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Philosophy of Science, Vol. 39, No. 3 (Sep., 1972), 322-340.

Stable URL:
http://links.jstor.org/sici?sici=0031-8248%28197209%2939%3A3%3C322%3APBC%3E2.0.CO%3B2-A

Philosophy of Science is currently published by The University of Chicago Press.

DISCUSSION

PHYSICS BY CONVENTION*

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> "*It ain't nuthin' until I call it.*"
> *Bill Guthrie, Umpire*

Numerous criticisms of Adolf Grünbaum's account of conventions in physics have been published ([1], [2], [3], [4], [13]), and he has replied to most of them ([6], [8]). Nonetheless, there seem to me to be good reasons for offering further criticism. In the first place Grünbaum's philosophy seems to me at least partly an extrapolation of one aspect of the views on conventions developed by Reichenbach and others. Since I think many of the issues which Reichenbach attempted to settle in his various discussions of conventions in physics are genuine and important, and I also think that those aspects of his views on which Grünbaum has focused are among the least satisfactory, it seems important to suggest questions and answers about conventions in physics which may develop more satisfactory Reichenbachian themes. Secondly, Grünbaum's philosophy centers on a distinction, that between extrinsic and intrinsic properties and relations, which has never been made satisfactorily clear either by Grünbaum or by his critics. Until it is clarified we will remain unsure of just what he is claiming and why we should think it true or think it false. Finally, Grünbaum's replies to his critics, especially his most recent reply, [9], involve unusually important claims which fail to be buttressed by the arguments he gives. I have in mind such claims as that we can learn something important about the ontological status of properties and relations by examining *descriptions* of them, that on (what I take to be) the most straightforward and literal interpretation, the general theory of relativity is inconsistent, and that the foremost advocates of geometrodynamics, Clifford and Wheeler, were and are enmeshed in contradiction. My own view is that all of these claims are dubious or false, but I shall be less concerned with establishing their falsity than with discrediting the arguments offered for them.

Reichenbach, [16], distinguished between sentences which express claims about the world and sentences which express decisions. The latter, he claimed, are of two kinds, "conventions" and "volitional bifurcations." Conventions are decisions which, in some sense, make no difference. Roughly, if either accepting or refusing to accept a sentence will not affect the total content of our system of knowledge, then the sentence is a convention. In such cases the results of two different alternative decisions are said to be "equivalent descriptions." Conventions together with

* Received August, 1971.

empirical facts may require, for consistency, the adoption of still further conventions, which Reichenbach called "entailed conventions." Whether a sentence in our scientific corpus is a convention *tout simple* or merely an entailed convention is entirely relative to a given rational reconstruction. But whether a sentence is conventional (in one way or the other) or empirical Reichenbach seems to have thought an entirely objective matter, a matter of "logical fact." One of the chief tasks of epistemology, according to Reichenbach, is to locate the conventions within the scientific corpus. The means he suggested derives from his "probability theory of meaning" according to which two sentences have the same meaning if they receive the same degree of inductive support on every physically possible observation. A sentence can be deemed a convention if we can elaborate two theories, one containing the sentence, the other containing instead some alternative sentence, so that both theories (and both sentences) receive the same weight on every physically possible observation. The evident difficulty is that Reichenbach provided us no details regarding how, on the basis of experiment, to rationally assign different probabilities to observationally equivalent hypotheses.[1] In practice, when Reichenbach analyzed particular theories such as special relativity or quantum mechanics, the probability theory of meaning seems to have been given only lip service and a more robust criterion of synonymy employed: two sentences (or theories) say the same thing if they are observationally equivalent, that is, if they make exactly the same claim about the outcome of every physically possible experiment. Reichenbach's several analyses of conventions in physical theories center on the development of such alternative empirically equivalent theories. In his presentations, the choice from among the alternative theories pivots on the choice of one sentence from among a class of "conventional" alternatives.

Now there are any number of philosophers who appear to think the verifiability theory of meaning very implausible and who think both obscure and dubious the

[1] The few examples he did give are unconvincing and even puzzling. For example:

> Einstein demands that two watches equally regulated during a common stay at A and moved in different ways and with different velocities toward B, will show at B, after their arrival, a difference in their readings. We can imagine a world in which this is not the case, but in which the indications of two watches are in correspondence after the different transportations from A to B. In this world transported watches would define a simultaneity which we call *transport time*, and we would say: If there were no upper limit to the velocity of signals, the infinite velocity would determine with great probability, as simultaneous to t_2 that time point t_2 which corresponds to the transport time. ([16], p. 128)

But it would seem that whether or not clocks which are in synchrony when together remain in synchrony when separated is just the sort of question which on Reichenbach's view is to be answered only by a convention; for certainly the question here is very like the question of whether separated rods remain congruent. And, in fact, Reichenbach himself said as much in another place:

> However, if relativistic physics were wrong, and the transport of clocks could be shown to be independent of path and velocity, this type of time comparison could not change our epistemological results, since the transport of clocks can again offer nothing but a *definition* of simultaneity. Even if the two clocks correspond when they are again brought together, how can we know whether or not both have changed in the meantime? This question is as undecidable as the question of the comparison of length of rigid rods. Again a solution can be given only if the comparison of time is recognized as a definition. If there exists a unique transport-synchronization, it is still merely a *definition* of simultaneity. ([17], p. 133)

notion that scientific sentences can be classified, once and for all, as simply empirical or conventional. I tend to agree, but even so I think it obvious that Reichenbach was doing something important in his analyses of particular physical theories. What he did, for example with special relativity, was at least to suggest how to develop alternative theories, and to argue, often convincingly, that in spite of apparent contradiction two such theories cannot be distinguished experimentally. For special relativity the alternative theories he suggested involve different synchronizations of distant clocks and hence different judgments of simultaneity even within the same inertial frame. The arguments given for their empirical equivalence are of two kinds: first, the consideration of hypothetical crucial experiments and the demonstration that if either theory is true the experiments cannot be performed or cannot accomplish their purpose, and second, the demonstration that the two theories have a common body of claims which are intuitively close to observation claims.[2] Reichenbach used both strategies for special relativity; more recently, Professor Winnie, [19], has characterized, for the one-dimensional case, those kinematic claims which are common to all of Reichenbach's alternative special relativity theories. Such analyses of theoretical equivalence have a value which is independent of Reichenbach's particular views about conventions. When a case can be made that two or more theories can serve the same scientific functions and are indistinguishable by any possible test, important philosophical questions arise immediately: are the two theories really just different presentations of the same theory? Are they both true? Are they contradictory? Are there rationally grounded methodological principles which will distinguish among them? And so on. I do not believe that these questions always have unambiguous answers, but sometimes they may, and even where they do not there is something to be learned in locating the ambiguities. Moreover, it is sometimes possible to suggest generalizations about empirical equivalence; that is what I take Reichenbach to have been doing when he wrote that "Topology is an empirical matter as soon as we introduce the requirements that no causal relations must be violated . . ." ([17], p. 80). I understand Reichenbach to have meant that if we find two theories which are empirically equivalent but ascribe different topologies to space, then at least one of these theories must also postulate "anomalous" causal relations. While I think this particular claim false, and have given reasons for my skepticism in [6], the justification or refutation of such generalizations is an epistemological enterprise which is both possible and important.

Now there is, at least implicitly, another, more metaphysical, aspect to Reichenbach's views on conventions. Rather obscurely, Reichenbach suggested that conventions arise because certain properties or relations are not "objective." The result of

[2] Other strategies may sometimes be available. If we look at special relativity four-dimensionally—that is, take the theory to specify a topologically Euclidean differentiable manifold together with an indefinite (Minkowski) tensor field—it becomes apparent that the alternative synchronization rules (e.g. those of Ellis and Bowman [1]) simply express different coordinatizations of the manifold and, consequently different representations of the metric tensor, the Lorentz group, etc. All of the geometrical invariants are the same. The "non-standard synchronization rules" correspond to simple coordinate transformations. If, then, we take seriously the principle of covariance it follows that the alternative versions of the special theory are empirically indistinguishable.

the analysis of conventions, he said, is to separate "physical facts" from "subjective features" of our descriptions. The idea, it appears, is that sentences which are conventions do not describe real states of affairs, and one of the reasons they may fail to do so is that certain properties they describe are *really* relations. Thus he claimed that the analysis of conventions in physical geometry shows that when we talk of length we are really talking about a relation between rigid rods and point pairs, not about a property of point pairs. But nowhere in Reichenbach's writings have I found an unambiguous statement of the relations between conventions and equivalent descriptions on the one hand, and the ontological status of properties and relations on the other. A similar point of view is much more explicit in the writings of Poincaré [14], who maintained that the axioms of geometry are concealed "definitions." They are not "experimental facts" because geometric properties of real space are in fact not properties of space itself at all, but rather just relations among material objects: "If, then, there were no solid bodies in nature there would be no geometry" ([14], p. 61).

Both Reichenbach and Poincaré, but the latter more explicitly, suggest an ontological *explanation* of why some sentences are conventions and others not. The explanation seems to turn on certain properties *actually* being relations, or certain properties of space depending (in an unspecified sense) on material objects, or both. The conventionalist views of Professor Grünbaum are, I think, extrapolations of the ontological aspect of the views of these earlier conventionalists. I have suggested that those aspects of conventionalist writings which bear on theoretical equivalence have something right about them, and something important as well, and I wish now to argue the contrary for the ontological aspect of conventionalism, at least as developed by Grünbaum.

So well as I understand him, Professor Grünbaum's view is this: A manifold, whether space or time or space-time or something else, has certain properties intrinsically. It would have these properties regardless of whether or not there were people, languages, automata, or anything other than the manifold itself. Such properties include the cardinality of pieces of space or of time or of space-time; topological properties such as connectedness or disconnectedness; in some cases relations of order or betweenness are intrinsic, but not always; further, causal relations, as between space-time events, are intrinsic[3] and so too may be certain qualitative properties, such as color or pitch. Other properties or relations are not intrinsic to a manifold, but depend on the existence of special objects within the manifold, e.g. people or rigid rods. The manifold could exist without these external objects and still be the same manifold, but without them it would have none of these extrinsic properties, only the intrinsic ones. A theory is conventional in a nontrivial way just if it concerns properties which are extrinsic not intrinsic; theories may, of course, also admit conventional alternatives in a trivial way, that is, when the two alternatives say the same thing in different words. We may sometimes take a variety of inconsistent views regarding the extension of a predicate signifying an

[3] I am not entirely sure about this; it may be that Grünbaum regards causal relations as intrinsic to space-time *with* matter but not to space-time alone. The explication of "intrinsic" which will be given below is flexible enough to accommodate such a notion.

extrinsic property, and we may do so for at least two reasons. First because the objects on which the extrinsic properties depend will often generate many alternative extrinsic properties, e.g. rigid rods in a continuous spatial manifold will generate a great many different metric relations on the manifold. Second, because if we ascribe an extrinsic property to a manifold, there is nothing in the manifold itself to bear witness against what we say.[4] Specifically, congruence is an extrinsic relation in a continuous, qualitatively homogeneous manifold but is intrinsic to a discrete, denumerable space, so the geometry of the former is conventional but not the geometry of the latter.

It should be emphasized that according to Grünbaum what is conventional are properties and relations themselves. The conventional status of sentences is entirely derivative. In fact, Grünbaum maintains that there is no necessary connection between the conventional status of properties and relations, on the one hand, and the existence of alternative theories on the other. For example, a discrete denumerable space has many different intrinsic metrics and hence there are many alternative geometries for the space, all equally good empirically, but the geometries are not conventional in any way, and that is just because the properties and relations they describe are intrinsic.

The essential thing, then, in evaluating Grünbaum's account of conventions in physics is to understand what an intrinsic relation might be, and how it is one can know which relations are, and which relations are not, intrinsic. Grünbaum's published remarks on this score have been rather obscure, and consequently there have been several attempts by others to explain what Grünbaum might mean by 'intrinsic'. Professor Massey, [13], has tried to explicate the notion of intrinsic properties of manifolds by those properties which are invariant under order preserving and order inverting mappings of a manifold onto itself. Professor van Frassen, [18], has suggested that the fundamental property Grünbaum regards as intrinsic is not a metric but a measure, and intrinsic measures are those which are suitably based on cardinality. Professor Grünbaum, in a recent mammoth article, has rejected these explications and tried to put the matter straight. His fundamental characterization of intrinsic and extrinsic properties is this:

> Since our concept of an intrinsic monadic property of an interval will rely on the notions of being a *non-external* and being a *general* property, we shall deal with these notions first. To begin with, I should emphasize that when I speak of a monadic or polyadic "property", I do so in the intensional sense of that term and *not* in the extensional sense of a class . . .
>
> (1) *Given* the elements of a manifold, we shall speak of an entity as being "*internal*" to an interval of the manifold (or as being an "inside" entity with respect to the interval), iff the existence of the interval depends on the existence of the entity. Thus every element belonging to an interval $[a, b]$ is internal to $[a, b]$ in this sense . . .
>
> (2) Now, in a given manifold, a monadic *property P* is said to be "*external*" to an interval passing it, iff the obtaining of *P* depends on entities which are *not internal* to the interval.
>
> (3) I shall take it to be antecedently understood what counts as a *general* property, and I explain this notion here only to the extent of pointing out the following: If a property is general, then its constitution does *not* involve particular individuals! Thus the cardinality

[4] Admittedly, these two reasons for the "usual" concomitance between extrinsic properties and alternative empirically equivalent theories involve an interpolation on Grünbaum's published remarks.

of an interval is a general monadic property of the interval. But the disjunctive property of being either singleton $\{a\}$ or singleton $\{b\}$ is *not* a general property of any singleton. . . .
In the preceeding three definitions I have invoked the *dependence* of (i) the existence of one entity on that of another and of (ii) the obtaining of a property on there being entities of a certain kind. I also appealed to the non-involvement of particular individuals in the constitution of a property. The cognate relations of dependence and involvement relevant here seem to be tantamount to the relation of logical implication (entailment) among propositions. . . .
(4) In a given manifold, a monadic property P is said to be "*intrinsic*" to an interval possessing it, iff P is a *general* property of the interval and *not external* to it. Thus, for an arbitrary interval of a continuous P[hysical]-space, the property of being 1 meter long is *not* an intrinsic monadic property. . . . ([9], pp. 525–26)

This characterization is certainly more explicit and detailed than accounts given by earlier conventionalists. It seems clearly unsatisfactory, however, to introduce a technical notion and compare it to a logical relation without detailing the comparison. So long as the account remains in such a state it is very difficult to focus the attack upon it. I shall, therefore, try to reformulate Grünbaum's account within an extensional model theory.

Let us imagine we have a formalized language which, according to context, we may take to be a first-order language, or a language for second-order logic, or even for type theory. A structure for a first-order language is a set, the domain D, with, for each n-place predicate letter of the language, a subset of the n-th Cartesian power of the domain. For a second-order language the structure is again a first-order relational structure, but we understand n-place predicate variables to range over all subsets of the n-th Cartesian power of the domain. I assume the usual semantics for such systems,[5] and I will say a finite sequence of entities in a structure satisfies an open formula if there is an assignment of variables which assigns entities in the sequence to the free variables of the formula and which satisfies the formula. I shall think of the world as determining a structure of the appropriate kind.

What sense can be given to Grünbaum's notion of the existence of one entity depending on the existence of another? He makes it clear that he does not have in mind a causal relation, e.g. the sort of dependence James Bond's existence might have on Dr. No's existence if there were a bomb strapped to Bond and fixed to explode in case No's heartbeat should cease. The only examples we have of what Grünbaum does mean are the more humdrum cases of intervals, manifolds, and their elements. For such cases it seems perfectly adequate to assimilate Grünbaum's notion of existential dependence to the notion of membership. The entities which are internal to an interval are just those entities which are members of the interval, considered as a set. We are explicitly warned against construing properties extensionally, but not against reducing the existential dependence relation to set relations. Whether or not this construal would fit what Grünbaum has in mind for other cases, it seems perfectly satisfactory for the geometric ones. What about the general properties? I have no reduction to propose, so instead I will just assume them given. In addition, I shall assume that another subset of the relations in a

[5] The semantics of unformalizable second-order logic is beautifully presented in chapter 15 of the forthcoming textbook by G. Boolos and R. Jeffrey.

structure is singled out, the *real* relations, by which I intend those relations which really are real. What is required altogether of a structure for our language, then, is that it be a first-order relational structure, possibly with a type hierarchy in addition, and that among the relations of any type a subset is designated as real.

Now it is possible to say what it is for a property (or relation) to be external to a set of entities. Remember that Grünbaum says a property is external just if its "obtaining . . . depends on entities which are not *internal* to the interval." So the natural idea is this: Given a subset S of the domain of a structure, the properties which are internal to the subset are just those properties which S has and which S would have even if everything else in the domain were to cease to exist. We shall make these ideas precise for the case of a relational structure for a first or second-order language:

Given a relational structure $M = \langle D, R_1{}^a, \ldots, R_m{}^1\rangle$ for a language L, we distinguish the set of all sets of ordered n-tuples on the domain; the set, R, of relations actually belonging to the structure, $R = \{R_1{}^a, \ldots, R_m{}^1\}$; and the set, W, of all sets of ordered n-tuples (relations) definable from R. We further suppose given, along with M, a non-empty set $R_e \subseteq R$ of extensions of real relations. If $S \subseteq D$, we define the real restriction of M to S to be the structure

$$M_S = \langle S, R_i{}^1 \cap S, \ldots, R_k{}^p \cap S^p\rangle$$

where S^j is the j-th Cartesian power of S and all and only relations in R_e occur among $R_i{}^1, \ldots, R_k{}^p$.

We must determine how properties are to be represented. A first-order property or relation is, in any structure, naturally associated with a set of n-tuples of elements of the domain; certain such sets, those which are the extensions of properties or relations which are expressible in the language, are naturally associated with open first-order formulas. We will reverse this chain of connections and associate with every first-order open formula, A, a property $P(A)$, and in any structure, M, the extension of the property will be taken to be the set of all n-tuples from the domain that satisfy A. An open formula containing either or both individual and predicate variables, free, is naturally associated with a property the extension of which in a given structure is the set of n-tuples satisfying the formula. In general any such n-tuple will have as members both elements of the domain and subsets of Cartesian powers of the domain. Finally, it is reasonable to associate properties with sentences of the language, as is sometimes done in English by means of such phrases as "the property that . . ." The extension of such a property is just the set of all structures in which the associated sentence is true.

We need some further definitions. By the *second-order closure* of an open formula, A, I shall mean the formula got by prefixing $\exists U$ to A for every predicate variable, U, which is free in A. The second-order closure of a formula containing no free predicate variables is that formula itself. A formula is *prime* if it contains a predicate constant and has none of the forms $\sim A$, A & B, $\exists U\ A(U)$, $\exists\ x\ A(x)$ or forms definable from these. A formula, A (or property, $P(A)$) is *reducible* in M if for every prime formula, C, in A, $P(C)$ is coextensive in M with some property $P(B)$, where B is a formula (not necessarily prime) all of the predicate constants of which designate real relations in M (i.e. members of R_e). By the *reduct* of a reducible formula, A, I mean the formula got by substituting in A, for every occurrence of any prime formula, C, the corresponding coextensive formula, B, containing only predicate constants designating members of R_e and having the same variables free as does C.

If A is an open formula, the property $P(A)$ is *internal* to a set $S \subseteq D$ in M if

i) A is reducible in M.
ii) The second-order closure of the reduct, B, of A is satisfied in M_S.
iii) Every k-tuple of elements of S satisfying the second-order closure of B in M also satisfies the second-order closure of B in M_S.
iv) Every n-tuple whose elements are members of S or subsets of Cartesian powers of S and which satisfies B in M also satisfies B in M_S.

If A is a sentence, the property $P(A)$ is *internal* to a set $S \subseteq D$ in M if

v) A is reducible in M.
vi) The reduct, B, of A in M is true in M_S.

I will simply assume it specified what properties are general: a property is intrinsic to S in M if it is internal to S in M and general. One way to see the motivation of the preceding account is to note that the operations of definitional expansion and of restriction on a relational structure are not commutative. If we start with a relational structure, M, expand it by adding all relations definable from those given in M, and then restrict the expansion to a proper subset, S, of the original domain, the result is a structure M^S_{ER} different from that, M^S_{RE}, obtained by first restricting M to S and then forming the definitional expansion on S. The n-tuples in S satisfying (in M) $\exists u\, P(u, w)$ will be a relation in M^S_{ER}, but that relation need not occur in M^S_{RE}; the set of n-tuples in S satisfying (in M_S) $\forall u\, P(u, w)$ may be a relation in M^S_{RE} which is not in M^S_{ER}. If all the relations in M are real, then M^S_{ER} is the world as it is, with our attention restricted to S. M^S_{RE} is the world as it would be if everything outside of S were to vanish.

The semantic characterization of 'intrinsic' seems to me in very good accord with Grünbaum's usage. What properties are intrinsic on our account depends on how the world is; just so, Grünbaum says that congruence is an intrinsic relation if space is discrete and denumerable but not if space is continuous and homogeneous. What properties are internal depends on what set is being talked about; just so, Grünbaum talks of properties which are internal to manifolds as a whole but not to the intervals of the manifold. Using the account of internal properties just given, we can see how it might be possible for congruence not to be an intrinsic property. Suppose, for example, that our domain consists of a family of rigid rods and a continuum of points. Suppose further that two pairs of points are congruent if and only if there is a rigid rod whose ends can be brought into coincidence with each pair. Suppose finally that the real relations and properties are those of being rigid and of being capable of being brought into coincidence. Then congruence is reducible in M since, using C for congruence, R for being a rigid rod, and C_o for being capable of being brought into coincidence:

$$C(x, y, u, v) \leftrightarrow \exists z\, [R(z) \,\&\, C_o(z, x, y) \,\&\, C_o(z, u, v)]$$

But congruence is not internal to the set of points S since its reduct is not satisfied in M_S.

Grünbaum appears to claim that cardinality properties are intrinsic. Using the preceding account, we can show that no matter what properties are real, the identity relation is internal to any set, and so are the properties of having exactly n elements for any finite n, of having an infinite number of elements, of being denumerable and of being nondenumerable. This seems a strong indication that the account captures the notion Grünbaum has tried to characterize. Identity is reducible in any structure since

1 $$x = y \leftrightarrow \forall U(U(x) \rightarrow U(y))$$

is valid. For any nonempty set $S \subseteq D$, $x = y$ is satisfied in M_S and trivially for any a in S, (a, a) satisfies $x = y$ in M only if it satisfies the same formula in M_S. The property of having exactly n elements might, for say $n = 2$, be expressed by

2 $$x \neq y \,\&\, \forall z(z = x \vee z = y)$$

or equally by the existential closure of this formula. The formula is reducible because identity is, and for any two element set $S \subseteq D$, the reduct of formula 2 is satisfied in M_S. If $S = D$, then every pair in S satisfying the reduct of 2 in M satisfies the reduct of 2 in M_S, and if $S \subseteq D$, there is no pair satisfying 2 in M. In either case the property is internal. We get the same result if we express the property by a sentence. The property of (Dedekind) infinity may be expressed by the sentence

3 $$\exists z \exists U[\forall x(z \neq U(x)) \,\&\, \forall x \forall y(x \neq y \rightarrow U(x) \neq U(y))]$$

where U is a function variable. In any structure this sentence is reducible since identity is. Moreover the sentence (and its reduct) is true in a structure if and only if there is a denumerably infinite subset of the domain.[6] Hence, for any infinite subset $S \subseteq D$, 3 is true in M_S and the property expressed by 3 is internal to S. Exactly the same conclusion is obtained if we express infinity by the open formula got by deleting the existential quantifiers in 3 and apply clauses i–iv of the definition of an internal property. The sentence

4 $$\exists z \exists U \forall V[(V(z) \,\&\, \forall z(V(x) \rightarrow V(U(x)))) \rightarrow \forall x V(x)]$$

where U is again a function variable, is true in a structure M if and only if the domain D is denumerable. Hence for any denumerable subset $S \subseteq D$, 4 is true in M_S, and since formula 4 is its own reduct, the property it expresses is internal to any such S. Again, the same result is obtained if we express denumerability by the formula got by deleting the two existential quantifiers in 4 and apply clauses i–iv. Undoubtedly, there are a number of other ways in which one might formalize the

[6] For a proof, see Boolos and Jeffrey, *op. cit.*, forthcoming.

idea which motivates our semantic account of internal properties, but it seems very unlikely the philosophical issues would be affected by whatever technical differences might arise. In the end, all that such an account can do is to give us some idea of what might be meant by "the obtaining of *P* depends on entities which are not internal to the interval" and related remarks; thereby, the account may at least give us some idea of what must be done in order to back up the claim that a property is, or is not, intrinsic. Whether or not the details are exactly as Grünbaum would wish, the semantic characterization seems a fair representation of his notions of intrinsic and extrinsic properties, and I think the problems with his view, so construed, are problems which would arise on any clear, reasonable construal of what he says.

The obvious question is how Grünbaum knows what he claims; how, in particular he knows that a continuous, homogeneous manifold has no intrinsic metric, that betweenness is intrinsic and that topological properties of manifolds are intrinsic. In a succinct criticism, William Demopoulos [2] has raised essentially these same questions. Grünbaum, in reply, introduces the notion of an "intrinsic description," to wit, a description or definition which correctly convinces someone that the property described is intrinsic. Whether and why the Unnamed *ought* to be so convinced by a description Grünbaum does not tell us for "... clearly the extrinsicality of a *description D* is a matter of individual mentality with respect to what *D* suggests to a particular person" ([9], p. 530). We may determine inductively that a property or relation is extrinsic by examining a variety of descriptions of that property or relation; if none of them are intrinsic descriptions, that is inductive grounds for concluding that the property or relation is not intrinsic. Grünbaum proceeds to give examples of several descriptions of metric relations on a continuous physical manifold, none of which convince him that these relations are intrinsic. He neglects to do the same for descriptions of topological properties or betweenness on physical spaces.

The objections to Grünbaum's argument seem obvious and insurmountable. First, if the extrinsicality of descriptions is an entirely subjective matter, a question solely of a person's response to a description, how can it possibly be used as a basis for a reasonable inductive inference to the conclusion that some property or relation is extrinsic? Second, even if Grünbaum were to provide reasonably clear and objective criteria for the extrinsicality of a description, his inductive inference would still very likely be unsound. We think that if all the ravens we have seen are black, that may be inductive evidence for the conclusion that all ravens are black. For if all ravens not sampled are like those sampled in respect of color then it follows, necessarily, that all ravens are black. We put some stock in our conclusion only because we have sampled from the very collection about which our conclusion makes some assertion. Grünbaum does not have us sample instances of a property but rather descriptions of it. Unless the characterization of "extrinsic description" is such that all descriptions of a property can be extrinsic only if the property is, or very probably is, extrinsic, the extrinsic character of the descriptions has no inductive bearing on the extrinsic character of the property. The word is not the thing.

Despite these objections, I think the semantic characterization suggests how it might sometimes be possible to determine inductively that a property or relation is intrinsic, or is extrinsic, and a crucial part of such a determination may involve the logical form of sentences which "describe" the property. For if we know which properties are real and which general, then we can determine, inductively at least, whether or not a given property is intrinsic to a given set. That a property $P(F)$ is reducible in the actual world to a property $P(H)$ we may suppose determinable inductively; if we can find no suitable property to which $P(F)$ is reducible, that is evidence that it is not intrinsic to anything. Given that $P(F)$ is reducible to $P(H)$, that is grounds for thinking $P(F)$ internal to a set S only if every n-tuple in S which satisfies H in the real world, M, also satisfies H in M_S. The latter question, in turn, can be answered provided we know the extensions of the real relations designated by the prime formulas of H and the quantifier structure of H. If we can find no $P(H)$ to which $P(F)$ is reducible and such that H meets the satisfaction criterion, that is grounds for thinking that $P(F)$ is not internal to S. The central question, therefore, is how Grünbaum can know which relations are real. While he nowhere introduces explicitly a notion corresponding to what I have called real relations, he does write of some relations providing an "intrinsic basis" for others,[7] and it seems evident that some notion analogous to what we have called 'real relations' is required. A congruence relation among the point pairs of a continuous manifold is, ideally, definable from the relations of these point pairs to undistorted rigid rods; we can talk about congruence as a relation among point pairs, or we can talk about the relation of rigid rods to point pairs, a relation which, ideally, permits us to define a relation which is extensionally the same as that of congruence. The heart of Grünbaum's thesis, it seems, is that these relations are not ontologically on all fours. Without the rigid rods the congruence relation would not exist. In my terms, this amounts to the claim that congruence in such a manifold is reducible but not real and not internal to the manifold. If congruence is not internal, then it cannot be real in the required sense.

How, then might it be determined which relations have this ontological primacy? Grünbaum's interesting claims are about the status of properties which clearly are not reducible to identity: betweenness is intrinsic, he says, and so are the topological properties of a manifold, but congruence is not intrinsic to a continuous space. In terms of the explication we have given, betweenness and topological properties are not reducible to identity, so if they are intrinsic they must either be themselves real or reducible to real properties. Now Grünbaum treats metric properties in a denumerable discrete space as reducible to betweenness and cardinality properties, but he nowhere suggests that betweenness or topological properties are in turn reducible to some ontologically prior properties. So, how does he know that these properties are real but that congruence, under the conditions mentioned, is not real? One might hold the view that the real properties are those assumed by our best physical theories, but that is clearly not Grünbaum's opinion. Or, one might argue that the real properties are those with some special epistemic character; but it is not clear what epistemic character it would be, nor why what there is should

[7] [7], passim.

depend in any way on how we know it. At the crucial point, we draw a complete blank from Grünbaum.

Grünbaum does offer an indirect argument for his theory. His account of conventions, he says, "illuminates and draws support from" the general theory of relativity. Such a claim is difficult to adjudicate but it cannot simply be dismissed. A philosophical thesis which is otherwise clothed only in skimpy argument may in fact be made more plausible if, using it, we are able to give a coherent account of some initially obscure or puzzling subject matter. I think it might fairly be said, for example, that verifiability theories of meaning have drawn support from the quantum mechanics, and, again, that the thesis that logic is empirical has led to the illumination of the quantum theory. But, I doubt that Grünbaum's remarks on general relativity do in fact afford much illumination or establish much support for his account of conventions. Indeed, I think rather the opposite is true.

In general relativity the metric of space-time and the metric of space depend on how matter and radiation are distributed in space-time. In Grünbaum's terms, the space metric depends on something external to the spatial manifold and that, Grünbaum would say, is because the space of general relativity is continuous, not discrete. In reply to Hilary Putnam, Grünbaum writes as follows:

> With a suitable change in the matter distribution and hence in the metrical field, meter sticks will coincide with different disjoint intervals under transport in the same region of space of a given reference system. But since there is no intrinsic metric the stick can be considered self-congruent under transport in the given region in each of infinitely many different patterns of coincidence behaviour under transport. And hence one can justifiably regard each one of these alternative patterns as generating the metric geometry prevailing at the time. . . . Thus with a suitable resulting change in the metric tensor of space, the same sticks will alike confer a different metric geometry than before upon the same region of physical space. And this is clearly a case of alternatively metrizing the same spatial manifold, the alternative metrizations being physically realized by alternative coincidence patterns of meter sticks at different times! ([7], p. 208)

John Earman, [3], has remarked that the different spatial metrics referred to in this passage are in no way alternative, for at any time in any cosmological model one and only one such spatial metric would be allowed on a given space-like hypersurface; it is not as though we had a choice as to the metric of space in such cases, so how can such nonstatic cosmologies exemplify the conventionality of spatial metrics? Grünbaum's reply, in effect, is that when he speaks of metrics as "conventional" he is not talking about the epistemic status of theories embodying such metrics but rather about the ontological status of the metric properties themselves. In the case of nonstatic cosmologies, the spatial metric cannot be intrinsic because it changes with time:

> We see that in the GTR, no less than in Newton's physics, we can specify the points of a three-dimensional *P*[hysical]-space by a suitable system of non-intersecting time-like world lines, each of these world-lines being taken in its

> entirety in the appropriate region of space-time. Thus, as stressed by Synge, an infinite time-like (equivalence) class of event elements of space-time . . . corresponds to a single element of the P_3 in question. And the very nature of the correspondence which generates a P_3 from space-time shows the following: Intrinsically, the points of any P_3 space and the P_3 manifold constituted by them are time-independent entities! In other words, with respect to all of its intrinsic properties, any existing P_3 manifold, qua manifold of spatial points, is a timeless entity. ([9], p. 557, italics omitted.)
>
> Indeed the field equations of the GTR . . . allow solutions in which . . . no space interval AB will ever have the same length $d\sigma$ at any two different times. . . . But no S-intrinsic, let alone I(nterval)-intrinsic, reflexive, symmetric and transitive dyadic property of space intervals could constitute the basis of this kind of time-dependent $d\sigma$ equality. For we saw that any and all of the intrinsic properties of a spatial manifold P_3 and of any of its intervals obtain timelessly! ([9], p. 559, italics omitted.)

Part of what I object to in this passage is the claim that ". . . the very nature of the correspondence which generates a P_3 from spacetime shows the following: Intrinsically, the points of any P_3 space and the P_3 manifold constituted by them are time-independent entities!" I do not see that anything of the sort has been shown, nor do I see how it could be. Certainly, in the sense of 'intrinsic' developed above, "the very nature of the correspondence" does not guarantee that the intrinsic properties cannot change with time.

Moreover, there is at least some reason to think that if the intrinsic properties of space and of spatial intervals in general relativity cannot change with time, then certain cosmological models contradict Grünbaum's views. For Grünbaum has maintained that the topological properties of space are intrinsic, and one supposes that the only reasonable topology to ascribe to space at a time is the topology of the constant time hypersurface at that time. But this topology *does* change with time in many cosmological models: most simply, in the model got by removing from Minkowski space all space-time points x = constant, y = constant, z = constant, $t > t^*$. Many more complicated and more interesting examples occur as well. For example, in the Elliptic De Sitter space-time there is no nonintersecting 3-parameter time-like congruence because the space-time is not isochronous,[7a] but there is a self-intersecting system of world-lines, and the constant-time hypersurfaces orthogonal to it are the natural representatives of space-at-a-time in this model. These hypersurfaces have, all but one of them, the topology of a 3-dimensional sphere; the odd hypersurface is elliptic. Of course, there are many things that Grünbaum might say to examples like these: he might say that the examples are not physically possible, or that in such models the intrinsic properties of space are not time-independent, or even that topology is sometimes not intrinsic. Indeed, it is partly because there are so many things he *might* say to examples such as these that

[7a] (Added in proof.) The text above is incorrect. Elliptic De Sitter space-time is diffeomorphic to the bundle space of a fibre bundle over 3-dimensional projective space, with fibres homeomorphic to the real line; the fibres form a system of non-intersecting time-like world lines.

I am unwilling to count Grünbaum's remarks on nonstatic Robertson-Walker metrics as any kind of illumination of general relativity. Insofar as there is a clear notion of intrinsic properties, there is no argument or evidence that the spatial metrics induced in "nice" cosmologies are not intrinsic; insofar as there is an argument at all, it appears to get its conclusion *ad hoc* by means of an obscure notion of "intrinsic" properties.

There is no question that, just as Grünbaum would require, the metric properties of space-time and of space at a time are, according to general relativity, dependent on the distribution of stress and energy in space-time. Presumably it was this sort of agreement which Grünbaum had in mind when he claimed that his account draws support from the general theory. I do not think this concordance warrants any such conclusion; and that is because I think the agreement between Grünbaum and Einstein is an entirely superficial covering for a more substantial disagreement. The interdependence between matter and metric postulated by general relativity is not at all of the *logical* kind Grünbaum requires. In general relativity changes in the distribution of stress and energy can influence the space-time metric and thus any spatial metrics, but the existence of mass and energy are *not* necessary for the metric of space to change with time. De Sitter's cosmological model is empty of matter and radiation, but admits a spatial metric which changes with time. Since, on any sensible reading of Grünbaum, the properties of an empty spatial manifold, whatever they may be, are internal to the manifold, and since metric properties are presumably general properties, general relativity is in flat contradiction with the second paragraph quoted above.

John Earman, [3], has pointed out that in denying that a continuous space empty of all material bodies could have a metric, Grünbaum's view contradicts general relativity, since that theory includes the possibility of empty space-times having a space-time metric and, derivatively, having spatial metrics as well. Earman's point seems to me an important objection to Grünbaum's theory. The latter's reply is that either talk of empty space-times is no more than a *façon de parler* in general relativity or else general relativity is inconsistent. Grünbaum does not detail what talk of empty space-times might really mean if it is no more than a metaphor, and I think he is right not to do so. I think that any survey of the scientific literature would show that, up to the operationalism endemic to their ilk, relativity physicists are very often entirely serious in their talk of the metric of empty space-times. Weak-field or linearized field theory is typically discussed in terms of perturbations on the Minkowski metric; again, the boundary conditions often used in obtaining the Schwarzschild solution are that the metric become asymptotically Minkowski at large distances from the source. Both of these requirements, it seems clear, are warranted only by the belief that the Minkowski metric is the metric which space-time would have were it empty.

On balance, Grünbaum seems obliged to illuminate general relativity by demonstrating that it contains an implicit contradiction. Certainly, if the general theory is tacitly self-contradictory in ascribing metrics to empty space-times, demonstration of the contradiction would be a genuine illumination. While it is not an obligation to be envied, and we would certainly require a very convincing case, attempts at such demonstrations are not unprecedented. Kochen and Specker's, [10], argument for the thesis that the quantum theory is inconsistent with the assumption that all observables have, at all times, arbitrarily precise values suggests itself as a sort of precedent. I do not think, however, that Grünbaum's several arguments on this score are or should be very convincing, nor do I think the insight, if any, they afford into general relativity compares at all favourably with the insight into quantum theory provided us of late by philosophically inclined mathematicians.

Grünbaum's first argument is as follows:

> Let us suppose with Earman that the so-called empty space-times involved here (i.e. those for which the energy-momentum tensor of the field equations is zero) are to be thought of as literally devoid of any of the "test particles" or infinitesimal metrical "test" standards (light clocks, atomic clocks, rods) of which relativity physicists are wont to speak. Then if he wishes to deny me the philosophical invocation of these test bodies as metric standards external to the space-time manifold, it would follow that the GTR contradicts itself by containing the following statement: A free particle of non-zero rest mass and also a photon (which has equivalent mass) has a geodesic path in Minkowskian space time.
>
> For if cognizance were taken by the theory of the permanent gravitational fields associated with these test bodies, their very presence would have to be held to destroy the Minkowskian character of the space-time by issuing in a nonvanishing four-dimensional Riemann tensor. And in that case, special relativity would cease to qualify as a theory of the behavior of light rays or of any other known physical agencies! ([9], p. 566, italics omitted)

What about the claim that "special relativity would cease to qualify as a theory of the behavior of light rays. . . ."? The special and general theories are different theories; in fact, they are frequently taken to be inconsistent and if so there can be no question of the general theory allowing the special theory as a literally correct account of things. This does not mean that if the general theory is accepted "special relativity would cease to qualify as a theory of the behavior of light rays," only that the special theory would cease to qualify as a possibly *true* theory of the behavior of anything. There is nothing shocking or novel in this inconsistency between two closely related theories; special relativity, in turn, contradicts Newtonian theory, statistical mechanics contradicts classical thermodynamics, and so on.[8]

Indeed, Grünbaum himself does not seem to think this a very serious objection for he writes that "Even if these considerations are rendered less telling by the pragmatic appeal to an *approximately* Minkowskian space-time, there is the serious problem of individuating the elements of an empty space-time. . . ." The latter problem forms Grünbaum's second argument against the possibility of empty space-times:

> If there are no extra-geochronometric physical entities to specify (individuate) the homogeneous elements of space-time and/or of P_3-space, then whence do

[8] This point has been frequently made by P. Feyerabend and others.

> these elements of otherwise equivalent punctual constitution derive their individual identities? Must the world points not be individuated before the space-time manifold can even be meaningfully said to have a metric? I see no answer to this question as to the principle of individuation here within the framework of the ontology of the Liebnizian identity of indiscernibles. Nor do I know of any other ontology which provides an intelligible answer to this particular problem of individuating avowedly homogeneous individuals. ([9], p. 567, italics omitted)

While I can find no reasonable construal which makes this argument a good one, with considerable labor one can construct a reasonably clear argument which seems to parallel Grünbaum's, is likely valid, and leads to his conclusions. It might go like this:

> Two objects are identical unless there is an ascertainable property or relation of one which is not a property or relation of the other. To be slightly more precise
>
> $$a \neq b \Rightarrow \exists R \exists x_1 \ldots \exists x_n [R(a, x_1 \ldots x_n) \ \& \ \sim R(b, x_1 \ldots x_n) \ \& \ R \text{ is ascertainable}]$$
>
> In an empty space-time, the homogeneous elements would all have exactly the same *monadic* properties. Since there are no physical objects in the universe, the only relations which might serve to individuate two elements of the manifold are their differing metric or topological relations to other elements of the space-time. But if the topology is nice (e.g. Euclidean) then for every topological relation $R(x, y \ldots z)$ with x variable over the elements of the manifold, $y, \ldots, z$ constants, if one value of x satisfies the formula then so will every value of x. But metric relations in a continuous homogeneous manifold are not determinable without the use of external objects. Hence no empty space-time can exist.

The obvious and overwhelming difficulty with the argument is not that to understand it we must imagine disembodied minds who can know and ascertain and discern certain relations but not others, although that is difficulty enough. More important, I think, is the fact that we have no good reasons to believe the first premise of the argument, namely that what are putatively two objects are really the same object unless there is a discernible property or relation of one which is not a property or relation of the other. There is nothing obviously incoherent in the notion that two objects might differ in some property or relation which we cannot discern; indeed, the claim seems almost obvious when we consider the resources which might be available to us (or to us disembodied) in an otherwise empty (or completely empty) universe. In fact, there seem actually to be objects, namely bosons, which while different still differ in no property at all (save identity) or at least in no discernible property. We may point out, in addition, that the argument seems as good (or as bad) when directed against the possibility of space-times which are perfectly homogeneous as when directed against empty space-times. Since a great deal of relativistic cosmology assumes, for simplicity, a uniform distribution of matter, the argument would maintain that this enterprise is not just wrong, but necessarily wrong and unintelligible.

The argument I have given may not be Grünbaum's, but any other version which comes to mind and which is clear enough that one can form some idea of its validity seems equally shaky. If we replace the version of the identity of indiscernibles which is used above with, say, a version which does not require that two objects differ in some discernible property or relation, then the argument is no longer valid, since we may then regard the entities of the manifold as individuated by their metric relations. If metric relations are specifically excluded from the class of individuating relations then the argument again appears valid, but we must ask for the grounds of this exclusion. If the grounds are that such relations are not ascertainable without external objects, then we are back to the argument given above; if the grounds are that metric relations do not exist in the absence of material entities, then the argument is circular when viewed, as it should be, as part of a counterargument to Earman's criticism. When not viewed in this context, the introduction into the argument of the claim that metric relations do not exist in the absence of material entities does not make the argument circular; it does, however, introduce an entirely unfounded premise, one for which Professor Grünbaum's "induction" on descriptions is no support.

There is one final issue about general relativity which, because of its bearing on contemporary physics, deserves discussion. Grünbaum charges the advocates of geometrodynamics, Clifford and Wheeler in particular, with inconsistency. Since these men have maintained that matter reduces to curved space, they must also have thought, according to Grünbaum, that curvature is an intrinsic property of space. Yet, Grünbaum argues, both Clifford and Wheeler deny that space has an intrinsic metric. But this is inconsistent, Grünbaum concludes, since "... this curvature would need to obtain with respect to a metric implicit in empty space" ([9], p. 523). Now I do not think this claim especially important, partly because I am not at all convinced that Wheeler would deny that space has intrinsic metric properties, and partly because the program of geometrodynamics certainly does not require such a denial. Even so, I doubt that Grünbaum has provided, or can provide, anything like sufficient grounds for his conclusion. He gives no argument at all as to why we should think curvature properties presuppose or require or "would need to obtain with respect to" a metric. It cannot be because the curvature tensor of space does in fact determine a unique Riemannian metric, for that is not true, as Grünbaum himself appears to have noted ([8], pp. 89–105). Perhaps by "curvature" Grünbaum intends properties some of which are not determined by the curvature tensor alone; affine properties generally, perhaps, or sectional curvatures. But a 3-dimensional manifold fitted with a Riemannian connection does not, in general, have a unique compatible metric, even up to similarity. The strongest result known to me ([13], section 13) for dimensions greater than two is that if the holonomy group is everywhere transitive in a manifold then there is, up to similarity, a unique compatible Riemannian metric. Recent work by Kulkarni, [11], has shown that for dimensions greater than three, two Riemannian metrics on a differentiable manifold can agree on all sectional curvatures at every point only if they are locally isometric. Whether the same result holds in three-dimensions is an open question and positive results are available only for rather special cases. Even if the properties in question include all affine properties *and* sectional curvatures it is not clear that they determine a unique metric. Again, the strongest result I know is that if g and h are Riemannian (or pseudo-Riemannian) metrics on a manifold and have the same geodesics and sectional curvatures at all points and $g = h$ at some point, then $g = h$ everywhere. (See Wolf, [20], p. 59). So interpreted, then, Grünbaum's contention that curvature requires metric is at best moot. Of course, Grünbaum may simply have meant that curvature properties are just not the sort of thing that can exist unsupported. But he has given us no shade of reason why that might be so, let alone demonstrated that what is required for their support is a metric.

In recent critical review, Arthur Fine ([4], p. 479) has sought to place Grünbaum's views within a long tradition of relational theories of space and has concluded that "... Grünbaum does offer the outline of a possibly successful relational view ... Even a confirmed absolutist will appreciate that Grünbaum's work provides a solid philosophical stone against which his own axe can be ground and, no doubt, sharpened." My own conclusions are not so sanguine. I believe Grünbaum has extended the ontological views of earlier conventionalists, and at the same time has abandoned the operationalist and verifiabilist theories of meaning which were their chief support. The result is a still rather opaque view of how things are, one for which I find not a single plausible argument. There is certainly nothing objectionable in offering an ontology, even one for which no good arguments are available; one can only regret that the views in question have not been given a more precise articulation. But Grünbaum's attempts to apply his ontological convictions to illuminate and criticize contemporary space-time physics seem to me to rely on very poor arguments almost exclusively, and to make a deal of use of the inchoate state in which he has left his notion of intrinsic properties. Not that Grünbaum's conclusions are always wrong; Geroch, [5], has, for example, given good (if not, perhaps sufficient) reasons for thinking geometrodynamics incoherent, but they are not Grünbaum's sort of reasons. We are, I think more likely to learn something important to physics and to philosophy if, rather than leaping into the ontological quagmire inherited from earlier conventionalists, we turn our attention to the other, epistemic, aspect of Reichenbach's thought.

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