t', of course, is the proper-time of C_t's trajectory). Then, according to STR, $t' = t(1-v^2/c^2)$. Now the salient point is that regardless of the distance separating P_a and P_b, the difference t-t' may be made arbitrarily small: as the velocity v of C_t's trajectory tends to zero, so too does t-t'.

The idea, then, is to invoke slow transport synchrony as an independent and non-circular test of standard signal synchrony and the principle of isotropy. The debate does not end here, however. Grunbaum has objected that slow clock transport is also inherently conventional,

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[7] B. Ellis and P. Bowman, "Conventionalism in Distant Simultaneity", Philosophy of Science, 34 (1967), pp. 113-136.

insofar as it must presuppose a time metric, a conventional standard for comparing temporal intervals.[8] Ellis subsequently published a reply to Grunbaum in which he argued that there may be substantial physical reasons for selecting a certain convention over other available alternatives.[9] But this simply seems to miss the point of the conventionalist's position, and presumably the latter's response to this tack would simply be to the effect that a good physical reason which is yet not sufficient to force a particular choice on us is not enough to make that choice a factual rather than a conventional one.

While this debate is far from being resolved, it must be remarked that it does not appear to be a theorem of STR that a one-way velocity cannot be measured.[10] Hence, Grunbaum's peculiar comments aside, the possibility that a satisfactory experiment to measure a one-way velocity may be devised cannot be precluded. Indeed, proposals for such experiments continue to appear in the literature of physics.[11]

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[8] A. Grunbaum, "Simultaneity by Slow Clock Transport in the Special Theory of Relativity", Philosophy of Science, 36 (1969).

[9] B. Ellis, "On Conventionalism and Simultaneity - A Reply", Australasian Journal of Philosophy, 49 (1971).

[10] R.B. Angel, Relativity: The Theory and its Philosophy (Oxford: Pergamon Press, 1980) p. 129.

[11] The most recent is presented by P. Kolen and D.G. Torr in "An Experiment to Measure the One-Way Velocity of Propagation of Electromagnetic Radiation", Foundations of Physics, vol. 12, no. 4 (1982). For a discussion of past proposals to verify standard clock synchrony by independent non-circular methods, in particular the Jackson-Pargateer proposal and the Feenberg "rotating shaft" thought experiment, see Peter Ohstrom's "Conventionalism in Distant Simultaneity", Foundations of Physics, vol. 10, nos. 3/4 (1980). These proposals have proved to be flawed.

While no completely adequate experimental design has been advanced to date, there is no reason to succumb to despair yet. One thing which should be noted is that, the question of empirical ramifications aside, standard signal synchrony is not conceptually identical with any of the possible forms of non-standard synchrony. In particular, it may be emphasized that the former is an equivalence relation, while any version of the latter will fail to be such; nonstandard forms of synchrony are intransitive. Considerations of this sort may buoy hopes that the empirical non-equivalence of standard and non-standard synchrony may eventually be established categorically.

It is also worth remarking that much of the discussion of this issue has been set within the framework of the kinematics of STR, isolated from the rest of physical theory. The motivation and justification for this approach is not hard to fathom: it only makes sense to investigate the possibilities for distinguishing between standard and non-standard synchrony within STR before complicating matters by widening the scope of the discussion. However, it may well be that if assumptions concerning the behaviour of light signals are carried over to other regions of physical theory then consequences might be teased out which will reflect the factual status of those claims. Some work in this direction has already been carried out by W.C. Salmon, who has traced the implications of assumptions concerning the one-way speed of light through additional physical theory, with particular attention to electrodynamics.[12]

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[12] W.C. Salmon, "The Physical Significance of the One-Way Speed of Light", Nous, 11 (1977).

In any event, the point is that in principle any claim to the effect that a certain element in a theory is conventional may be rigorous[illegible] assessed in a straightforward manner. To refute the claim one has only to identify the points at which the adoption of alternative components will betray different empirical commitments on behalf of the theory. If, on the other hand, the claim is accurate, one can establish this by ascertaining that the empirical content of the theory is not altered by removing or replacing this component.

In the case of universal conventionalism, the situation is quite different. There is no convenient touchstone for evaluating the cognitive status of theories in their entirety which might be considered analogous to the aforementioned principle whereby we may scrutinize individual components of a theory. In the context of global conventionalism we are forced to consider, ultimately, questions of a more general and more impenetrable kind concerning, for example, the way in which theoretical terms acquire their meaning. In the case of theories of space and time, which convey a physical geometry, the question as to how geometrical terms stack up against other theoretical terms must also be dealt with - as must also the matter of the foundations of geometry (Do geometrical terms have an "ontology" of their own?). These questions are not so easily settled; however, a final and completely informed assessment of global conventionalism will presumably depend on our answers to questions of this sort. What this means is that the arguments either for or against global conventionalism may be less than conclusive, that the most we may reasonably expect or demand are plausibility arguments. Such arguments fall short of demonstration but they may be convincing nonetheless.

4. The Duhem Thesis

So much for local conventionalism. This brings us to a fourth variety of conventionalism, one which is intimately associated with the name of Pierre Duhem. The Duhem thesis is to the effect that isolated hypotheses cannot be refuted, that in science the results of observation inevitably reflect not on any single hypothesis but rather only on the theoretical ensemble concerned as a whole. In The Aim and Structure of Physical Theory Duhem wrote:

> The Physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses; when the experiment is in disagreement with his prediction, what he learns is that at least one of the hypotheses constituting this group is unacceptable and ought to be modified; but the experiment does not designate which should be changed.[13]

Stated so baldly, the thesis seems false. The suggestion appears to be that when a theory is refuted by experimental results, the theory in its totality must be blamed; it is not possible to isolate and identify some precise hypothesis or set of hypotheses to which blame may be attached from within the group of hypotheses comprising the theory. This thesis would be true in general only if the postulates of physical theories tended invariably to betray an unlikely and, indeed, undesirable trait, namely logical inter-dependence. In point of fact the

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[13] P. Duhem, The Aim and Structure of Physical Theory, trans. by P.P. Wiener, (New York: Atheneum, 1974) chapter 7, p. 187.

postulates of physical theories tend by design more frequently to be independent. It follows we may expect that in some cases at least it will be possible to single out the hypothesis, or subset of hypotheses, responsible for the failure of a theory. One can accomplish this quite rigorously by providing an independence proof, the simplest form of which consists in presenting a model which satisfies all the axioms of the theory concerned except the tainted one(s).[14]

There is, however, another somewhat more generous interpretation which can be attached to Duhem's writings: According to this interpretation the Duhem thesis exploits a certain inductive latitude involved in the testing of hypotheses which results from the necessary utilization of certain auxiliary assumptions that include bridge principles or correspondence rules, statements of initial conditions, and sundry collateral theory.[15] On this interpretation, isolated hypotheses are immune from conclusive falsification not because they cannot be distinguished sufficiently from the other hypotheses contained in the theories in which they appear, but rather because their testing necessitates bringing to bear additional assumptions and further, extraneous,

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[14]Karl Popper, Conjectures and Refutations (London: Routledge & Kegan Paul, 1969), pp. 238-239.

[15]A. Grunbaum, "The Duhem Argument", Philosophy of Science 27, no. 1 (January, 1960).

theoretical groups. The claim here is that fault may be laid at the doorstep of this ancillary apparatus, rather than pinned to a particular constituent hypothesis (or, perhaps, even to the theory as a whole).

On this reading the Duhem thesis amounts to the claim that given a theory T with a constituent hypothesis H, wherever H is threatened by virtue of the role it plays in the derivation of an observation sentence, O, which is contradicted, it (H) may be preserved by altering the set of auxiliary assumptions, A, which figure in the derivation and which are external to T. Schematically we have $\{[(H\&A) \rightarrow O] \ \& O'\} \rightarrow [(\exists A') \ (H\&A') \rightarrow O']$ where again H stands for any constituent hypothesis in a theory, A for the set of auxiliary assumptions, O for some observation sentence which is a consequence of H and A, and O' for the true observation sentence which is incompatible with O.

This thesis appears false, also, or at the very least unwarranted. To assert that A' must exist in all cases where it is required is simply to make that claim. It is an assertion and not an argument. Yet there is no assurance on general logical grounds that the necessary set A' will always be forthcoming.[16] That A' will always exist is apparently something which can only be advanced as an article of dogmatic faith. Philosophies and philosophical doctrines which are thoroughly whimsical, based on presumption rather than argument, have a way of falling out of favour. So, apparently, it has been with this version

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[16] A. Grunbaum, "The Duhem Argument", Philosophy of Science, 27 (1960), p. 77.

of the Duhem thesis.

An alternative reading of Duhem's intentions assimilates his viewpoint to that espoused by Quine and encapsulated in the latter's assertion that "any statement can be held come what may."[17] Wedeking argues convincingly for the position that on this account the Duhem thesis reduces to the trivial proposition that there will exist some language in which H and O' are consistent.[18] This account stops short of the assertion that O' will be non-trivially deducible in this language. There is, he points out, no reason to hold Quine to the further condition that O' must be derivable from any of the true sentences in this language (call it S') other than itself. In other words, Quine is not constrained, in defending his viewpoint, to assert of any particular sentence in S' that it is dependent on some of the other truths of that language. This is presumably entirely as it should be, for here again there is no guarantee on general logical grounds that there will exist any consistent language in which a given observation statement can be non-trivially deduced. Now of course the objection may be voiced that no physical theory ought to contain any observation statement which

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[17] W.V.O. Quine, "Two Dogmas of Empiricism", in From a Logical Point of View (Cambridge, Mass.: Harvard University Press, 1980), pp. 30-46.

[18] Gary Wedeking, "Duhem, Quine and Grunbaum on Falsification", Philosophy of Science, 36 (1969).

cannot be deduced from the theoretical statements of the system. I am not inclined to debate this point; however, as Wedeking replies, this is a requirement of science per se, and the objection simply does not speak to the matter of the language S' and the question of the strength of the requirements by which we may characterize it.

Yet another commentator in the Duhem controversy, Laurens Lauden insists that it is incorrect to assimilate the views of Quine and Duhem.[19] He also rejects the preceding interpretation of the Duhem thesis to the effect that $(\exists A')\,[(H \& A') \rightarrow O]$. On his view, Duhem was actually asserting a weaker thesis: "Duhem is not asserting that every hypothesis can be saved, but only that unless one has proven that it cannot be saved, then it is not falsified."[20] So according to Laurens, the Duhem thesis asserts only the following. Unless we can prove that $\sim(\exists A')\,[(H \& A') \rightarrow \sim O]$, then $\sim O$ does not constitute a conclusive refutation of H, even if $(H \& A) \rightarrow O$. This is indeed a weaker thesis, and consequently not a very interesting one. In fact, it is virtually a truism.

On this interpretation, the Duhemian notion that isolated hypotheses cannot be refuted is certainly saved, for in general it will not be possible to establish that A' does not exist. Actually it will be

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[19] Laurens Laudens, "Grunbaum on the Duhemian Argument", Philosophy of Science, 32 (1965).

[20] Ibid., p. 297.

possible to prove this only in the case where H is flatly contradictory, or where very strong extra-logical requirements are imposed on A'. Doubtless these extra-logical requirements would have to be so strong as to constitute a way of surreptitiously smuggling in the premise that A' does not exist. But in any event, the thesis as it has just been framed is open to the charge of irrelevance. While it is a truism that a hypothesis cannot be conclusively refuted unless it can be proved that there is no way to save it, the scientist is concerned with something somewhat more pedestrian than "conclusive" refutation. In general, he is satisfied with a refutation which is context dependent, the context being constituted by the theory in which the refuted hypothesis occurs. He is, I think, entitled to be satisfied with such a form of refutation, especially as there seems to be a complete lack of support for any of the stronger versions of the Duhem thesis. (Indeed, as a practical matter he would be justified in accepting this form of refutation even if the strongest form of the Duhem thesis, which asserts that any hypothesis can be saved, could be established. In that case the notion of "conclusive" refutation would doubtless have to be abandoned as unintelligible, but the point is that the scientist is concerned with advancing and improving his science and not, first of all, with "saving hypotheses" at any cost.)

It appears, then, that the Duhem thesis holds little interest. The various possible interpretations of the thesis which we have examined have revealed themselves to be false, unwarranted or trivial. None of this, however, has any genuine implications for global conventionalism.

The Duhem thesis, in its non-trivial form, asserts something to the effect that any hypothesis can always be saved. It asserts that compensatory adjustments (adjustments which will enable us to retain the hypothesis concerned) can always be made within the body of auxiliary assumptions and collateral theory which are required in order to generate testable consequences from a theory. One might think, at first, that this amounts to an assertion of global conventionalism. But it does not. There is no version of the Duhem thesis which of itself entails the view that distinct theories may exist which are empirically indistinguishable.

To clarify this point, let us consider an imaginary situation. Suppose we have a theory T which contains a finite number of postulates. And suppose that one of these hypotheses, in conjunction with the necessary auxiliary assumptions and collateral theory, yields a prediction which is refuted by subsequent observations. If some change in the auxiliary apparatus can "save" that hypothesis then the Duhem thesis will have been vindicated in this particular instance. But it by no means follows that there exist distinct theories which handle exactly the same data. This would not be assured even if it were possible that the theory could also be salvaged by replacing the problematic hypothesis with another. In that case we would have the original theory T with a new set of auxiliary hypotheses A' alongside a new theory T' (T, but with H replaced by H', combined with the original set A of auxiliary hypotheses), the two being, let us generously allow, empirically indistinguishable. On the face of it, these are two distinct theories. But (T & A') need not necessarily be distinct from (T' & A). They might only be semantic alternatives, especially in view of the fact that the body of auxiliary assumptions in question includes the rules of correspondence or coordinative definitions which fix the sense of at least some of the terms of the theories.[21] In this case it is not even certain that T and T' need be said to be distinct, since we may decide to include the rules of correspondence as an integral part of the theories, rather than as external assumptions. This is a reasonable measure: it is somewhat arbitrary to insist on excluding semantic rules from the theory for the purposes of philosophical analysis. In fact, this is an understatement. Scientific theories, after all, are not uninterpreted calculi. There is just no such thing as a scientific theory without semantic rules.

In brief, then, no argument for the Duhem thesis, in any of its forms, necessarily works for global conventionalism. Likewise no criticism of the thesis is bound to impugn universal conventionalism. Neither does global conventionalism imply the Duhem thesis, for the former asserts that there are distinct theories which handle the same data and this does not entail that any arbitrary hypothesis can necessarily be saved. Evidently the two doctrines are quite distinct.

5. The Weltanschauungen View

The last form of conventionalism which must be identified is perhaps the most remote from global conventionalism of those considered

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[21] This points to another difficulty with the Duhem thesis in its supposedly non-trivial guise, for one can hardly say that an hypothesis has been "saved" if its sense has been altered.

in this chapter. This is the view of science sometimes labelled conventionalism but more aptly referred to as the "Weltanschauung" view.[22] The chief purveyors of this view have been T.S. Kuhn and P. Feyerabend. As we shall see, it is entirely at odds with the basic precepts of global conventionalism as represented in this thesis.

This school offers a radical interpretation of the growth and development of scientific knowledge. In its Kuhnian incarnation it asserts that the evolution of scientific knowledge does not proceed primarily as a continuous and cumulative process. Rather, science is said to advance by means of a series of catastrophes constituted by "conceptual revolutions," in which old methodologies and conceptual frameworks ("paradigms") that have proven no longer viable are sloughed off by the scientific community to be superceded by radically new ones. The differences between the sciences which exemplify distinct paradigms are regarded as so extreme that they are said to be incommensurable.

The incommensurability thesis lies at the very heart of the Kuhn-Feyerabend interpretation of science. Kuhn seems to believe that a consideration of the history of science is alone sufficient to support his views.[23] However, his critics have rightly countered that the

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[22] Olaf Tollafson is one commentator who uses the term "conventionalism" in conjunction with this view. See his article "Realism, Conventionalism and the History of Science," New Scholasticism 56, no. 3 (Summer, 1982), 292-305.

[23] T.S. Kuhn, The Structure of Scientific Revolutions, 2nd ed. (Chicago, 1970), p. 4.

very interpretation of the history of science, of conceptual change within the discipline, must involve some prior assumptions of a philosophical nature. In this connection it is worth remarking that the conceptual revolutionist's bible, Kuhn's <u>The Structure of Scientific Revolutions</u>, contains surprisingly little in the way of carefully detailed case studies.[24] A good deal, perhaps the greater proportion, of his argumentation proceeds from general considerations in epistemology and the philosophy of language, and anecdote.

In fact, the argument for incommensurability sits squarely on the meaning-variance thesis, which is to the effect that the meaning of the terms appearing in different theories is determined entirely by the syntactical rules and usage implicit in these theories. So according to this view the meanings of terms employed in science are entirely relative to the theories in which they appear. The meaning-variance thesis, if swallowed whole, is sufficient to ground incommensurability, or at least it is when taken in conjunction with another central tenet of the weltanschauungen position which is to the effect that the sharp distinction which empiricists have traditionally posited between observational and theoretical terms is not supportable. Hence the meaning-variance thesis is construed as extending to the entire vocabularies of theories. On this view there are no extra-logical terms whose meanings are preserved through the transition from one theory to another.

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[24]<u>Ibid</u>.

Couched in these terms, the meaning-variance thesis thus contradicts the idea of a neutral observation language, of a separate vocabulary which remains semantically stable despite changes in theory. Without presupposing such a predominantly neutral observation, language theories must appear as incommensurable: there can simply be no relations of consistency, incompatibility, relative confirmation or reduction (deducibility) between them.

Needless to say, radical incommensurability is completely at odds with the notion of global conventionalism as it has been previously characterized, for the latter has been said to address itself to distinct theories which are empirically equivalent. If there are no theory-independent phenomena then plainly our theories cannot be underdetermined by the phenomena: the entire notion is devoid of sense. Within the framework of the weltanschauungen view the very idea that two distinct theories are "about" the same phenomena is beyond the pale.

Kuhn's views are not intended to be taken as a form of conventionalism, at least not on his own terms. Kuhn addresses himself to the history of science. He sees it as advancing in the following fashion. At some point an accepted theory suffers a crisis. It is no longer able to save the phenomena it is "about". Subsequently there is a conceptual revolution, the scientific community adopts a new paradigm, and a new theory is developed which is incommensurable with its predecessor. The new theory, contrary to the tenets of conventionalism, can be preferred on epistemic grounds, even though there is no neutral set of facts which could be referred to in order to make a comparative assessment of the two. This is because the first theory has failed the phenomena on its own terms, while presumably its successor has not yet evidenced that defect.

So Kuhn is appealing to the history of science, claiming that each major advance in the discipline occurs as a result of this process which consists in a theory undergoing a crisis, becoming no longer viable on its own terms, and then subsequently being supplanted by a new theory embodying a novel paradigm. And to this extent, Kuhn is not saying anything that implicitly carries a commitment to conventionalism. However, the question remains whether a form of global conventionalism may be compatible with the weltanschauungen analysis of science, whether one can make sense of the notion of a global conventionalism set in a context in which there is said to be no stable observation language. The answer, it seems, is affirmative. Let us suppose that Kuhn and his advocates are right concerning the incommensurability of theories. Then imagine a situation in which a theory embodying a new paradigm is advanced by a part of the scientific community, while there is an accepted pre-existing theory in place which has not yet fallen into a crisis. By hypothesis, these theories would be incommensurable and the choice between them would have to be regarded as a conventional or non-epistemic one.

It is possible, then, to make sense of the notion of a form of global conventionalism within the constraints of the weltanschauungen view. However, we shall still procede to regard conventionalism as the doctrine which asserts that there may exist theories which are observationally equivalent but distinct, a definition which presupposes a neutral, stable, observation language and which locates the potential for conventionalism in the theoretical portion of our theories.

The justification for this approach is simply that the incommensurability thesis is false. It has been pretty well demolished by a number of arguments. In the first place, there have been carefully detailed analyses which have shown the commensurability of such theories as those of Newton and Einstein.[25] These case studies tend to presuppose a framework of referential semantics which seems entirely appropriate. The arguments for incommensurability trade, we think, in part on an equivocation between two components or types of meaning: sense and reference. The sense we attach to the terms which designate observable properties and processes can certainly be modified or informed somewhat by the way they are subsumed under a particular conceptual (theoretical) framework; but this does not dictate that we can have no assurance that the reference of our observation terms is preserved in the shift from one theory to another.

Of course, the incommensurability thesis also draws on the claim that no useful or legitimate distinction can be drawn between theoretical and observational terms. However, the arguments for this

[25]M. Friedman, "Simultaneity in Newtonian Mechanics and Special Relativity", in J. Earman, C.N. Glymour & J.J. Stachel, eds., Foundations of Space-Time Theories, Minnesota Studies in the Philosophy of Science, vol. 8 (Minneapolis, 1977). Also, R.B. Angel's "The Commensurability of Classical and Relativistic Mechanics", chapter 6 of his Relativity: The Theory and its Philosophy (Oxford: Pergamon Press, 1980).

claim, arguments which were once fairly well received, have not stood the test of time. The pendulum has since swung back and the observational-theoretical bifurcation has been strongly reasserted. The principle argument adduced against the distinction, the argument from a continuum of cases that extend between detection by direct observation and by inference, establishes at best that the distinction is a somewhat vague one. However, as Van Fraasen and others have pointed out, most of our predicates are vague, and providing that there are clear instances and counter-instances of a predicate it can be gainfully employed.[26]

It is also worth noting that the meaning-variance thesis, asserting as it does that the meanings of terms are theory-dependent, runs counter to the intuitive notion that terms have meaning within a language, not within a theory. Consequently, the thesis has highly paradoxical implications if it is taken seriously. Let us suppose, for example, that we have a theory, T, which yields a prediction, O, which turns out to be false. Well, it appears that if we push the meaning-variance thesis to its logical conclusion, then we cannot really say that the theory has been refuted, for the sentence O' or $\sim$O, which we would normally say contradicts the theory, is not commensurable with the theory or its derived consequence, O. $\sim$O is not a sentence belonging to the theory T; it is rather a sentence in the pretheoretic "observation" language. On the meaning-variance thesis it is meaningless

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[26]Bas C. Van Fraassen, The Scientific Image (Oxford: Oxford University Press, 1980), p. 16.

from the perspective of the theory. Hence, if one accepts the meaning-variance thesis completely, one is pushed ultimately to the position that a theory can never be refuted, not even on its own terms, short of revealing itself to be internally inconsistent. This does serious violence to the notions of meaning and of refutation and hence constitutes an effective reductio ad absurdum of the position. It may be objected that if O is a consequence of T, then $\sim$O must be in the language of T also. Granted, $\sim$O is in the language of T. But the point is this: we do not know, so long as we assume the meaning-variance thesis, that the terms occuring in $\sim$O have the same interpretation as they do in O. Again, this is because in the context of the weltanschauungen view we do not properly speak of meaning within a language but rather of meaning within a theory.

CHAPTER II

THE RELATIONS OF GLOBAL CONVENTIONALISM TO INSTRUMENTALISM, REALISM AND THE MODEL-THEORETIC APPROACH

To sharpen our characterization of global conventionalism it will first serve some purpose to consider the relation of the doctrine to other positions which address themselves to the question of the cognitive status of theories. Such an analysis will reveal significant differences between global conventionalism and these other accounts, differences which suggest that global conventionalism really does not belong in the same class as the various other perspectives with which it is generally compared and contrasted.

Apart from conventionalism, there are three principal candidates so far as the analysis of the cognitive status of theories is concerned. These are scientific realism, instrumentalism, and the model-theoretic approach. Global conventionalism we maintain, is essentially neutral with respect to these positions.

1. Conventionalism and Instrumentalism

Instrumentalism is the doctrine which regards theories as mere instruments of prediction or as "inference tickets", and which asserts that the meaning of a theory is exhausted by its observational or empirical consequences. According to this view the theoretical terms postulated by science are just useful fictions; their value is merely pragmatic or heuristic. On the instrumentalist account a theory is only a partially interpreted calculus. The model, at least the theoretical component of the interpretation, is regarded from a cognitive standpoint as virtually just so much ornamentation, as a cosmetic embellishment.

Instrumentalism no longer enjoys much favour, its demise following on the heels of a number of failures in the programme. For this reason, it is all the more important that we should not misconstrue the relationship between conventionalism and instrumentalism. Conventionalism is often closely associated with instrumentalism - in fact, the two are sometimes identified.[1] This is a serious confusion. Conventionalism and instrumentalism are logically quite distinct. To conflate the two is to obscure the real nature of the claims asserted by conventionalism, and to raise the danger that it may be rejected for the wrong reasons.

The essential claim of the significant variety of conventionalism is that there can exist alternative theories which handle the same data, but which are genuinely distinct. Conventionalism need not, logically speaking, carry a commitment to instrumentalism, although adherence to conventionalism may certainly dispose one towards instrumentalism. Adherence to conventionalism does not force acceptance of instru-

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[1]A perusal of philosophical dictionaries will bear this out. In P.A. Angeles' Dictionary of Philosophy (Harper & Row: New York, 1981) the entry under "conventionalism" urges us to "compare with instrumentalism". In A.R. Lacey's A Dictionary of Philosophy (Routledge & Kegan Paul Ltd.: London, 1976) we are told that "Conventionalism is close to instrumentalism..."

mentalism for the following reason. Suppose we are confronted with a pair of observationally equivalent but distinct theories. It is always possible that the ontology presented by one theory (i.e., the ontology conveyed by the theoretical portion) is the true ontology of the world, whereas the second theory is simply a false theory (insofar as it embodies a false ontology) which happens coincidentally to be an effective instrument for generating predictions concerning observable phenomena.

Looking in the other direction, neither does instrumentalism lead us inexorably down the path to conventionalism. One can embrace the instrumentalist interpretation of the status of the theoretical portion of science, one can accept that the role played by theoretical terms is purely methodological, without thereby committing oneself to the further view that there can exist distinct theories (<u>qua</u> instruments) that handle exactly the same data. In short, one can advocate an instrumentalist position that stops short of conventionalism.

While instrumentalism is distinct from conventionalism, the two are obviously quite compatible. Let us consider this for a moment, also. Conventionalism presents us with the following dilemma. We are confronted with two (or more) theories. These theories are observationally equivalent and descriptive of the real world insofar as they agree in their observational consequences, but they differ in regard to statements they contain which have no observable referents. These theories are underdetermined: that is, there are no directly accessible facts in the world which could be appealed to in order to settle the theoretical dispute they pose.

Now there are two general interpretations of this dilemma which suggest themselves and which we may call, respectively, the ontological or "no facts" version, and the epistemological account. One alternative is to say that conventionalism rests on the circumstance that there are no factual referents of certain terms in two "conventional" theories. The second alternative is to assert that certain crucial facts, facts which would render a non-conventional decision between the theories possible, are unknowable. The former may be called "ontological" conventionalism, the latter "epistemological" conventionalism.

Admittedly, there is a close affinity between the ontological variant and instrumentalism. Accepting the ontological version of conventionalism, one has two apparent choices. If ontological conventionalism rests on the claim that there are no factual referents for certain terms in two conventional theories, then we may decide to say that both theories are, strictly speaking, false - for they ostensibly ascribe properties to reality which it does not actually possess. Or, we may elect to treat the theories as only partially interpreted calculi (i.e., we may assume the instrumentalist stance), thus evading the former conclusion.

Of these two options, the latter may well appear more attractive. And seen in this light, conventionalism may seem to foist instrumentalism upon us. However the relationship between the two is still clearly not that of identity, for one can maintain an instrumentalism which stops short of even the "no facts" interpretation of conventionalism.

2. Conventionalism and Scientific Realism

Furthermore, in its epistemological guise global conventionalism is quite compatible with a form of realism, even with something which approximates scientific or theoretical-entity realism. It is necessary to draw a distinction between two forms of realism and also between two forms of scepticism, if this point is to be made.

The first crucial distinction is between what one may term "metaphysical" realism, and scientific realism. The latter is predicated implicitly on the former, but contains further additional claims. Metaphysical realism, roughly speaking, is the claim that our world has a determinate ontology, an ontology which exists independently of our attempts at theorizing. Metaphysical realism makes no claims concerning the efficacy of science in fathoming this ontology. In fact, it makes no claims concerning the aim and the cognitive status of theories whatsoever.

Scientific realism goes a bit farther. Expressed succinctly, it involves the following claims:

(1) The world has a determinate ontology. (Metaphysical realism is presupposed.)

(2) The methodology by which this ontology is to be discerned and rendered explicit is science.

(3) The theoretical language of science is to be taken literally. Theories are fully interpreted systems, although generally only a substructure of a given theory will be directly provided with an empirical interpretation.

(4) The theoretical language carries ontological commitments. Or, at least, where a theory is successful this is grounds for affirming confidence in the ontology reported by the theory.

As Sellars puts it: ". . . to have good reason for holding a theory is _ipso facto_ to have good reason for holding that the entities postulated by the theory exist."[2] (Of course, this commitment is generally regarded as tentative and provisional in character. Our theories are at best only approximately true, and we have good reason to expect that our present theories will be significantly transformed or surpassed. The critical realist does not presume that our present efforts constitute the last word in matters ontological. Scientific realism can of course be couched in more naive terms. But there are serious difficulties which attend these less sophisticated versions, and scientific realism in its most acceptable and most widely subscribed to form is critical in nature. The critical variety of scientific realism regards each successive advance in theory as conveying a closer or more complete approximation of reality; however, it does not regard any particular stage of theoretical development as final or irrevocable.)

Scientific realism is clearly at odds with ontological conventionalism at the global level, for the latter contradicts the metaphysical realism which is implicit in scientific realism. However, it is very nearly compatible with the epistemological form of which we spoke. As remarked earlier, confronted with the two observationally

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[2] W. Sellars, _Science, Perception and Reality_ (Humanities Press: New York, 1962), p. 97.

equivalent theories which are significant alternatives, one is always free to claim that one of these theories may portray the real ontology of the world. The scepticism one would be forced to by this circumstance is not necessarily an ontological one, hence one may resist instrumentalism. What one is forced to is rather an epistemological scepticism. One of the observationally equivalent theories may be assumed to render an approximation of "reality". But one cannot be sure which one, for the theories are underdetermined.

The inference invoked by scientific realism from the success of a theory to ontological commitment, of course, does not survive conventionalism in its usual form. However, an analogous but weaker claim may take its place. One might still agree that science affords us the only insight into ontology that we may secure, but allow that this insight only amounts to the elaboration of a number of alternative "pictures" of reality which are observationally equivalent but distinct. Our failure to further narrow the choice might plausibly be attributed to our limitations as sentient beings - to the capacity for apprehending and discriminating reality inherent in our conceptual and perceptual apparatus and their possible extensions.

Now this idea that global conventionalism may be compatible with realism (that is, with metaphysical realism, and with something which approaches scientific realism as we are familiar with it, but which is somewhat weaker) raises a number of points. First, it presumes that a false theory can be as efficacious as a true one, and it assumes this against the backdrop of some form of realism. It is allowed that the ontology of one alternative theory is likely to be the actual or the true ontology of the world, whereas one of the principal tenets of scientific realism has been the inference mentioned above from explanation or theory success. The success of a theory, on the realist view, is generally taken to be sufficient grounds for a commitment to the ontology presented by the theory, that is, to the ontology presented by a particular theory.

But this objection is nothing more than an affirmation that conventionalism is anathema to the died-in-the-wool orthodox realist precisely because he is anti-conventionalist in his sympathies, and this is simply not germane to the discussion at this point. Conventionalism mocks this sort of realist not because it is fundamentally or necessarily anti-realist, but rather because the realist is anti-conventionalist. The claim which has been made is that conventionalism is compatible with something which closely approaches traditional expressions of scientific realism. Metaphysical realism, an essential ingredient or underpinning of scientific realism may be preserved, as may all the other essential elements - excepting that the inferential basis or argument for realism, must be weakened. The commitment involved in a realist position formulated within the constraints imposed by a pro-conventionalist attitude must be a commitment to a class of ontologies - those incorporated in a class of distinct but observationally indistinguishable theories - rather than to a uniquely determined ontology.

The accompanying idea that a false theory may be as efficacious as a true one is perhaps a bit distasteful. However, there is really no insuperable difficulty with the notion that a false theory may be successful. What is difficult to accept, perhaps, is that a false theory could enjoy precisely the same degree of success as a true one. And this is perhaps really the same difficulty that we encounter in trying to accept the notion that any two, distinct, consistent theories, leaving aside the matter of their veracity, could be so precisely matched. For the moment, this is just to say that conventionalism may well strike many of us as counter-intuitive.

The following objection also springs to mind. If our theories are really underdetermined, how could we ever know which (if any) ontology was correct? Presumably we couldn't - which circumstance, it might be urged, calls the whole notion of there being an ontology into question. This objection, if it goes through, leads us away from metaphysical realism and takes us back to the no-facts version of conventionalism and thence to instrumentalism.

There are a number of possible responses to this line which come to mind. The right one, we think, is to assert that the ontological question does survive. We see no obvious reason to abandon the notion that the world has an ontology on the hypothesis that we cannot ever identify that ontology precisely. If the best that science can do is to provide a set of theories, one of which may reflect the true ontology of the world, but which are underdetermined by the available facts, then we will simply have to settle for that situation. Why say that the failure of science (and, remark, it is only the possibility of a limited failure and its implications which we are discussing here) must strip the world of its autonomy? Surely it would be bloody-minded and unconscionably anthropocentric to deny that the world has an independent ontology merely on the basis that we cannot know it. Even if nature should prove, ultimately, to be so inscrutable, it does not follow that she has nothing to hide. Epistemic humility may be preferable to some form of subjective idealism.

There is another reply to this challenge directed to the intelligibility of the ontological question which occurs. Someone might wish to argue that the condition of underdetermination is only a temporary one, and that the theory embodying the correct ontology will inevitably show its true colours and establish itself over its competitors by admitting of further expansion and development, where its competitors might not (owing to the fact they rest on a faulty ontology). But this strategy will not work. If underdetermination is just a temporary condition of an evolving theory, then clearly theories cannot be said to be underdetermined in any real sense, at least certainly not in the interesting sense advocated by conventionalism. One of the conditions which we should presumably impose on two theories, call them T_1 and T_2, which aspire to the status of conventional alternatives is the following. For any successful extension of T_1, call this T_1', there must exist an extension of T_2, call it T_2', which brings T_2 up to observational equivalence with T_1'. And conversely.

This is obviously a very strong condition given that T_1 and T_2, as well as any extensions they may spawn which are also observationally equivalent, must be distinct. We would expect the condition to be satisfiable in the case where the theories T_1 and T_2 are isomorphic, for then they have exactly the same structure and hence exactly the same resources for modelling the phenomena to begin with. However, we shall argue later that if two theories are isomorphic then they are not distinct in the sense required by conventionalism.

There are no model-theoretic results which support the contention that theories must exist which satisfy all of these conditions. In other words - and this is a very important point - conventionalism is not supported on any general logical grounds. (The Lowenheim-Skolem theorem may come to mind, but I shall have something to say on that subject later.) So already we see that conventionalism involves a commitment to some very strong claims which, in the absence of any pair of theories which exemplify the doctrine, must be accepted, seemingly, as an article of faith. Conventionalists, one begins to suspect, are rather Kierkegaardian under the skin.

Of course, the conventionalist can always try to get his way by resorting to something which amounts to a devious trick. Suppose we have two theories T_1 and T_2 which are observationally indistinguishable. Let us imagine that T_1 is provided with an axiomatic extension to yield a broader, successful theory T_1'. In the event that no corresponding extension of T_2 should be forthcoming, the conventionalist might be tempted to safeguard his position by the following strategy. Let us call the set of theorems in the observational language derivable from each of T_1 and T_2, Γ. And let us call the additional theorems available from the extended theory T_1' (the theorems of T_1', excluding the set Γ), Γ^*. Now T_2 can always be brought up to observational equivalence with T_1' by the expediency of adding to it the entire set Γ^* as axioms. Call the resulting theory T_2^*.

But this invidious device hardly serves to establish conventionalism as an interesting proposition. It rescues conventionalism at the cost of trivializing it. Furthermore, in so doing it even renders it irrelevant to science, for there are sound reasons for withholding from T_2^* the ascription of the term (empirical) "theory". One of the necessary conditions which can be invoked in delimiting science is the requirement that all observation theorems be deducible from a minimal set of axioms that must include some sentence which is couched in the theoretical vocabulary of the theory concerned. Thus, for example, one is prohibited from taking a bunch of true observation sentences, affixing a number of unrelated "axioms" expressed in a different vocabulary, and calling the resulting miscellany "science". This, in essence, is the same type of manoeuver as that which generated T_2^*. In fact, it is simply the same maneouver in reverse.

3. The Neutrality of Conventionalism

So far, we have discussed the compatibility between conventionalism on the one hand and both instrumentalism and realism on the other. Instrumentalism, it was suggested, is entirely consistent with the no-facts version of conventionalism, whereas some of the basic precepts of realism may be retained under the epistemological rendering of the doctrine. In fact, it seems more fitting to view conventionalism as neutral with respect to the points of view asserted by instrumentalism and realism; that is, to regard conventionalism solely as the claim that alternative theories may exist which are observationally equivalent but distinct. Conventionalism, construed in this way, does not really speak to the matter of the cognitive status of theoretical entities, except insofar as it maintains that they are underdetermined by the empirical findings. The no-facts version of conventionalism may be understood as neutral conventionalism plus instrumentalism, and the epistemological or verificationist characterization as conventionalism coloured by realism. After all, the arguments, such as they are, that will decide for us the discrepancy between the no-facts version and the epistemological account are just the same arguments that will decide the issue as between instrumentalism and realism. And should the arguments there prove inconclusive - I don't maintain they actually do - then the two versions of conventionalism would have to be regarded as being, one might say, philosophically underdetermined.

There is a decided advantage to this approach, in that it permits us to broach the question of the viability of conventionalism without becoming embroiled in a dispute between instrumentalism, scientific realism and, further, the third party which has not yet been introduced, namely, the model-theoretic approach. There is no point in becoming entangled in a dispute over the relative merits of the no-facts and epistemological versions of conventionalism if we can possibly convince outselves that theories are simply not underdetermined, regardless of whatever else one may want to say about their import.

4. Conventionalism and the Model-Theoretic Approach

To this point, we have concentrated on the connection between realism and instrumentalism on the one hand, and conventionalism on the other. Again, in so doing we have excluded any mention of the third candidate for the analysis of the cognitive status of theories, namely, the model-theoretic approach (MTA).

However, before proceeding, there are a few things which must be noted. It must be acknowledged that MTA is a convenient label that encompasses a number of specific approaches which vary in their details, but which share a common assumption. The common premise which binds these approaches together is the conviction that the analysis of scientific theories requires a semantic approach. In some cases the semantic techniques which have been invoked are in fact precisely those of standard model-theory. Von Neumann's proof of the equivalence of the wave mechanics and matrix mechanics formulations of quantum theory is a notable example which falls within this category.[3] However, more recently, semantic approaches have tended to involve the development and application of various semantic tools specifically designed to meet the unique requirements of physical theories. Two main representational tendencies have also emerged within MTA. These are the set-theoretic structure approach developed primarily by P. Suppes and later elaborated by J. Sneed and others, and the state-space approach which originated with H. Weyl and which has been further developed by Evert

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[3]J. von Neumann, Mathematical Foundations of Quantum Mechanics (Princeton, N.J.: Princeton University Press, 1955.)

Beth.[4] It is not possible to do justice to the details and subtleties of these variations within MTA in the present context. However, neither is it truly necessary to address these variations in order to fulfill our present purpose. The representational differences which have been referred to are most significant with respect to the question of the logical structure of theories. However, here we are primarily concerned with the relation between our theories and the world, and the relations which may obtain between theories.

In establishing the claim that MTA is compatible with conventionalism, it will suffice to consider briefly the formulation presented by van Fraassen. This particular choice is prompted by two considerations. First, van Fraassen's version of MTA is perhaps the broadest in conception. He has developed a semantics for the analysis and evaluation of physical theories in general, and this semantics is compatible with either of the two major representational trends within MTA.

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[4]See the following: (1) E. Beth, "Semantics of Physical Theories", in H. Freudenthel (ed.), The Concept and the Role of the Model in Mathematics and Natural And Social Sciences, (Dordrecht, Holland: Reidel, 1961.) (2) J. Sneed, The Logical Structure of Mathematical Physics (Dordrecht, Holland: Reidel, 1971.) (3) P. Suppes, "What is a Scientific Theory?", in S. Moregenbesser, P. Suppes & M. White, eds., Philosophy of Science Today (New York: Basic Books, 1967.)

The semantics developed by Beth, in contrast, are designed to accomodate specific theories. Secondly, van Fraassen's approach is actually an alternative to both instrumentalism and realism; in a sense, it falls between the other two approaches. This is not true of all "semantic" approaches. For example, the programme for developing a semantics of science advocated by M. Bunge assumes the position of critical realism.[5]

The general idea in MTA is that to present a theory is to present the class of structures which are its models. Van Fraassen summarizes the approach as follows:

> To present a theory is to specify a family of structures, its models and secondly, to specify certain parts of these models (the empirical substructures) as candidates for the direct representation of observable phenomena. The structures which can be described in experimental and measurement reports we can call appearances: the theory is empirically adequate if it has some model such that all appearances are isomorphic to empirical structures of that model.[6]

According to MTA, we are to take the theory (or its statement, rather) literally. That is, we are instructed to construe the theoretical claims of the theory literally. So in this respect MTA is like scientific realism and unlike instrumentalism. The reductionism of

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[5]M. Bunge, "A Program for the Semantics of Science," Journal of Philosophical Logic 1 (1972), pp. 317-328. Also vol. 2 of his Treatise on Basic Philosophy, entitled Semantics II: Interpretation and Truth (Dordrecht, Holland: Reidel, 1974).

[6]B.C. van Fraassen, op. cit., p. 64.

instrumentalism is rejected explicitly. But van Fraassen's version of MTA stops short of scientific realism, for it asserts that we are not permitted to the truth of the theories which we accept. Like instrumentalism, this variant of MTA involves no realistic commitment to an ontology postulated by a theory.

MTA in this guise may be characterized, perhaps, as a "disinterested" realism. Theories on this account are to be taken literally, but what is considered important is not whether a theory is true, but whether it is "empirically adequate". This disposition does not preclude the possibility that a theory may be true; rather, it makes it a non-issue. Van Fraassen's MTA is deliberately non-commital in this respect: the agnosticism it conveys is intended to "deliver us from metaphysics".[7] Van Fraassen himself prefers to label his position as "anti-realist", but this is simply a terminological difference which reflects the motivation behind his work: to provide a strong empiricist alternative to scientific realism which does not exhibit the defects that vitiate instrumentalism. Van Fraassen's position is just as aptly described as anti-instrumentalist, and its characterization as a "disinterested" realism appears in no way misleading.

This form of MTA is clearly compatible with global conventionalism since, in a sense, it is a further diluted form of the weakened scientific realism which has been shown to be compatible with that doctrine. And if instrumentalism does not induce conventionalism, then obviously neither does MTA.

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[7] Ibid., p. 69.

5. Formal Approaches to Conventionalism

It is time to change the direction of the discussion and to consider some of the possible implications of the last few pages. We have argued that conventionalism is neutral with respect to instrumentalism, scientific realism, and the "model-theoretic" approach. This is contrary to a discernible tendency in the literature to associate conventionalism with instrumentalism, or at least to regard the doctrine as being fundamentally at odds with realism.[8] And there is some sense to this association. Adherence to instrumentalism makes conventionalism more plausible, possibly, for if our theories are just linguistic devices that happen to furnish successful "prediction machines" then the question naturally arises: why should there be only one such device? But the relationship is no stronger than this, and at that it is somewhat tenuous.

This suggests the following possibility: conventionalism may be a semantic claim and only derivatively an epistemological one; that is, it may be a claim about the formal properties possessed by linguistic or symbolic entities, and hence a claim which might potentially turn out to be true or false in principle.

There has been some discussion of conventionalism which has appealed to model-theoretic results, especially the Lowenheim-Skolen

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[8]See, for example, Richard Boyd's article "Realism, Underdetermination, and a Causal Theory of Evidence," Nous, 7 (1972), 1-12.

theorem.[9] However, more typically the discussion over global conventionalism has proceeded in a different spirit. The tendency seems to have been to regard global conventionalism as consistent and only contingently true or false. This tendency derives largely, I think, from the perception that conventionalism is diametrically opposed to realism, a perception which is not entirely on the mark. Scientific realism surely constitutes a synthetic claim (or set of claims) and one would naturally assume that its contradictory is also a synthetic claim. However, according to the preceeding analysis, conventinalism and realism do not appear so obviously as contradictories. Traditional theoretical-entity realism certainly contradicts conventionalism but one may say it does so precisely because it is anti-conventionalist in addition to being "realist". Realism has been thoroughly informed by anti-conventionalist sentiments throughout its history, so much so that one does not immediately see that the basic tenets of realism might be stated apart from the anti-conventionalist claims with which they have invariably been allied. My point is that this long-suffering marriage appears to have an historical basis rather than one of logical necessity.

If it is correct to construe conventionalism in this way, then ultimately formalization is indispensible to assaying the doctrine.

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[9]See Hilary Putnam's "Models and Reality," Journal of Symbolic Logic vol. 45, no. 3 (September, 1980). Also W.V.O. Quine's "Ontological Relativity," in his Ontological Relativity and Other Essays (Columbia University Press, 1969) pp. 26-28.

If conventionalism is false in principle then one must suppose that there will be a model-theoretic result pending to the effect that theories which satisfy certain formally stated criteria (including appropriate criteria of distinctness) cannot yield exactly the same theorems in a certain restricted vocabulary. It is no use objecting that the concepts and results of classical model theory (in the sense of Tarski, 1936), are inadequate to the evaluation of scientific theories. It is quite true that these concepts were intended to handle the semantics of formal systems which differ in important respects from physical theories, and that they cannot be used to mark the semantic distinctions important for science. And admittedly some powerful nonsense has resulted from the injudicious application of these concepts beyond their proper domain. (The so-called semantic theory of truth is a prime example.) But what is being suggested here is the possibility that the problem of conventionalism is not one peculiar to science *per se*, that it might be simply a claim about the types of relations which may obtain between symbolic entities which satisfy certain requirements that can presumably be stated formally. In this case it is even possible that there is some validity in a purely syntactical approach to the problem. What is required for the project to get off the ground is that we grant there is a distinction between observational and theoretical terms and, further, that this is a distinction which can be marked syntactically. (What we cannot do, of course, is to defend the distinction as essential to science by invoking the fact that it can be made syntactically. The existence and significance of the distinction and where it is to be drawn are substantive philosophical questios which must be solved through philosophical reflection and argumentation.)

Having made these rather speculative comments, it must be acknowledged that there are no existing model-theoretic results which might obviously be adapted to this purpose, and there is no sense in holding our breath while we can perhaps make some headway with a more philosophical, informal, approach. Still, even if the problem were not a logical one, the concepts of model-theory might still be useful for the purposes of trying to state more clearly what we mean by two theories being distinct, a concept which is crucial to conventionalism. Until we have a more precise statement of what we mean by "distinctness" in this context, the problem cannot even be said to be well-defined.

CHAPTER III

THE MEANING OF CONVENTIONALISM: A MORE PRECISE STATEMENT

1. Criteria of Theory Synonymy

In this present chapter we would like to undertake to discuss various possibilities for advancing a more precise statement of global conventionalism (henceforth, GC). The key will be to provide a more exact criterion for the assertion that two observationally equivalent theories are distinct. To this point we have relied on the intuitive notion that two theories are distinct providing that they are not merely trivial semantic alternatives. This is not very edifying, however, unless a more exact account of TSC can be furnished. We shall argue that two theories are trivial alternatives if and only if they are isomorphic (in a special context). To this end we shall first try to discount both weaker and stronger requirements for theory synonymy.

To begin with we shall consider briefly two other requirements for theory synonymy which might appear to carry a certain amount of initial plausibility. The first is based on the notion of a match. The idea behind this criterion is that two theories are distinct providing they have different domains. The second possibility we shall explore is the notion that two theories are synonymous (are semantic alternatives) in the event they share a model.

Let us first consider a more precise formulation of conventionalism that invokes as a condition the account of distinctness which is couched in terms of the concept of a <u>match</u>. (An interpretation $\mathcal{I}$ "matches" a set Γ of quantifier-free sentences iff: (a) $\mathcal{I}$ is a model of Γ, and (b) if any terms (names or function symbols) occur in Γ, then each object in the domain of $\mathcal{I}$ is the denotation of some such term. A match is thus an interpretation in which every object in the domain is referred to - but not necessarily named.) We have the following:

(i) T_1 and T_2 are two theories.

(ii) L is the observation language.

(iii) $\mathcal{I}_L$ is the standard interpretation of L.

(iv) T_1 and T_2 have exactly the same observational consequences; that is, for any sentence S, if $\mathcal{I}_L$ is an interpretation of S then $(S \in T_1 \leftrightarrow S \in T_2)$.

(v) The language L_1 and L_2, (of T_1 and T_2, respectively) each includes L.

(vi) Terms occur in the sentences of T_1 and T_2 which are not interpreted by $\mathcal{I}_L$ (i.e., there are theoretical terms).

(vii) There is no set of objects D, such that some interpretation $\mathcal{I}_1$ with domain D matches T_1 and some interpretation $\mathcal{I}_2$ with domain D matches T_2.

This list is not intended to exclude any further condition on T_1 and T_2 which might reasonably apply. In particular, it is not to be construed as pre-empting any conditions they might have to meet in addition to those specified in order to qualify themselves legitimately as examples of "science". We shall assume that there are no objections to conditions (i) through (vi), such as they are. Our intention here is to focus on (vii).

Condition (vii), on reflection, is unacceptable. It constitutes too strong a requirement in two different respects. First of all, it necessitates that T_1 and T_2 exhibit the same number of terms. This is somewhat in keeping with the spirit of an idea advanced at one time by both Hilary Putnam and Clark Glymour to the effect that theories cannot pose as conventional alternatives unless they are equivalent in such matters as descriptive simplicity. However, this idea strikes us as completely misguided. Surely all we require for conventionalism is that two theories be observationally equivalent, but distinct. This, of course, leaves open the possibility that there might be obvious pragmatic advantages attached to one particular alternative. But such advantages, we maintain, can never translate into epistemic superiority; they cannot be called upon to push aside the problem of underdetermination, should it genuinely arise.[1] If this is sound, then there is no possible

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[1] In his "The Refutation of Conventionalism", Nous, 8 (1974), Putnam argues that conventionalism is to be rejected as a covert expression of the generally discredited philosophical doctrine of essentialism, and that the reference of our theories - he deals specifically with geometrical theories - can be fixed uniquely by an appeal to such considerations as simplicity and agreement with our pre-theoretic intuitions, considerations which he lumps under the rubric of "coherence". In a similar vein, Glymour ("The Epistemology of Geometry", Nous, 11 (1977.) rejects the notion that geometric theories concering the world are underdetermined on the ground that alternative theories which all "save the phenomena" are not all equally well tested by the phenomena. This approach seems wrong-minded. Surely all we require for the underdetermination thesis is that we be confronted with two theories which handle the same data, but which are genuinely distinct. Methodological principles, such as a preference for simpler theories, cannot be invoked to establish a non-conventional choice between two theories which instantiate GC.

warrant for insisting that theories which exemplify GC contain an identical number of terms. Yet this condition is implicit in (vii) as it stands.

In addition, the requirement that two theories which instantiate the doctrine must involve different domains is itself apparently too strong, also. Presumably we would not want to call two theories which exhibited the same domain, but which described that domain differently (suppose, for example, that they contained different relations on that domain), trivial alternatives. But on requirement (vii) as stated, two such theories would fail to qualify as significant conventional alternatives. Of course, it may be that no two physical theories with the same observational consequences and the same domain (which also satisfied all the requirements of science per se) would likely be distinct in this other sense. But we do not appear to have any absolute guarantee of this, and so there is no real justification for entering the thesis as a premise in our discussion - however intuitive it may seem.

To provide an epistemic basis for some such methodological principle, we must employ an inductive argument of some form. We must be able to argue that since simpler theories have a better track record such theories are more likely to be successful in the future. But the GC scenario explicitly precludes any such argument. Recall that a requirement of a pair of theories exemplifying GC is that for any successful extension of one, there must exist an equivalent extension of the other. Another way of expressing this point is to say that GC also applies to an entirely complete or "total" science. If GC is sound, then it follows no inductive argument is available for establishing an epistemic justification which will underwrite the non-conventional selection of some theory over its alternatives on any methodological grounds such as simplicity.

Now the first difficulty associated with (vii) might be circumvented by withdrawing the employment of the concept of a match. Suppose we retain the form of (vii) as it stands, but substitute "model" for "match". We then obtain the following weakened requirement, (vii)': "There is no domain D such that for some interpretations $\mathcal{J}_1$, $\mathcal{J}_2$, with domain D, $\mathcal{J}_1$ is a model of T_1 and $\mathcal{J}_2$ is a model of T_2."

This gets around the first objection outlined above to the original formulation (vii), namely that it unjustifiably required significantly conventional, alternative, theories to exhibit the same number of terms. But it does not meet the second objection that was raised. Moreover, it generates its own peculiar problems. For instance, we can generate the required domain D trivially in every case: just let D = $\{D(\mathcal{J}_1) \vee D(\mathcal{J}_2)\}$.

There is no apparent way to salvage the approach followed in (vii) and (vii)', an approach which focuses exclusively on the domains of theories which might be nominated as significant conventional alternatives.

Let us proceed then to consider another possible approach. Glymour has suggested in passing that two theories might be considered synonymous on the condition that they share a model.[2] Correspondingly, we would say that two theories are distinct only if they do not share any model. Intuitively, this criterion doesn't seem satisfactory either. It appears to be too weak. If two theories satisfy only the

[2] C. Glymour, "The Epistemology of Geometry", Nous, 11 (1977), p. 230. It must be emphasized that this is merely an alternative identified by Glymour. He does not defend the criterion in this particular article.

requirement that they share a model, then they can certainly be used to say the same things. But unless the theories concerned are both "categorical"[3] - and it is far from certain that our empirical theories would be, since any consistent categorical theory must have a finite domain - there is no assurance that they can necessarily be employed to say all the same things. Consider the case where a theory T_1 is embedded in another theory T_2. T_1 and T_2 must then share a model. But one would hardly want to say that they must be synonymous, for T_2 is richer in structure and consequently may be interpreted so as to convey information that simply cannot be modelled by T_1. On this basis it seems that we must also reject the notion of "sharing a model" as a sufficient criterion for theory synonymy.

Now we can easily anticipate a possible objection to this dismissal. It would run something like this: "If two theories share a model, but satisfy no stronger criterion such as isomorphism (say T_1 is embedded in T_2 and they share a model M) and they are empirically equivalent, then we may expect that T_2 contains some additional structure which is not being put to work, as it were. This extra structure may be described as so much excess "metaphysical baggage", and it should not be possible for such excess baggage alone to differentiate between two theories. But if we do reject the criterion of synonymy that invokes the idea of simply sharing a model and insist instead on some

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[3]G. Boolos and R. Jeffrey, <u>Computability and Logic</u>, (Cambridge: Cambridge University Press, 1980). "T is a <u>categorical</u> theory if any two models of T (that are interpretations of T's language) are isomorphic." p. 191.

stronger requirement - perhaps that they share all their models - then this is precisely what may happen. In fact, we could produce pairs of theories which we would have to acknowledge as legitimate instances of GC by the following trivial method. We simply take an existing theory T and append certain axioms to it, being careful to ensure that they cannot facilitate the derivation of any new observation theorems - so that they cannot in any way affect the empirical commitments of T. This is easy enough to accomplish: one simply uses a disjoint vocabulary for the appended axioms. The original theory T and the amended version with the extraneous baggage, call the latter T^*, might then assert a legitimate claim to status as significant conventional alternatives, for we could easily construe T^* in such a way as to ensure that it would have a model which was not also a model of T."

There is certainly something to this objection. However, we do not believe that the proper response is to allow the notion of sharing a model to serve as an account of theory synonymy. Rather, the appropriate way to deal with the problem addressed in this objection is through strengthening the requirements that a theory must meet in order to qualify as genuinely scientific discourse. The rebuttal sketched above invokes a strategy we have already encountered in a slightly different context, and the appropriate response is the same now as it was on that occasion: we impose the requirement that our theories be suitably austere, that they carry no such purely metaphysical baggage. To this end we may insist that theories betray no theoretical principles which do not figure indispensibly in the derivation of some theorem in the observation language. This may be expressed, equivalently, as the requirement that our theories be cast in a form that is "empirically minimal". The notion of empirical minimality owed to van Fraassen, and he defines it thus:

> We may call a theory empirically minimal if it is empirically equivalent to all logically stronger theories - that is, exactly if we cannot keep its empirical strength the same while discarding some of its models.[4]

There might well be some objection to this reply. Van Fraassen himself, after pointing out that "sophisticated" theories invariably reveal some extra baggage, argues that empirical minimality is not to be praised as a virtue. He says that empirically minimal theories, so far from being required, are less preferable than their more complex counterparts. He justifies his attitude essentially by contending that this extra baggage which is involved has "potentialities for future use."[5]

But this resistance can be met quite effectively. There are two points to be considered in this regard. (1) Our requirement that theories be stated in an empirically minimal form is not necessarily to be interpreted as a prescription to the scientist to eschew any theory which contains extra baggage. From the standpoint of the pragmatics of theory development, such theories may offer certain advantages. However, for the purposes of conceptual analysis or philosophical

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[4]van Fraassen, p. 68.

[5]*ibid.*, p. 69.

appraisal, we may still insist that theories be cast in empirically minimal form. The point is not that such extra metaphysical baggage is necessarily useless, but rather that it may be methodologically misleading. Our tendency to construct theories with optional content may be more significant from the point of view of cognitive psychology and pragmatics than from a methodological perspective. (2) Furthermore, the rejection of extra-theoretical baggage, at least so far as the matter of the epistemological analysis of our theories is concerned, may be defended by the following considerations. Van Fraassen grounds a preference for more complex theories in the potential for future use that may be inherent in the additional structure which they possess. The question arises, however, why we should favour these more complex theories when we have no assurance before the fact that the extra baggage they are carrying is precisely the sort of extra baggage which will invariably prove valuable. It appears, therefore, that there is a strong case to be made for the requirement of austerity. This is certainly so with respect to the trivial case we posed earlier. The type of metaphysical baggage that T^* carries can always be introduced as it is required.

In any event, it seems preferable to exclude such pairs of theories as T and T^* from consideration as significant conventional alternatives by this strategy just adumbrated, rather than by choosing the alternative solution presented by the "sharing a model" criterion of synonymy. The extra baggage packed into T^* may well turn out to be entirely superfluous; we may never find a purpose for it. But if one is intent on declaring T and T^* synonymous then one must be prepared to label the extra baggage belonging to T^* as meaningless. This course appears to set us on a route towards some form of a verificationist account of meaning, and this is currently not a particularly attractive position. Irrelevance is not to be confused with meaninglessness.

We require, then, a stronger criterion for theory synonymy than that afforded by the condition of sharing a model. Such a criterion is now suggested, rather naturally, by the condition that synonymous theories share all their models; that is, for any synonymous theories T_1 and T_2, every model of T_1 will be a model of T_2, and conversely. This condition will be satisfied just in case the two theories are isomorphic. We can then specify a routine or mechanical procedure for converting T_1 and T_2, and vice versa. The procedure consists in the specification of a "dictionary": a set of sentences, S, of definitional form, which will enable us to perform the translation procedure. The procedure works as follows. Given two theories T_1 and T_2, the set of sentences S would be appended to T_1 to form a new theory T_2'. T_2' would be an extension of T_1. From T_2' we could generate as a set of theorems all the sentences of T_2. We could then drop all sentences of T_2' containing non-logical symbols of L_1 (the theoretical vocabulary of T_1). The remaining theorems would be the sentences of T_2. An analogous procedure would of course transcribe T_2 into T_1.

We can now say that a theory T_1 is distinct from another theory T_2 if either (or both) of two conditions obtain. (a) On translation T_1 is found to contain sentences which are incompatible with sentences occurring in T_2, or (b) on translation one of the two theories is found to contain sentences which are not expressed in the other. Note that this requirement of intertranslatability for theory synonymy does not commit us to the view that a theory may be strictly synonymous with one of its own extensions.

2. Intertranslatability and Theory Synonymy: A Defence

The requirement of isomorphism or intertranslatability as explained above certainly seems strong enough. In effect it says that two theories are synonymous if and only if they are trivial semantic alternatives. It is difficult to imagine any further condition that might be required for theory synonymy. If two theories which are completely intertranslatable are yet not to be construed as synonymous, then it is difficult to conceive what precisely would count as a sufficient condition for theory synonymy. Pushed that hard, the very notion of theory synonymy seems to become opaque; it takes on a highly mystical aspect.

Michael Friedman, however, has recently criticized at length the notion that intertranslatability constitutes a sufficient condition for theory synonymy. In his book, Foundations of Space-Time Theories: Relativistic Physics and Philosophy of Science, Friedman argues that intertranslatability represents at best a necessary condition for theory synonymy.[6] He maintains that two theories which are empirically equi-

[6] Michael Friedman, Foundations of Space-Time Theories: Relativistic Physics and Philosophy of Science (Princeton: Princeton University Press, 1983).

valent and strictly intertranslatable may still fail to prove fully equivalent. He asserts that such theories may fall short of full equivalence or synonymy because they may still embody different "ideological" commitments of a theoretical nature. Friedman (who incidentally is not a pro-conventionalist) builds his argument around a counter-example. He claims to have actually constructed theories which are empirically equivalent and fully intertranslatable but nonetheless quite distinct. We shall argue that Friedman's putative counter-example is beside the point. The family of theories he appeals to violates the condition of austerity or empirical minimality. Each theory in this family is, strictly speaking, an instance of the T^*-type theory previously described. The portion of each theory which serves to distinguish it from its competitors is precisely that portion which may be described as superfluous metaphysics. Friedman's example thus fails to demonstrate that intertranslatability is not a sufficient condition for theory synonymy, where the theories concerned are empirically equivalent and suitably austere. We shall proceed as follows. First, we shall outline the theories which Friedman invokes. Afterwards we shall comment on Friedman's assessment of their relationship to one another.

In chapter three of this book, Friedman sets out to construct a spacetime formulation of Newtonian Kinematics, a theory which incorporates the notion of absolute space. Within such a theory the notion of absolute rest and absolute velocity are well-defined. (Note that "well-defined" does not entail "empirically significant".) After he has completed this initial task he then proceeds to show how a "Galilean" system of kinematics may be represented. A Galilean kinematics is one which features only the assignment of relative velocities. In a Galilean theory there is no privileged inertial frame of reference; that is, no frame at rest with respect to an absolute space. All inertial frames are regarded as being on an equal footing. Let us proceed to examine how these theories are formulated.

First, the Newtonian version: A number of geometrical objects are specified. The basic object is the spacetime manifold, M. There are several geometric structures which must be defined on M in order to produce a kinematics. In Friedman's case, the geometrical structures are associated with the trajectories and events that occur within spacetime by means of numerous field equations and various laws of motion. Friedman thus develops his theories by imposing local field conditions on the topology of M.

The first condition which must be satisfied by a Newtonian kinematics is that spacetime must be flat. So we must posit a flat affine connection D defined on M. Newtonian spacetime is also characterized by an absolute time: for any arbitrary point p, in M, we have the concept of all points which are simultaneous with p. Since in a Newtonian spacetime the temporal dimension is absolute, this notion does not depend on the specification of any particular frame of reference.

The notion of "simultaneity" is clearly an equivalence relation: it partitions Newtonian spacetime into successive "simultaneity sets" or "planes of absolute simultaneity".[7] Friedman has thus stratified

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[7]Friedman, p. 72.

the four-dimensional spacetime into a series of three dimensional absolute spaces which define the notion of simultaneity. Temporal duration is simply the distance between any two such planes, and the corresponding geometrical object is a co-vector field dt. The planes themselves are Euclidean three-spaces and their metrical geometry is defined by a symmetric tensor field h.

What is now required is some structure that will tie the instantaneous three-spaces together, to bind them into one single "enduring" space. The necessary relation of spatial coincidence is established by introducing a new geometrical object, which Friedman describes as a "rigging of space-time". This is just a "family of non-intersecting geodesics that penetrates each plane of simultaneity."[8] It is important to note that the choice of a rigging is essentially an arbitrary one. Any member of an infinite class of families of non-intersecting geodesics will suffice for this purpose. In Friedman's treatment this rigging is introduced by means of a geometrical object V, a tangent vector field. As he notes, our choice of a rigging can be viewed as the arbitrary selection of one particular inertial system over all others as a system at rest with respect to absolute space.

To summarize, Friedman's Newtonian spacetime kinematics is a theory constructed from a set of geometrical objects defined on the spacetime manifold M: an affine connection D (which imposes the condition of flatness on M); a co-vector field dt (which represents abso-

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[8]*ibid.*, p. 74.

[9]*ibid.*, p. 77.

lute time); a symmetric tensor field h, of type (2,0) (which describes the Euclidean metrical geometry of the planes of absolute simultaneity); and a contra-vector field V (the "rigging" which defines absolute space.) The complete theory, of course, includes local conditions imposed on these objects by means of a set of field equations and the specification of the laws of motion for objects within spacetime. For the sake of brevity, I have not presented these as they are not essential to our present discussion. The important point at the moment is that an infinite class of Newtonian kinematic theories can be constructed. One simply associates absolute space with a different inertial frame in each case. We may designate this class of theories: $\{T_V, T_{V'}, T_{V''}, \ldots\}$.

From here Friedman moves on to construct a formulation of Galilean kinematics, a kinematics which features a spacetime in which there is no privileged inertial frame to which all motions are to be related. In a nutshell: "To formulate Newtonian kinematics in a way that dispenses with absolute space and treats all inertial systems on a par we simply drop the rigging - the vector field V - while retaining the flat affine connection D and the notion of inertial frame."[10]

The new Galilean kinematics thus postulates the same geometrical objects as the Newtonian theory, with the notable omission of the vector field V. We have an affine connection D, a co-vector field dt, and a symmetric tensor field h. The same local conditions, provided in the form of local field equations, are imposed on these objects.

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[10] ibid., p. 87.

Now, what does Friedman have to say about the status of the class of alternative Newtonian spacetime theories, $\{T_V, T_{V'}, T_{V''}, \ldots\}$? He says that they are empirically equivalent, but incompatible. They are incompatible, he says, "because they generate mutually inconsistent assignments of absolute velocity to any given object."[11] At the same time, they are obviously also intertranslatable: "Simply translate 'absolute velocity' in one theory by 'velocity relative to inertial frame F_V' (i.e. the priveleged frame) in the other."[12] Thus, on Friedman's view the class of empirically indistinguishable Newtonian spacetime theories constitutes a counter-example to the claim that intertranslatability represents a sufficient condition for theory synonymy. These theories, he maintains, are not fully equivalent for they betray different ideological commitments. "They are not fully equivalent until after we revise the ideology of our theories."[13]

In response it is tempting to counter that what we are confronted with is clearly a distinction without a difference, and that any perception that these various Newtonian theories involve different ideological or theoretical commitments is simply an illusion - moreover an illusion which is exposed by their strict intertranslatability and empirical equivalence. Such a reply is not entirely satisfactory, however. It smacks too strongly of verificationism.

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[11] ibid., p. 276.

[12] ibid., p. 282.

[13] ibid., p. 280.

But there is an alternative response which allows us to deal with this alleged counter-example, and with which we may feel comfortable. Here is our reply: It is quite clear from the preceding exposition of Friedman's Newtonian and Galilean kinematics that each variety of the Newtonian version is simply a different extension of the Galilean theory. To arrive at a Newtonian kinematics one in effect simply tacks a further geometrical object (any one of the contra-vector fields $\{V, V', V'', \ldots\}$) onto the set of geometrical objects which taken together form the basis of the Galilean system. Moreover, these extended theories, $\{T_V, T_{V'}, T_{V''}, \ldots\}$, are all instances of the T^*-type theory we discussed earlier, and which we argued could not constitute a basis for conventionalism. The point is that the Galilean theory is simply the austere counterpart to each of the Newtonian theories. The addition of the "rigging", of the concept of absolute space, to the Galilean account constitutes the importation or incorporation of some superfluous baggage. We should not be deceived into believing that the notion of an absolute space is integral to Newtonian theory by an historical argument. So far as explanatory power is concerned, the idea is entirely gratuitous. Epistemically speaking, it is definitely not required and to the extent that Newton himself clung to the idea of absolute space we may say that he simply misunderstood his own theory.

What we have arrived at, then, is the following position. Intertranslatability alone is not a sufficient condition for the synonymy of two sets of sentences which are closed under deduction. This is a trivial point which must be conceded. But our empirical theories are more than just sets of sentences. They are sets of sentences which satisfy certain additional requirements. And there is no reason to suppose that the intertranslatability of two theories which are empirically equivalent and which satisfy these further conditions - in particular, the requirement of empirical minimality - is not a sufficient condition of their synonymy. To make his point stick, Friedman would have to find a pair of theories satisfying the following conditions: They would be empirically adequate, empirically equivalent, empirically minimal, and strictly intertranslatable. And he would then have to establish that these theories were somehow distinct in a significant sense.

What Friedman actually presents falls short of this scenario. And in fact it is impossible that it could ever be realized - for empirically equivalent, austere, intertranslatable theories are merely instances of TSC. They cannot be otherwise, unless one is willing to locate ideological differences somewhere other than in the sentences of two theories. One would then have to posit some mysterious faculty of intuiting, extralinguistically, these ideological differences. Needless to say, such differences would be ineffable and impossible to corroborate. We are entitled, indeed obliged, to repudiate this sort of obscurantism.

Of course, the isomorphism of just any two sentences or sets of sentences is no guarantee that they are synonymous. For example, the concept of a harmonic oscillator appears in several branches of physics. And the actual object referred to in each case is different. Similarly we find that Rowland's law, $\Phi = F/R$ (where Φ = flux in maxwells, F = gilberts and R = reluctance) has the same form as the definition of the measure of current flow, $I = Q/t$ (where I = current in amperes, Q = coulombs, and t = time) and the definition of velocity, $v = s/t$ (where v = velocity, s = distance, and t = time). But these three equations are each located in wider and distinct background theories relating, respectively, to magnetic, electrical and mechanical phenomena, which serve to differentiate them. While the equations are isomorphic, they are distinguished by their context. They definitely do not say the same thing. But what differentiates them is a linguistic or symbolic entity - the theory in which each one appears. However, when one reaches the level of intertheoretic isomorphism (i.e., when two complete theories are isomorphic) coupled with empirical equivalence and austerity, then one is dealing with quite a different matter. At that level there is nowhere left to locate "ideological" differences.

To summarize: we have, in this chapter, defended the view that isomorphism or complete intertranslatability is the sufficient condition for the synonymy of theories which are observationally equivalent and which satisfy certain other conditions which have been specified. We have now identified the following conditions which must obtain if GC is sound.:

i) T_1 and T_2 are two theories which are consistent and empirically adequate.

ii) L is the observation language.

iii) $\mathcal{J}$L is the standard interpretation of L.

iv) T_1 and T_2 have exactly the same observational consequences;

that is, for any sentence S, if $\mathcal{I}_L$ is an interpretation of S, then $(S \in T_1 \leftrightarrow S \in T_2)$.

v) The languages L_1 and L_2 (of T_1 and T_2, respectively) each include L.

vi) Terms occur in the sentences of T_1 and T_2 which are not interpreted by $\mathcal{I}_L$ (i.e., there are theoretical terms).

vii) T_1 and T_2 are not isomorphic (i.e., the theories are distinct.)

viii) T_1 and T_2 are each empirically minimal.

ix) For any sentence S, if $\mathcal{I}_L$ is an interpretation of S, and $S \in T_1$ ($S \in T_2$) then there is a minimal subset of axioms of T_1 (of T_2) which is sufficient for S and which includes some sentence or sentences not interpreted by $\mathcal{I}_L$.

x) For any successful extension of T_1 which increases the empirical content of the theory (call the extended theory T_1'), there is an extension of T_2 (call it T_2') such that T_1' and T_2' satisfy conditions i) through x), and conversely.

3. Some Conceptual Confusions

Now that we have established these conditions, we begin to see more clearly that GC does not really possess the strong initial plausibility which seems to be presumed in much of the literature on the subject. Clearly, there are no two theories in existence which satisfy these conditions. (More on this in chapter IV.) Nor are we compelled to affirm the possibility of constructing such theories on any logical grounds. In fact, intuitively GC now looks a rather dubious proposition. Yet conventionalist literature, by and large, proceeds on the assumption that if such theories do not actually exist, they could readily be constructed. This, in the absence of genuine support, is a vast presumption and one which the anti-conventionalist is fully entitled to refuse.

Oddly, many anti-conventionalists who have written on the topic seem themselves to have been prepared to grant that GC has a noteworthy <u>prima facie</u> case. In this respect they have simply been too obliging. Their generosity can only be attributed to a failure to pay strict enough attention to the meaning and the implications of the doctrine. This is just to say that GC has not been stated with sufficient clarity and rigour. There is considerable conceptual confusion operative in the literature which has also served to obscure the real issues.

A few additional words about these confusions are in order. For one, arguments which really support nothing more robust than TSC are frequently mistaken as support for GC. In fact, GC is even conflated with TSC in a most serious fashion. The most striking example of this occurs in P. Horwich's recent article, "How to Choose Between Empirically Indistinguishable Theories".[14] Horwich asks us at one point to consider two total theories, T_1 and T_2, which include atomic physics. T_1 and T_2 differ only in that each occurrence of the term "proton" ("electron") in T_1 is replaced by the term "electron" ("proton") in T_2. Thus, Horwich concerns himself with what he believes are "those cases of underdetermination which involve incompatible, empirically equivalent, isomorphic total theories."[15] He regards this problem as a significant one and proceeds to argue that a non-conventional selection of the standard total theory T_1 may be established by an appeal to our standard reference-fixing practices. He writes:

> Our adoption of a whole theory formulation will constrain the referents of its terms in such a way that the alternatives will violate the requirements of our reference-fixing practice, and can therefore be rejected *a priori*.[16]

But, one may ask, why should one adopt one particular whole theory formulation *a priori* as opposed to any other, to begin with?

Horwich also errs seriously in maintaining that T_1 and T_2 are necessarily incompatible but underdetermined. T_2 is incompatible with T_1 only on the assumption that the reference of the terms "electron"

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[14]Horwich, pp. 62-77.

[15]*ibid.*, p. 65.

[16]*ibid.*, p. 63.

and "proton" is held constant. However, if it is then it is a gross assumption that T_2 will be empirically equivalent to T_1. In fact, if T_1 is assumed to be empirically adequate, then T_2 may well fail to be true to the phenomena. Horwich calls these two theories "potential notational variants".[17] So long as they are potential (as opposed to actual) notational variants, they may be incompatible; but then they may not necessarily be underdetermined. And as soon as they become actual notational variants, by suitable reinterpretation in T_2 of the terms concerned, the problem no longer warrants serious consideration. A clearer case of TSC we could not ask for.

Horwich's error may also be diagnosed at a deeper level. He is apparently treating the terms "electron" and "proton" as what Kripke calls "rigid designators".[18] While the position of the terms within T_1 has been altered in order to produce T_2, they have supposedly retained their original reference - hence they are functioning as rigid designators. But this hardly makes sense; theoretical terms are simply not amenable to treatment as rigid designators in this context. Theoretical terms get their import from two sources: their implicit definition (that is, their relation to other theoretical terms occurring in the postulates of the theory concerned), and (in some cases) *via* their association with observational terms through correspondence rules. If

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[17] *ibid.*, p. 67.

[18] S. Kripke, *Naming and Necessity* (Oxford: Basil Blackwell, 1980). p. 15.

one simply interchanges the position of two terms within a theory as Horwich has done, then one unavoidably alters their implicit definitions, and hence their sense. Syntax is essential in determining the import of theoretical terms. Moreover, on the assumption that the two theoretical terms involved may occur individually in certain postulates, one may well alter the phenomena to which certain law statements are directed by interchanging them. Viewed as rigid designators, we must assume that the terms will carry with them any correspondence rules in which they appeared in the original theory formulation. Thus, as we have suggested, it is by no means self-evident that the new theory will be empirically equivalent to the original.

There are other dimensions to the conceptual confusion inherent in the literature. If Horwich's article is a prime example of TSC masquerading as GC, then on the opposite side of the coin we encounter instances where GC is dismissed as a true but entirely trivial set of claims. And to further confound matters, as we have seen, GC is often mistakenly identified with a defunct programme in the philosophy of science, namely instrumentalism.

4. Empirical Equivalence

A few comments concerning the notion of empirical equivalence are also in order. ~~We have~~ already addressed (Chapter I) very briefly the possibility that GC might be incoherent on the basis that the notion of empirical equivalence, which is crucial to the very concept of underdetermination, may be unintelligible. M. Gardner is one commentator who has explicitly used this gambit in an attempt to dispense with the problem of conventionalism.[19] To this end he employs certain of the standard arguments against the observational-theoretical dichotomy. Our position in this thesis is clear. We have adopted the stance that the distinction stands, and that it must play a pivotal role in any acceptable formulation of GC. The standard repertoire of arguments against the distinction includes the following: arguments which are designed to demonstrate that the distinction is too vague; arguments which purport to show that the distinction is context-dependent (that the boundary which marks the distinction shifts over time as our theoretical knowledge and technological abilities evolve and progress); and arguments which are intended to show that the distinction cannot be made on the basis of an appeal to "privileged access".

The problem of vagueness we have already commented upon. So far as the argument which addresses the alleged "context-dependency" of the distinction is concerned, we may note that, even if sound, it does not necessarily pose a serious threat to GC. A distinction which is context-dependent in the sense indicated may serve as an adequate basis for a formulation of GC. The same may be said for the arguments directed against the possibility that the distinction may be made or justified by reference to something "given" in the sense advanced by radical empiricism or phenomenalism. That the distinction cannot be

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[19] M. Gardner, "The Unintelligibility of Observational Equivalence", in Frederick Suppes and P.D. Asquith, eds., Proceedings of the 1976 Biennial Meeting of the Philosophy of Science Association. Vol. 1, (East Lansing, Michigan: Philosophy of Science Association, 1976).

explained in terms of considerations generated from a foundational epistemology of the sort represented by phenomenalism is, I think, quite clear. Such an epistemology is no longer considered viable. The basis of the distinction is therefore not a question of "privileged access". But again, the circumstance that the distinction is, in the last analysis, a pragmatic one (a matter of degree rather than one of kind) does not entail that it is neither an important nor a genuine one.

There is, however, another type of difficulty associated with the notion of empirical equivalence. In the case of complex theories, it may be extremely difficult to assess their relative empirical content and to establish their strict empirical equivalence. The reason for this is that there is no general decision procedure for observational equivalence. This follows straightforwardly from the fact that there exists no decision procedure for theoremhood in an individual theory. There is a positive test for theoremhood, and hence for observational equivalence. But there is no negative test.[20] In the case where two

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[20]Suppose that we have two theories T_1 and T_2. Take a sentence S in the observation language which is known to be a theorem of T. If we can produce a derivation of S from T_2, we will have confirmed the equivalence of T_1 and T_2 as regards S. If, on the other hand, we can derive $\sim S$ from T_2, then we will have established that the two theories are not empirically equivalent. But what happens if neither S nor its negation is forthcoming from T_2? We are faced with a dilemma in that case. It may be that T_2 is simply not complete with respect to the observational language (i.e., T_2 features neither S nor $\sim S$ as a theorem), or it may be that either S or $\sim S$ is indeed a theorem of T_2 and we simply have not succeeded in formulating the required derivation. This is just to say that we lack a general decision procedure for observational equivalence. Another problem arises from the fact that complex theories will generally yield an infinite number of predictions (some of which for practical reasons may not be testable) so that it may not be possible to actually compare exhaustively their respective empirical content.

theories are strictly intertranslatable, we may have some assurance that they are empirically equivalent. But any pair of theories which exemplify GC are not isomorphic. This does not mean that GC is incoherent. Two theories certainly may be empirically equivalent. The difficulty lies with establishing their equivalence. There is thus a heavy burden on the part of the conventionalist who must persuade himself and others that two complex theories which he maintains exemplify GC are indeed strictly equivalent.

Some words concerning the relevance of W. Craig's theorem to the present discussion might be deemed appropriate. This result constitutes an algorithm for the re-axiomatization of a theory in a restriced vocabulary.[21] Utilizing Craig's result, we may achieve the following. Given a standard scientific theory T expressed in a language L (which contains both an observational and a theoretical vocabulary), it will always be possible to construct another theory T', formulated in a language L' (where L' has the same logical apparatus as L, but where the descriptive terms of L' will consist only of the observational vocabulary of T), which has exactly the same observational consequences as the original theory T. T and T' will thus be observationally equivalent, though they will not be isomorphic. In fact, while a standard theory will generally have only a small finite number of axioms, the "Craigian" counterpart to such a theory may have an infinite number.

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[21]William Craig, "Replacement of Auxiliary Expressions", Philosophical Review, 65 (1956), pp. 38-55.

Thus, Craig's Theorem presents us with the possibility of constructing two "theories" which are guaranteed to be empirically equivalent, and yet which are also known not to be intertranslatable. It does not, however, serve to undermine our contention that no sound argument has ever been produced to support the thesis that alternative scientific theories may exist which are empirically equivalent, but genuinely distinct. In the first place, the Craigian counterpart to a regular theory is indisputably not itself a "scientific" theory: that is, it fails some of the essential requirements of scientific discourse per se. Secondly, while it is true that a regular theory and its Craigian analogue are not intertranslatable - although we have an effective procedure for generating T' from T, we cannot reproduce T from T' - neither are they truly distinct. They are not "distinct" since T' is generated by an effective mechanical procedure from T and since, indeed, this is the only way of acquiring T'.

5. The Statement of Global Conventionalism as a "Reconstruction"

Hopefully, the analysis of GC which has been presented in this thesis and summarized in the present chapter (points (i)-(x)) constitutes a precise and accurate account of the doctrine which avoids certain errors common in the literature. The possibility arises, of course, that in "reconstructing" GC we have merely produced a straw man. Perhaps the reader may be tempted to protest that no one has ever actually asserted the position we have identified as GC. In this present section, we shall try to allay any such misgivings.

As a preliminary we should like to draw attention to the fact that the "straw man" charge is in any event essentially a rhetorical one. Even if it turned out to be the case no one has ever asserted GC as we have characterized it, or anything very close to it, we would still argue that the position we have elaborated is the appropriate characterization of what would count as a significant form of global conventionalism. If what we have presented is not a reasonably exact and reasonably correct reconstruction of what the conventionalists have been trying to assert, then it would appear we are forced to conclude that there is extant a substantial literature which is devoted exclusively to a trivial subject, a subject which is consequently entirely undeserving of such serious and concerted attention.

In point of fact, it seems clear to us that conventionalists have asserted something described by GC. Actually the major proponents of conventionalism seem to have entertained something stronger than the position labelled GC. Recall that the crucial condition imposed on any pair of theories instantiating GC is that they be distinct. Of course, two theories would satisfy this condition if they were incompatible; however, only distinctness was demanded, and this is a weaker requirement. In this respect, we have been as fair and as generous towards conventionalism as one could expect. The major advocates of conventionalism, however, have been more adventuresome. They have asserted that there exist (or that there can exist) scientific theories which are observationally equivalent, but genuinely incompatible.

Quine - at any rate, the Quine who asserted the indeterminacy-of-radical-translation thesis - is certainly one representative of this school of thought.[22] Clearly anyone who subscribes to that thesis is prevented from asserting anti-conventionalism. Even if there is available a translation procedure of the sort previously described, a procedure which successfully transformed a theory T_1 into a putative alternative T_2 (and conversely), such a person would not yet be able to say that the theories concerned are synonymous. In fact, if one carried the indeterminacy-of-radical-translation thesis to its logical conclusion, one could not even maintain that Relativity Theory in German asserts the same thing as Relativity Theory in English. Worse still, one could not even be sure that two Englishmen mean the same thing when they utter identical sentences of Relativity Theory in English. (Of course, the indeterminacy thesis has never been widely accepted. Without entering into the details of the matter, it may be remarked that the chief difficulty is that the thesis relies on a strict behaviourist account of meaning which is generally not regarded as acceptable.)

So adherence to the indeterminacy-of-translation thesis prevents one from maintaining an anti-conventionalist posture. On the other hand, it does not entail a commitment to conventionalism. Agnosticism is a possible alternative. Quine, however, does assert GC, or rather his approximation of it, quite explicitly:

> Theory can still vary though all possible observations be fixed. Physical theories can be at odds with each other and yet be compatible with all possible data even in the broadest

[22] W.V.O. Quine, "On the Reasons for Indeterminacy of Translation", Journal of Philosophy, (1970), pp. 178-183.

> sense. In a word they can be logically incompatible and empirically equivalent.[23]

There is no ambiguity in this passage. It represents about as clear an assertion of GC - the form which asserts the incompatibility rather than the mere distinctness of alternative conventional theories - as one might expect.

Adolf Grunbaum is categorically a proponent of GC, also. In his 1970 article entitled "Space, Time and Falisfiability", he explicitly claims that empirically adequate, empirically equivalent, theories do not necessarily say the same thing - although he maintains they do agree concerning what he calls the "intrinsic" facts.[24]

Reichenbach, also, we take to be a proponent of GC. This requires some explanation, admittedly, since Reichenbach appears to assert in his doctrine of "equivalent descriptions" that empirically adequate, empirically equivalent, theories are synonymous.[25]

We should not allow ourselves to be fooled by such appearances. The first things to note is that Reichenbach and his latter day commentators - both disciples and detractors - believe his conventionalist

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[23]ibid., p. 179.

[24]A. Grunbaum, "Space, Time and Falsifiability", Philosophy of Science, 37 (1970), pp. 469-588.

[25]The doctrine of "equivalent descriptions" is explained in chapter IV of this thesis.

position is a significant one which warrants serious attention. So Reichenbach's views are presumably not to be confused with TSC, whatever else they may amount to.

How, then, does one interpret Reichenbach's claims that the alternative theories he discussed are "synonymous" ("equivalent"). The answer seems quite straightforward. Reichenbach has to say they are synonymous in order to be consistent with his reductionism. His verificationist principles entail that the import, the meaning, of a theory is exhausted by, or encapsulated in, its empirical consequences. On such a view, of course, empirically equivalent theories are by hypothesis synonymous. So in saying that the theories which he (and his critics and expositors) regard as putative significant alternatives are "equivalent", Reichenbach is, in effect, just posing as the victim of his own radical empiricist assumptions.[26]

One can only suppose Reichenbach would have ventured that the alternatives he discussed are syntactically distinct, if he had used more precise language. On any alternative view, it is difficult to see how any sense can by made of the idea that he perceived the alternative theories he described to be of interest from a philosophic standpoint. And one may presume that, had he not been the victim of his

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[26]These assumptions have been abandoned for reasons which are well known. Interestingly, C. Glymour has another refuation of the semantic framework involved in this radical form of empiricism. He argues cogently that it does not allow a proof theory with an effective notion of "proof". See his "Theoretical Realism and Theoretical Equivalence", Boston Studies in The Philosophy of Science, vol. VIII, (1970), p. 278 & pp. 285-286.

verificationist tenets, of his flawed semantics, he would have made the appropriate semantic claim - namely, that the theories he discussed are not synonymous - as well.

Against this reasoning Friedman notes that Reichenbach has actually referred to empirically equivalent theories as "logically equivalent". However, it is noteworthy that the reference Friedman cites is Reichenbach's Experience and Prediction, rather than his The Philosophy of Space and Time where Reichenbach develops explicitly his doctrine of "equivalent descriptions" and his conventionalist views regarding theories incorporating a physical geometry.[27] The reference is an isolated case which may, in fact, simply constitute an instance of careless usage.

Admittedly, one has to be careful about imputing motives to other writers. But the writings of Poincaré, Reichenbach, and even Grunbaum who is still writing, are so ambiguous on a number of points that one is forced, sometimes, to fix a meaning to their words. Part of the problem lies in the vagueness of their language. It would help, for example, if (in the case of contemporary writers) the distinction between a theory and its possible models were employed. But this terminology is rarely utilized. And as we have just seen, we must also deal with the fact that the views of the various proponents of conventionalism are naturally cast in a manner which is consistent with

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[27]Friedman, p. 280 ff. He cites Reichenbach's Experience and Prediction, (Chicago: University of Chicago Press, 1938), p. 374.

other philosophical doctrines to which they are committed. We have indeed put some words in Reichenbach's mouth. But we believe the case for putting them there is a good one: they are the only words that truly fit.

A final comment: it is worth remarking that proponents of conventionalism seem invariably to have held the stronger version of GC which asserts that there are incompatible theories which are empirically equivalent. In some instances these same conventionalists have also supported the no-facts account of conventionalism. This is interesting from the following perspective. On the more doctrinaire, no-facts account of conventionalism, theories are regarded, baldly stated, as mere definitions. Quite apart from the implausibility of the notion that mere definitions could exhibit many of the important characteristics of theories (e.g., the characteristic which has been called "serendipity": the circumstances that theories reveal unexpected results which are subsequently verified, or are sometimes found to be extendable to new domains in a manner not anticipated at their inception) one is entitled to ask in what sense two definitions may be said to be incompatible.

CHAPTER IV

CONVENTIONALISM AND THEORIES INCORPORATING A PHYSICAL GEOMETRY

One of the most intriguing, and perhaps also one of the most vexing, problems in the philosophy of physics is the question of the nature of our knowledge of the geometric properties of the world. More specifically, the question debated is whether our ascription of geometrical properties and relations to physical reality is of an arbitrary or "conventional" character. This is the matter we would like to address in this chapter.

While the more extreme or significant version of conventionalism has been severely discredited as an account of the status of physical theories in general, and rightly so, it continues to enjoy a certain vogue with respect to theories which incorporate a physical geometry: theories of space and time, or of spacetime. The doctrine of conventionalism in this sphere is usually associated with the name of Henri Poincaré. In its more sophisticated version the thesis that an empirical geometry or theories incorporating an empirical geometry are radically "underdetermined" by the data they are designed to explain, and that consequently alternative, incompatible, theories may exist which serve the same data equally well, breaks down into two subvariants. The first of these appears to rest on claims of an epistemological character. Reichenbach is the leading proponent of this species of conventionalism. The second is the "geochronometric" conventionalism championed by Adolf Grunbaum. Grunbaum makes the claim that our geometric theories concerning the world are conventional (arbitrary) in a special sense which does not extend to all physical theories, and that their conventional aspect has an ontological basis in the peculiar nature of the manifold of space, or spacetime.

We shall try briefly to characterize and evaluate these positions. Our contention is that the arguments for the epistemological version really support nothing stronger than TSC. As for the "geochronometric" position, we maintain that this view collapses, in the last analysis, into the epistemological variant, and thence is likewise reducible to TSC.

1. Background

The question which geometry is the geometry of physical reality, or which geometry is the geometry of physical space, first became an issue in the nineteenth century, following the construction of various pure geometries which presented alternatives to the system of Euclid. Prior to the construction of these alternative systems, the matter was unproblematic for the simple reason that only one geometry was known. Newton, of course, had certain physical or empirical reasons for treating space as Euclidean. But the fact remains that he was only acquainted with one system of geometry, and that he apparently had no ground for suspecting that any other was possible. However, once these alternative geometries had been discovered, it was no longer feasible merely to assume that Euclidean geometry provides the exclusively correct interpretation of the relations among physical objects in space. Consequently, as Nagel remarks, the issue of the connection between geometry and physical space - of the status of applied geometry - stood in need of critical reappraisal.[1]

The various alternative systems of pure geometry came about as a result of attempts to determine whether Euclid's parallel postulate was independent of the remaining axioms of the system. For some

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[1]E. Nagel, The Structure of Scientific Theories (New York: Harcourt, Brace, 1961), p. 234.

two thousand years geometers were dissatisfied with the parallel postulate, which supposedly lacked the 'self-evident' character of the others. Attempts to produce a derivation of the troublesome postulate - to show, in other words, that it was a theorem of Euclidean geometry - failed, however. Finally, in the nineteenth century two mathematicians (Bolyai and Lobachewski), working independently, attempted to establish the logical dependence of the parallel postulate by means of indirect proof. Postulates incompatible with the standard parallel postulate were assumed, and the attempt was made to derive a contradiction from the resulting sets of postulates. No such contradiction issued, however. A number of odd theorems (odd as compared with their Euclidean counterparts) were derived, but no contradiction was apparent.

Subsequently, the new geometries created by altering the parallel postulate were shown to be consistent in a rigorous way. Proofs of relative consistency were adduced. In effect, a proof of relative consistency establishes that a given system is consistent if some other system with which it can be coordinated in a certain way (and whose consistency is presumably not in doubt), is. If, for example, an appropriate correspondence is made between terms in the two systems, theorems of pure Riemannian geometry are transformed into the theorems of Euclidean spherical geometry. Nagel uses the expression 'formally intertranslatable' to characterize this relationship between Euclidean and non-Euclidean geometries.[2]

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[2]Nagel, p. 252.

The significance of the interrelation of the various geometries is that it indicates that the question 'What is the geometry of physical space?' is an inappropriate one, so long as we confine ourselves to the subject of pure geometry. To this extent, Poincaré's doctrine of conventionalism - the thesis which asserts that 'geometry is a matter of convention and there is no meaning in a statement which purports to describe the geometry of the physical world'[3] - is certainly vindicated. If we are given a free hand in deciding which objects are to be designated as 'circles', 'triangles', 'straight lines' and so forth; that is, if we are permitted to make the appropriate coordinative definitions (definitions which coordinate or associate the terms of a pure geometry with physical entities), then any of the various pure geometries may be adequate for the analysis and organization of the relationships among objects in space. In a sense, what we are confronted with are alternative systems of notation: 'systems for codifying the same thing in different ways, or different things in the same way.'[4] The apparent incompatibility between the geometries is then explained by the fact that we presumed that the terms 'straight line', 'triangle', etc., must function in the same way, must refer to the same objects, throughout the different systems.

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[3]H. Reichenbach, The Rise of Scientific Philosophy (Los Angeles: University of California Press, 1951), p. 133

[4]Nagel, p. 252.

The first thing to be considered is Poincaré's extension of the doctrine of conventionalism to the realm of applied geometry. Following our comments concerning the interrelation of the various geometries, we might presume that once a set of coordinative definitions has been selected for the terms of a pure geometry, the question regarding the geometry of actual space is immediately transformed into an empirical one, decidable by experimentation only, by carrying out actual measurements. This is the assumption Gauss worked under when he performed his famous experiment involving the triangulation of three distant mountain peaks. When Gauss failed to detect any significant deviation in the angular sum of this triangle from the Euclidean case, he interpreted his results as evidence of the Euclidean character of space. Poincaré, however, subsequently argued that no experiment could ever compel us to favour one geometry over another, and that we would always continue, as a matter of convention, to adopt as our physical geometry the system of Euclid (on grounds of greater simplicity). He thus inaugurated a philosophical controversy which is still going on today.

Expressed succinctly, Poincaré reasoned that had Gauss obtained measurements which indicated a significant departure from Euclidicity, there would have been a way to escape the conclusion that physical space is non-Euclidean. Measuring angles formed by distant objects is achieved by means of optical instruments. Hence, in Gauss's experiment light rays define the sides of the triangle whose properties were investigated. Consequently, had Gauss encountered a non-Euclidean result he would not perforce have been obliged to abandon Euclid. Instead, he could have interpreted the result to mean that light rays are curvilinear (in Euclidean space), and hence that the experimental measurements obtained did not refer to a triangle whose sides were Euclidean straight lines. Now the hypothesis that light rays are curvilinear is one which in principle could be tested. This would be achieved by searching for shorter paths between the objects than those traversed by light rays, with the aid of measuring rods. But the failure to detect a shorter path could be accounted for on the assumption that the measuring rods, when transported along the path of the light rays, were somehow deformed (expanded in this case). And the failure to detect any change in the transported rods on the basis of differential effects on various materials could be explained away on the hypothesis that the forces which produce the deformations are 'universal': that is, they affect the lengths of all physical objects in the same way. Clearly, the presence of these alleged universal forces is not a matter which could be investigated experimentally.

At this juncture, with the introduction of the notion of universal forces, it might appear as if the question concerning the geometry of physical space is once again converted into a matter of pure convention. Regardless of the outcome of any actual measurements we might record, our ascription of Euclidean or non-Euclidean characteristics to space depends on a definitional stipulation concerning the rigidity of transported rods with respect to universal forces (a definition of congruence). The idea of universal forces has all the earmarks of an ad hoc hypothesis, since the only reason for introducing it is to retain some particular geometry. However, as Reichenbach points out, 'there is nothing (inherently) wrong with a coordinative definition established on the requirement that a certain kind of geometry is to result from the measurement.'[5]

Nonetheless, Poincaré's peculiar version of the conventionalist thesis is weak and misleading in several respects. In the first place, his statement of the doctrine carries its emphasis on the role of definitional stipulation too far. Even if it is true that we can formulate different descriptions for any empirical observations, it does not follow that our choice of a description is entirely free from empirical considerations. Reichenbach asks us to consider the following classes of what he calls 'equivalent descriptions':[6]

CLASS I

a) The geometry is Euclidean, but there are universal forces distorting light rays and measuring rods.

b) The geometry is non-Euclidean, and there are no universal forces.

CLASS II

a) The geometry is Euclidean, and there are no universal forces.

b) The geometry is non-Euclidean, but there are universal forces distorting light rays and measuring rods.

He points out that while within each class a) and b) are

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[5]H. Reichenbach, The Philosophy of Space and Time (New York: Dover, 1958), pp. 33-34.

[6]Reichenbach, The Rise of Scientific Philosophy. pp. 136-137.

equally 'true', classes I and II are mutually incompatible - they describe different worlds. The first problem with Poincaré's conventionalism, then, is that it only recognizes the equivalence of the descriptions within such classes while failing to take into account the objective difference between classes. According to Reichenbach, because we ascribe empirical 'truth' to only one such class of descriptions, we actually describe the world objectively, and not merely by convention.

The second error Poincaré made was in assuming that for reasons of convenience or simplicity we will always retain the Euclidean system as our physical geometry. The problem here is that adopting the simplest geometry may not be a move conducive to formulating the simplest, most integrated, physical theory. In fact, as we know, by assuming the spacetime continuum to be a four-dimensional Riemannian space with variable curvature, Einstein was able - contrary to Poincaré's expectations - to construct, in his GTR, a theory which was at once conceptually more integrated, and far more inclusive, than any of its predecessors.

2. "Epistemological" Conventionalism

In some respects (at least, in the superficial ones) the dis-distinctions between the conventionalism espoused by Poincaré and that asserted by Reichenbach are obvious. In others, they are perhaps more elusive, depending on one's reading of the two. The points of disparity would appear as follows:

1) As we have just seen, Reichenbach is more guarded in his

statement of the thesis: he is more careful not to overexaggerate the implications of the possibility of alternative applied geometries for their epistemological status. Specifically, his remarks concerning classes of equivalent descriptions and the mutual incompatibility of such classes make it clear that we are not completely free in formulating descriptions of the geometrical properties of the world. 'Reality', he realized, is a harder mistress than Poincaré assumed. She does impose some constraints on our theorizing.

2) Writing in the era of relativistic physics and familiar with the GTR; Reichenbach was not seduced by the dogma that theories incorporating Euclidean geometry will always be favoured because they will inevitably prove 'simpler'.

3) When confronted with different theories which explain as effectively the relevant data, there would appear to be two possible accounts of the situation. Either the theories constitute genuine alternatives which are truly incompatible with one another, or else they are simply the same theory in different guise. Now, it appears that Poincaré favoured the first alternative; whereas Reichenbach, as an exponent of the empiricist philosophy of science dubbed 'reductionism', must be construed as favouring the latter.[7] Reductionism is the thesis which asserts that the meaning of a theory is exhausted by ('reducible to') its observational consequences. Such an account clearly entails that any two theories with exactly the same consequences have the same meaning; that they are, in effect, simply different formulations of

[7] L. Sklar, Space, Time and Spacetime (University fo California: Berkeley Press, 1974), pp. 119, 133-142.

the same theory. This raises an interesting point, to which we shall now turn, for it seems to suggest that Reichenbach must finally be construed as a proponent of TSC in some form.

Proponents of non-trivial conventionalism tend to support the doctrine only in connection with physical geometries, as remarked. It has been thoroughly discredited as an account of science in general. The standard position, then, within the pro-conventionalist camp is that geometrical theories are somehow more conventional than other physical theories, or conventional in a different sense. And in many cases commentators seem to ascribe this same position to Reichenbach. They are, at any rate, generally inspired by him. But it appears that Reichenbach attributed a conventional aspect to all physical theories, and indeed, to all theoretical concepts.[8] And this simply lends support to the notion that Reichenbach's conventionalism was, in the last

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[8]M. Friedman, "Grunbaum on the Conventionality of Geometry", Synthese, 24 (1972), p. 231. cf. The Philosophy of Space and Time, p. 80, where Reichenbach claims that even our ascription of topological properties to space rests on a conventional stipulation. Of course, if Reichenbach believed that every theoretical term appearing in a theory must be coordinated with an experimental or observable one, then he was mistaken; but this is beside the point. (In most theories, some of the theoretical terms will only be defined implicitly by the postulates in which they occur. In fact, it is probably this very feature which accounts for the ability of such theories to explain a wide variety of experimental laws and phenomena. The assumption that every theoretical term must be explicitly defined is essentially the same error as that involved in operationism.)

analysis, an instance of TSC.

The immediate question, then, is this: How does one make sense of the notion that physical geometries are conventional in some special sense? Presumably, the answer is to be found along the following lines. Physical geometries are conventional in a peculiar way because even after one has elected to use certain linguistic signs to represent metrical concepts (the words 'length' and 'distance', for example), one is still faced with a further choice concerning the use of these signs, a choice concerning which intervals will be said to be equal in length or distance, and this can only be settled by recourse to a conventional stipulation regarding congruence or the operation of universal forces. And, of course, it is this latter choice that determines our geometry. The claim, then, may be that Reichenbach's conventionalism rests upon an epistemological circumstance which renders it interesting, namely, the impossibility of comparing measuring rods that are separated from one another by direct inspection.

I confess that I fail to see how this argument establishes that selecting a definition of congruence is a conventional act of a different sort than is choosing certain symbols to represent our metrical concepts. At most, it might show that the possibility of formulating a certain class of trivial semantic variations of a peculiar type of theory rests on some epistemic fact. But this needn't entail that they are not trivial alternatives.

Surely one can plausibly maintain that by introducing universal forces, one has simply redefined our usual concept of 'length'.

(Or perhaps it might be better to say that we have matched the word with a new concept.) Certain statements which are true in a physical geometry which sets universal forces at zero will not be true in accordance with an alternative total theory that postulates the operation of universal forces in conjunction with another geometry. In particular, statements regarding whether certain intervals are equal, or whether measuring rods that are separated from one another are the same length, will have different truth values. But this is hardly staggering, and it need not have any serious epistemological (i.e., conventionalist) implications. As Sklar notes, if, for example, we interchange the usual meaning of the words 'lion' and 'tiger', then certain statements which were true under the old assignment of meanings - for example, 'Tigers have stripes' - will be rendered false. But it does not follow that zoology is arbitrary in any significant sense.[9]

In the same spirit, we might suspect that the employment of universal forces or the invocation of new congruence definitions will not 'save' Euclidean geometry, where such mechanisms are required to make it feasible, because the meanings of the geometrical terms are not the same as they are in a standard theory employing Euclidean geometry.[10] And the 'Euclidean geometry plus universal forces' theory is just a trivial semantic variation of the equivalent standard non-Euclidean geometry.

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[9] Sklar, p. 97.

[10] _ibid._,

Of course, the case for construing alternative physical geometries as semantic variants rests squarely on the assumption that the meanings of the geometric terms vary from one theory to the other. And this, in turn, requires that geometric terms acquire their meaning essentially from rules of correspondence associating them with observables. But an applied geometry will contain two sorts of statements: theoretic statements containing only geometric terms, and statements containing both geometric terms and terms designating observables. How, then, do we justify the claim that geometric terms get their meaning essentially from the latter? The answer lies in the 'hyper-theoretic' nature of geometric concepts: geometrical terms denote entities that are in principle unobservable. As Sklar's TSC interlocutor comments:

> What on earth gives meaning to the geometric terms, terms designating purely observable entities, aside from their appearance in general sentences in which terms designating observables also appear?[11]

Sklar himself resists the conclusion we have drawn, concluding only that:

> ... questions of the meaning of terms, when the terms are at the most abstract theoretical level as are those of geometry, are simply not amenable to our present way of talking about meaning.[12]

Admittedly, if a realist or quasi-Platonist interpretation of the foundations of geometry is to be favoured, then our position may

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[11]L. Sklar, "The Conventionality of Geometry", APQ Monograph Series, no. 3

[12]ibid.

be jeopardized. Geometrical terms would then have their own 'ontology'. They would refer to objects which exist in some sort of independent realm (which may, of course, be itself mind-dependent). And then it must be conceded that the terms of an applied geometry acquire their meaning in large part from the theoretical postulates (i.e., the ones containing only geometric terms). We are not altogether sure that the TSC account of alternatives could no longer be maintained under these circumstances (a few comments relevant to this point in a moment), but at any rate, the realist position on the foundations of geometry is by no means obviously the correct one.

It should be noted that accepting the TSC position with regard to alternative physical geometries does not necessitate the conclusion that all theoretical terms are 'fictions' or that scientific realism is false in connection with all theories. The theoretical terms of other theories may be less theoretic than those of geometry; that is, they may not be 'hyper-theoretic'. Even if it did, one might be inclined to accept that consequence and regard it as a relatively small price to pay. Meaningful distinctions between the interpretation of theoretical entities as 'useful fictions' and as 'real' existents are not all that easy to formulate.[13]

In fact, opting for some form of instrumentalism has further advantages for the defence of the TSC interpretation of genuine alternative theories. Consider the following. A theory T_1 is clearly a

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[13]cf. Nagel, Chapter 6: 'The Cognitive Status of Theories'.

trivial semantic variation on another theory T_2, if T_1 plus some set of statements, S, of definitional form yields T_2. But what happens if we are confronted with two genuine alternatives - theories with precisely the same observable consequences - but it turns out to be impossible to specify S, the set of definitions which will transform one into the other? If it should thus prove impossible to isolate and identify the changes in meaning which occur between the two theories, the TSC interpretation might begin to look suspect. However, if we accept the instrumentalist account of theories, then we can still maintain that the theories are different formulations of the same theory. In other words, some form of reductionism becomes possible. Of course, we realize that there will be attendant difficulties. But they may still be construed as semantic alternatives, and to this extent the TSC account will be preserved.[13a]

The reader may find these last comments difficult to credit. Fortunately, we do not need to stake much upon them; we don't believe that we actually have to retreat that far, for we challenge him to present us with genuine candidates for non-trivial conventional alternatives - theories for which the changes in meaning which occur cannot be identified, or for which a 'routine' procedure for formulating one from the other cannot be specified.

This last point deserves some elaboration. To begin with, it seems that any convincing argument for the conventional character,

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[13a]The alternative theories will not be trivial variants in the sense that given one, a routine procedure can be specified for constructing the other.

in a significant sense, of any theory or type of theory must begin with the existence of alternatives which are every bit each other's equal as regards their 'saving the phenomena', but which are not readily exposed as trivial linguistic reformulations of the same theory. In the absence of such alternatives, so far as I can see, the question of the conventional status of a theory is, at best, a moot point. But for all the talk about them, such alternatives don't really seem to be in evidence.

Nagel, for one, suggests that it is hardly obvious that successful theories can be constructed which contain provisions for universal forces.[14] However, more recently, Glymour has provided some indications of what a classical theory incorporating universal forces would resemble.[15] (Interestingly, he concludes that introducing universal forces would alter the affine properties of the geometries, but need not directly involve the metrics.) He subsequently sets about sketching a non-Euclidean Newtonian theory. Here, then, we have an apparently <u>bona fide</u> case of two equivalent theories incorporating different geometries. However, the fact that Glymour can supply us with the recipe for obtaining the non-Euclidean variant from the standard theory only reinforces our intuition that they are trivial alternatives.

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[14]Nagel, p. 265.

[15]Glymour, "The Epistemology of Geometry", <u>Nous</u>, 11 (1977), pp. 244-246.

In the case of the General Theory of Relativity (GTR), the position of the conventionalist is somewhat more desperate. There doesn't appear to exist a flat spacetime theory which is every bit the equal of GTR. Two points emerge here. First of all, conventionalists generally acknowledge that the conventional status of the spacetime metric is not on a par with that of the metric of space. Even Grunbaum, the current highpriest of conventionalism, concedes that.[16] So far as the General Theory's spacetime metric is concerned, it would appear that:

> [It] is implicitly specified by the whole system of physical and geometrical laws and 'correspondence rules'. No very small subset by itself fully determines the metric; and certainly nothing that one could call a 'definition' does this.[17]

Secondly, as suggested, the alternative required to base the conventionalist's case simply isn't available (although there are a number of pretenders to the role). And it is not obvious that a Euclidean (flat) spacetime theory can be formulated which could pose as a genuine alternative to GTR.

Glymour discusses some of the work that has been done in this vein.[18] A number of special relativistic theories of graviation have been constructed, theories which attribute the Minkowski metric of

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[16] A. Grunbaum, Geometry and Chronometry in Philosophical Perspective (Minneapolis: University of Minnesota Press, 1968), 'Reply to Putnam'.

[17] H. Putnam, 'An Examination of Grunbaum's Philosophy of Geometry', in B. Baumrin (ed.) Philosophy of Science: The Delaware Seminar, vol. 2 (1962-63).

[18] Glymour, pp. 242-249.

STR to spacetime, and which treat gravity as a field separate from the metric field. The more successful of these treat the gravitational field as a tensor field, and in some cases as a dynamical one (i.e., as one which varies according to the contingent distribution of matter and energy in local regions). The general strategy, then, has been to separate the dynamical metric field of GTR into two components: a fixed Minkowski metric, and a dynamical gravitational field tensor.

The most sophisticated versions are even somewhat more complex. For example, the procedure in the case of Thirring's flat spacetime gravitational theory is to take the metric field of some set of solutions of the field equations of GTR, divide the metric into the two components referred to above, and then formulate new field equations which are satisfied by these objects. But does Thirring's theory, for example, afford a serious alternative to GTR? Apparently not: it even turns out to be inconsistent.[19]

Time now to pause and take stock of our position. So far we have argued that:

1) Nothing Reichenbach or Poincaré have to say demonstrates that alternative physical geometries are arbitrary or conventional in any interesting sense.

2) It appears that it may be possible to make a case for the position that any alternatives we might encounter, any physical geometries which equally save the phenomena, are simply linguistic variations of the same theory.

3) This forces us to an instrumentalist interpretation, of

[19] Glymour, p. 244.

sorts, of physical geometries. But this is made plausible by the 'hypertheoretic' character of geometric terms, and need not compel us to regard all theoretic terms as designating 'fictions' or constructs.

4) Convincing arguments for the conventional character of applied geometries must begin with the presentation of genuine viable alternatives which are not readily exposed as trivial semantic variations or reconstructions. Neither in the case of relativistic nor Newtonian science do these alternatives exist. (They are simply hypothesized, and this won't do. This is not to say, however, that there are not a number of examples which come close. It's just that coming close isn't enough.) It may be possible actually to construct non-Euclidean counterparts to Newton's theory, but these are readily seen to be cases of TSC. In the case of relativistic physics (GTR, at any rate), the required alternatives aren't apparent at all.

Based on 1) - 4), we conclude that the doctrine of the conventionality of physical geometries hasn't been established by any of the arguments considered thus far, arguments based on the comments of Poincaré or Reichenbach.

What remains to be considered is the last recourse of the conventionalist: refuge in ontological considerations. We must, then, consider briefly the 'geochronometric' conventionalism championed by A. Grunbaum.

3. "Ontological" Conventionalism

Grunbaum maintains that the conventionality of the metric (and hence, of the geometry which it determines) has its basis in certain ontological or structural features of space, specifically, its continuity. And it is this fact which he believes renders this conventionality significant. We find Grunbaum's arguments difficult to fathom (and many of his commentators do!), and it appears that they can be discredited on a number of grounds. We shall begin with a statement of the substance of his argument.

Grunbaum reasons that if space were discrete or granular, then the relationship of congruence would be an intrinsic property of pairs of intervals. By 'intrinsic' Grunbaum appears, ultimately, to mean 'topological'. The point is that in a discrete or quantized space, congruence could be fixed by the topology: the distance between any two points could be defined by the number of spatial units or quanta between them. However, as a contingent matter of fact, space is not discrete; it is continuous, and the cardinality of any two intervals is therefore identical. Consequently, there is no intrinsic property of the intervals of our space by reference to which they might be declared equal or unequal. Which intervals are equal is determined solely by the measuring procedures or the congruence definitions we adopt. And these are conventional. No intrinsic property of a continuous space imposes a particular choice on us. In Grunbaum's terminology, a continuous space is 'metrically amorphous'. (Grunbaum originally claimed that the continuity of space is a sufficient condition for its 'metrical amorphousness'.[20] This is a mistake, as he later acknowledged,

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[20] A. Grunbaum, Geometry and Chronometry. "op cit".

though apparently not a serious one. Continuity is actually only a necessary condition. We require also the 'homogeneity' of space.)[21]

Now, it must be conceded that physical space is metrically amorphous. But the question remains whether Grunbaum's claim that his conventionalism is distinct from that of Reichenbach can be upheld. Is Grunbaum's contention that the conventionality of the metric is special because it rests on ontological considerations defensible? If it can be undermined, and it appears that it can, then Grunbaum's claim to have established the significant character of the conventionality of geometry fails, and the last support for conventionalism has been removed. In brief, the problems with Grunbaum's position appear to be as follows.

1) To establish a measuring procedure in a continuous space, we require two elements. We must first assign coordinates (an arbitrary act) to the manifold. Then we select a metric function. (The simplest metric function would associate the distance between two points with the numerical difference between their coordinates. But, of course, an infinite variety of alternative functions are available). Having established a measuring procedure in this manner, one might be prompted to think that one could determine whether the intervals which are equivalent according to the procedure implemented are really equal, by means of measuring rods. But, of course, this is not so, as we also require a definition of congruence specifying the behaviour of

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[21]R.B. Angel, Relativity: The Theory and its Philosophy (Pergamon Press, 1980), p. 237.

the rods. But then the following question arises: Since Grunbaum's ontological thesis requires, ultimately, this latter point, why doesn't it reduce in the last analysis to an epistemic or epistemological claim like that attributed to Reichenbach (and thence to trivial conventionalism)?

2) To the extent that Grunbaum's argument for the special character of the conventionality of the metric rests upon the contrast with a possible space which does possess intrinsic metrical features, it may collapse.

In the first place, it is not clear that the notion of a quantized space, which Grunbaum appeals to, can be described in a coherent way. For one, it is not obvious that such a space would have a definite dimensionality, other than by conventional stipulation. For another, as of yet we do not appear to have a fully developed geometry for discrete spaces. In light of these two points, it is questionable whether the claim that a discrete space would have an intrinsic metric can really be counted a meaningful one.[22]

Secondly, even if the notion of a discrete space is an intelligible one, in order to defend the claim that the topology in such a space can fix the metric, we still require the assumption that the quanta are equal, and this presumably would require a conventional stipulation. Also, while it is true that in discrete space we could define a metric in terms of the topology, we are not compelled to. As

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[22] W.C. Salmon, Space, Time and Motion (Dickenson Pub.: Encino, California, 1975), p. 66.

Sklar points out, even discrete spaces allow of different metrics, i.e., of different functions on pairs of points that satisfy the axioms: $M(x,y) = m(y,x)$; $m(x,x) = 0$; $m(x,y) \geq 0$, and; $m(x,y) \leq m(x,z) + m(z,y)$.[23]

Finally, we might recall that Reichenbach claims that even our ascription of topological features to space requires a conventional stipulation: one which rules out causal anomalies.[24]

3) Another strategy for countering Grunbaum's argument is developed by Michael Friedman.[25] In a more careful analysis of what an 'intrinsic' property amounts to, he concludes that Grunbaum must intend by the term any property which is not definable in terms of topological and/or ordering relations. Again, he admits that in this sense the metric of continuous space is not 'intrinsic'. But the philosophical question to be answered, he says, is whether this supports the claim that metrical properties are not really 'objective' properties of continuous space. He suggests that Grunbaum's discussion of discrete and continuous spaces simply doesn't establish this latter assertion. Grunbaum merely demonstrates that the metrical properties of a continuous space cannot be defined in terms of its topological and order relations. And this simply shows that the metric of a physical geometry is a primitive notion of the theory. It doesn't necessarily follow from this that the metric is not an objective property of physical space. Cardinality and order relations are similarly primitive terms.

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[23]Sklar, (1974), p. 111; (1969), p. 48.

[24]Reichenbach, (1958), p. 80; also Sklar, (1974), pp. 101-103.

[25]Friedman, (1972).

But Grunbaum claims that these are objective ('built in').

Of course, Grunbaum appeals to the availability of alternatives to support the claim that metrical properties, unlike topological properties such as cardinality, are not objective. But this argument presupposes an intimate connection between the objectivity of a property and the existence of alternative theories about it. Friedman points out that Grunbaum never argues for such a connection, and that its existence, at any rate, is highly implausible: 'The existence of alternative theories is a matter of our ingenuity and creativity; whether or not a property is objective is presumably a matter of ontological fact.'[26]

4) Finally, while Friedman's rebuttal consists in arguing that Grunbaum fails to prove that metrical properties are not 'objective' properties of physical space, there is another alternative available.[27] Angel's strategy is roughly as follows: He begins by admitting that space doesn't have an 'intrinsic' metric. But then he proceeds to ask why we should bother to endow it with one. In his opinion, all the talk about the metric of space just constitutes a philosophical 'red herring' for which Poincaré may assume the credit.[28] Physicists are interested not in the geometry of empty space, but rather in the geometry of physical or material systems, in particular, of the gravitational field. This immediately makes conventionalism less plausible,

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[26] ibid., p. 229.

[27] Angel, op. cit.

[28] Angel, p. 241.

for the answer to the question 'What is the metric of the gravitational field?' is determined by observing the paths of gravitating masses and photons - the geodesics of spacetime. Angel acknowledges that he thus draws a distinction between spacetime and gravitation which runs counter to current orthodoxy. But given that the rationale behind the popular view is open to question, this may not prove a serious objection. On the orthodox view, if there is no mass-energy then there is no gravity and no space: in short, there is nothing. Angel's view, however, seems to be that an absence of mass-energy only entails the absence of gravity. This is a defensible position. The existence and distribution of mass-energy does not determine all of the features of spacetime. Matter determines the metric and affine properties of spacetime, but it does not determine the topological properties. For example, spacetime is four-dimensional; however, matter doesn't make it four-dimensional. Similarly, the continuity of space and the fact that it is differentiable are characteristics which are not determined by the presence of matter.

It seems, then, that Grunbaum's position is highly problematic. It can by assayed from a number of angles, and a variety of serious objections emerge. Apparently we have good grounds for rejecting geochronometric conventionalism, the conventionalist's last ploy.

4. A Diagnosis

Having made an effort to dispense with the basic arguments advanced in favour of the conventionality of physical geometry, we would like to conclude with a few remarks concerning what must motivate the doctrine. As Sklar remarks, if we reject conventionalism it is incumbent on us to provide some explanation of its popularity. On our account, some of the most serious thinkers in the field over a considerable period of time have been duped. If our rejection of conventionalism is to be really plausible, we must somehow account for this circumstance.

According to Sklar, one reason is simply the inertia of a two thousand year old doctrine asserting that the geometry of the world can be known a priori, a doctrine that began with Aristotle and climaxed in Kant's assertion that geometric truths are synthetic a priori propositions. Old philosophical theories and traditions often die hard (even then, they sometimes don't stay dead!). Witness the various attempts to defend Kant's thesis in some diluted form.[29] And as recently as 1974, an article appeared in Kant-Studien, the journal devoted to Kant studies, attempting to defend Kant's theory of geometry in total.[30] From the point of view of Poincaré's doctrine, we can know that space is Euclidean a priori, because we are allegedly free to choose it as a conventional description. And Poincaré, of course, set the stage for the subsequent development of more sophisticated versions

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[29]B. Russell, "An Essay on the Foundations of Geometry" (1897), discussed in R. Torreti's Philosophy of Geometry from Reimann to Poincaré (D. Reidel Publishing Co.: 1978). Russell argued that we can know a priori that the curvature of space is constant.

[30]J.L. Wiredu, "Kant's Synthetic A Priori and the Rise of Non-Euclidean Geometries", Kant Studien vol. 61 (1970), 5-27. Wiredu tries to undermine the argument - now generally accepted - that Kant's synthetic a priori theory of geometry cannot be maintained in the face of the development of successful non-Euclidean applied geometries. His argument constitutes an attempt to demonstrate the mutual compatibility of Euclidean and non-Euclidean applied geometries.

of conventionalism.

But why should conventionalism be considered plausible in the case of applied geometries, while it is not deemed so with respect to other types of theories? Again, the answer is apparently fairly straightforward. According to Sklar's diagnosis, the salient fact is that geometry was the first theory to be completely axiomatized, and it is still the only one to be so neatly and elegantly formalized. This has two consequences. In the first place, it has an historical influence already alluded to. Geometry was the theory which 'suggested itself to everyone from Aristotle to Kant as the paradigmatically

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He begins by distinguishing between what he calls 'weakly' and 'strongly' non-Euclidean geometry. A weakly non-Euclidean geometry is a 'set of geometrical postulates containing at least one member which is different from, but not incompatible with, any thesis of Euclidean geometry'. [7] A strongly non-Euclidean geometry, in contrast, 'contains at least one thesis which is incompatible with at least one thesis of Euclidean geometry'. [8] To say that two propositions are incompatible is to say the one implies the strict contradictory of the other and vice versa.

According to Wiredu, all known non-Euclidean geometries are only weakly non-Euclidean, hence they are not incompatible with Euclidean geometry. It follows, if this view is correct, that we cannot argue against Kant's theory by appealing to the existence of non-Euclidean geometries.

On the face of it, Lobachewskian and Riemannian geometries each contain a postulate which is incompatible with one of Euclid's. The former substitutes for Euclid's parallel postulate the assumption that through a point external to a given straight line there are at least two parallels to the given line; in the latter, the parallel postulate is supplanted by the assumption that there are no parallel lines. If we designate the three alternative postulates 'E', 'L' and 'R', respectively, then it would appear that $L \rightarrow \sim E$ and also $R \rightarrow \sim E$: i.e., Lobachewskian and Riemannian geometries would appear to be incompatible with the Euclidean system. What is overlooked, Wiredu suggests, is that E varies in its context. It does not represent the same proposition in the Euclidean geometry as it does in the statements '$R \rightarrow \sim E$' and '$L \rightarrow \sim E$'. More specifically, it is the term 'straight line' which varies in its context, having a different significance in each of the alternative postulates.

a priori theory.'[31] Again, this presumably inspired Poincaré. But more importantly, only in the case of physical geometries is it possible to actually construct (or imagine constructing), with some ease, the 'alternative' theories which make conventionalism seem plausible. And this, of course, is a circumstance which is due directly to the elegant formalization of geometry.

Wiredu then goes on to assert that since both Euclidean and non-Euclidean geometries are applicable - the former within our local physical environment, the latter to astronomical regions of space - we cannot but conclude that they are compatible: 'Surely if the Euclidean-thesis is analytic, then any genuine contradictory or contrary of it will be ... self-contradictory and, *a forteriori*, physically inapplicable'. [10]

In the first place, it is difficult to see how we can reconcile the assertion that both Euclidean and non-Euclidean geometries are applicable with Kant's thesis that there exists but one form of outer intuition. Secondly, Wiredu fails, it seems, to appreciate the relation between physics and geometry. The Kantian doctrine that geometry is about space obscures the distinction between pure and applied mathematics - for on this account both a pure geometry and a physical theory employing a geometry are essentially about the same thing. This tends to obscure also the role of definitional stipulation in applied mathematics. Clearly, once we have supplied the required coordinative definitions associating the terms of pure geometry with certain physical entities and have adopted a definition of congruence (a conventional stipulation regarding the behaviour of transported measuring rods), the question which geometry is the geometry of physical space is a matter for empirical investigation as Gauss recognized, and the Euclidean and non-Euclidean geometries are incompatible in so far as measurement will confirm one or the other, but not both. The point Wiredu presumably misses is that the term 'straight line' does not vary in its meaning if we coordinate it, for example, with the path of a light ray.

[31] Sklar, (1969), p. 56.

CONCLUDING REMARKS

Previously we drew attention to Craig's Theorem and discussed its possible implications for conventionalism. Before concluding, a second result which might appear to have some bearing on the issues at hand needs to be considered, namely, the Lowenheim-Skolem Theorem.[1] This theorem asserts that any satisfiable first-order theory expressed in a countable language has a countable model. Of particular interest is the strong form of the theorem which is referred to as the "downward" Lowenheim-Skolem Theorem. The downward version of the theorem states that any satisfiable first-order theory expressed in a countable language exhibits a countable model which is a submodel of a given (nondenumerable) model.

This result would seem, at first blush, to lend some support to conventionalism, or at least to open the way to a new formulation of the doctrine which might prove sound. It indicates that there may exist two models for a given theory and that, moreover, these models may be non-isomorphic. Furthermore, satisfaction of the various requirements encapsulated in our formulation of GC (Chapter III) - including syntactic requirements such as austerity - does not exempt a theory from this result. Consequently, it may occur to the diehard conventionalist that there remains the option of retreating entirely to semantic

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[1] John Bell & Moshé Machover, A Course in Mathematical Logic (Amsterdam: North Holland Pub. Co., 1977), pp 168-173.

considerations. So inspired, he may try to develop his position along the following lines: "Conventionalism is a true and significant claim which is not crucially dependent on the syntax of our theories. It is a claim about the structures in which the disinterpreted calculus of an empirical theory (that component of an empirical theory which corresponds to the concept of a "theory" in the formal or model-theoretic sense of the term) is satisfied: its models. Conventionalism is thus not a claim about the underdetermination of our theories in any sense which can be captured through essentially syntactical considerations. Rather, it is a claim about the underdetermination of the models for a given theory. It is the various possible models of a given theory - and hence the ontologies conveyed by these models - which are underdetermined." The conventionalist would then presumably assert that this conception of conventionalism derives support from the Lowenheim-Skolem Theorem.

It is the spectre of precisely this sort of "conventionalism" which is raised by Putnam in an article entitled "Models and Reality".[2] Putnam addresses the problem of how we can specify which of the models that satisfy a theory is its intended model:

> Now the arguments that Skolem gave, and that shows that the "intuitive notion of a set" (if there is such a thing) is not "captured" by any formal system, shows that even a <u>formalization of total science</u> (if one could construct such a thing), or even a <u>formalization of all our beliefs</u> (whether they count as "science" or not), could not rule out denumberable interpretations, and, <u>a fortiori</u> could not rule out unintended interpretations of this notion.[3]

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[2]Hilary Putnam, "Models and Reality" <u>op. cit</u>.

[3]<u>ibid</u>., p. 466.

Putnam proceeds to outline a solution to this problem of specifying the intended interpretation of a first-order theory. Whether this solution is successful or not is an issue which is not really relevant to our assessment of the form of conventionalism we are presently contemplating. Let me explain this briefly. If we grant that the Lowenheim-Skolem Theorem really has implications regarding the possibility that the models of our theories (and hence the ontologies that they convey) may be underdtermined in a significant sense, then it is of no help to assert that we can specify the intended models. This only ensures that we can know exactly what we are saying, so to speak. It does not follow that what we are saying is necessarily true. An assurance that we can know exactly what we are asserting leaves intact the metaphysical and epistemological problems suggested by the existence of alternative ontologies which are compatible with the syntax of a given theory. Even if we can know which is the intended model, how can we be sure that this particular model, as opposed to any alternative "unintended model," reflects the true ontology of the world (assuming, as we do, that the latter phrase is meaningful)? We seem to be confronted with the circumstance that it may only be possible to know the "syntax" of the world, that the "semantics" may escape our theoretical nets: a modern analogue, perhaps, of the Aristotelian maxim that knowledge is of form, not of matter.

A closer look at the Lowenheim-Skolem Theorem will put to rest any fears that the conventionalist is building a case for himself. The theorem asserts that if we are presented with a nondenumberable model M of a theory T which is formulated in a countable language, then we can always find another model M' of T, where M' is a countable model. But now comes the sticking point. The predicate symbols contained in M', while restricted to a domain of suitable size, will represent the same relations as they did in M. Thus, the Lowenheim-Skolem Theorem only has implications regarding the size of the domains of our models. This hardly qualifies as the basis for an interesting or significant form of conventionalism. If all the conventionalist wishes to assert is that our physical theories, which include "magnitudes" (real numbers) and therefore have nondenumerable models, also have countable models, then we may be willing to grant this point without feeling that we have conceded anything of real note. In fact, we virtually acknowledged this point in Chapter I, where we suggested that the incorporation of the real number system in our physical theories is an instance of a local convention.

Furthermore, it may even be that the intended interpretations of our theories are actually denumerable ones. One current cosmology posits the existence of a finite number of particles in the universe. Presumably, if once we produced a formulation of this cosmology which relied on the rational number system rather than the real number system, then we would have a genuinely countable model of the universe. Of course, such a formulation would inevitably prove cumbersome and impractical from a computational standpoint, but we are not concerned with matters of mere convenience here. In fact if we begin from the standpoint that the "intended" model of the universe is just such a truly countable one, then the problem posed by the "Skolemization" of nondenumerable models does not even necessarily arise.

Now that we have dispensed with this final possible recourse of conventionalism, we are in a position to summarize what we have accomplished. Admittedly, there is no decisive refutation of GC to be found lurking within the pages of this thesis - if what one intends by a "decisive refutation" is a demonstration that the doctrine is false. In fact, we have actually rejected a number of purported refutations of conventionalism -- specifically those attributable to Putnam and Horwich -- which have been published recently. However, we suggest that we have at least succeeded in establishing the implausibility of GC. We have accomplished this by refuting the principal arguments and examples which comprise the existing case for GC, and by providing a clear statement of the doctrine.

To reiterate our findings: there appear to be no sound arguments in existence which support GC. What arguments there are to be found presently in circulation in the literature, as well as a few candidates we constructed ourselves within this thesis, have proved on examination to be invalid and/or to rest on faulty assumptions. Invariably they have been found to trade on serious conceptual confusions. A prevalent confusion, we have witnessed, is the failure to properly identify (or to keep distinct, if once identified) the doctrines of TSC and GC. These two doctrines have been conflated in one way or another by both opponents and advocates of GC alike. As an example of the former, we have seen how Horwich was inspired to represent as GC a position which is readily identified as TSC. On the opposite side of the coin, we have also discovered that the major arguments mustered in support of the significantly conventional status of theories which incorporate a physical geometry are arguments which actually support nothing stronger than TSC.

So we have determined that no current argument is adequate to support GC. Similarly, we have found that none of the examples cited to support the doctrine carries any real weight. No pair of theories has ever been constructed which satisfies the conditions advanced by our more rigorous formulation of the doctrine presented in Chapter III. Nor are we presently compelled on any general logical grounds to assert that such theories could necessarily be constructed. In particular, it appears GC does not derive any support from either of two results which might appear at first blush to carry such implications, namely, Craig's Theorem and the "downward" Lowenheim-Skolem Theorem. Furthermore, our intuition may well suggest the unlikelihood that it is possible to construct such a pair of theories.

The burden of proof now lies with the conventionalist. As it stands there is simply no case for GC, and the onus is on the conventionalist to advance a new argument if he wishes us to entertain his point of view. Such an argument may be forthcoming; it is still conceivable that conventionalism is true and that at some point in the future we will find ourselves confronted with, say, a formal existence proof or even a compelling example (i.e., an instantiation) of GC.*

*An existence proof would not, of course, be decisive in itself. Much would depend on the assumptions built into the proof, Such things may come to pass. We can only say that as of this moment we find it improbable.

Finally, quite apart from our assessment of the case for GC, we hope that we have performed a useful service in our attempt to clarify the meaning of the doctrine and in our efforts to analyze its relations to other forms of "conventionalism" and to instrumentalism, scientific realism, and the model-theoretic approach.

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and the extent to which it could be assured that the theories addressed in the proof satisfied the formal requirements appropriate to physical theories. Still, an existence proof would do much to further the case for conventionalism.

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