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THEORETICAL REALISM AND THEORETICAL EQUIVALENCE

> The true correlate of sensibility is not known and cannot be known...
> – KANT

A great many philosophers have thought it impossible that there should exist two distinct theories between which no possible evidence could discriminate. This doctrine, and more rarely its denial, have often been the cynosure of dogmas and disputes about conventions within physical theory, about simplicity, about measurement in quantum mechanics, and most recently, about radical translation. The list is long enough to make it important to know whether or not the doctrine is true. I shall argue that it is, indeed, not true.

The thesis that empirically equivalent theories are synonymous was central to Hans Reichenbach's philosophy of science, and especially to his notion of 'equivalent descriptions' and to his account of simplicity:

> There are cases in which the simplicity of a theory is nothing but a matter of taste or of economy. There are cases in which the theories compared are logically equivalent, i.e., correspond in all observable facts.... For this kind of simplicity which concerns only the description and not the facts co-ordinated to the description, I have proposed the name *descriptive simplicity*. It plays a great role in modern physics in all those places where a choice between definitions is open to us. This is the case in many of Einstein's theorems.... Thus the choice of a system of reference which is to be called the *system in rest* is a matter of descriptive simplicity. It is one of the results of Einstein's ideas that we have to speak here of descriptive simplicity, that there is no difference of truth-character such as Copernicus believed. The question of the definition of simultaneity or of the choice of Euclidean or non-Euclidean geometry are also of this type.[1]

Reichenbach's views on theoretical equivalence have been retained by Professor Putnam, who has suggested that empirically equivalent theories are 'thoroughly intertranslatable',[2] and even more emphatically by Professor Salmon.[3] Adolf Grünbaum,[4] a philosopher who has written a great deal about empirically equivalent geometrics, quotes with approval from the passage of *Experience and Prediction* given above, and claims the correctness of Reichenbach's doctrine of 'equivalent descriptions', which is no other thesis than that empirically equivalent theories always do say

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the same thing.[5] The view against which I shall argue is, therefore, frequent if not common, and even its few critics have been apostates.[6]

The argument is best begun by giving its point and, in outline, the whole of it. The view I deny is the conjunction of the following theses:

1. The sentences of scientific theories are either true or false; they are not simply 'instruments' for inferring observation statements.

2. If two theories are empirically equivalent, i.e., if any possible piece of evidence for one is likewise evidence for the other, and any possible piece of evidence against one is likewise evidence against the other, then the two theories say the same thing.

There seem to be only two initially plausible ways of defending the conjunction of these claims, and both defenses, I shall argue, must be rejected:

A. The set of all sentences which are statements of possible evidence for a theory form the truth conditions for that theory. Which is to say that a theory is true if and only if all of the observation statements which would be evidence for it are true.

B. The criteria for what is required of a body of sentences in order for it to count as a *theory* are in fact so strong that it is not possible for there to exist two different, empirically equivalent theories.

These are very different lines of defense; the first proposes a novel semantics for the language of science, the second presupposes the usual semantics and, based upon it, a criterion of synonymy for collections of sentences. Against A I shall argue that it makes true what is false and by making logic – in a traditional sense of 'logic' – impossible also makes important aspects of scientific reasoning unintelligible. Against B I shall offer a very weak necessary condition for the synonymy of theories in formalized languages and using it show by counter-examples that reasonable criteria for what is to count as a scientific theory do not suffice to eliminate the possibility of distinct but empirically equivalent theories. Finally I shall mention an example of two geometries which can be interpreted so as to be empirically equivalent but which, I contend, cannot be understood to say the same thing.

What is and what is not a possible piece of evidence for a theory is intolerably fuzzy. The way to keep it clear for our analysis is by dogma rather than by argument. Assume that our scientific theories can be formalized in a first-order language, and that all of the evidence given us by our senses and by our apparatus can be stated using only part of the non-logical vocabulary of the language. It is, for our purposes, of no consequence how this distinction between 'observation terms' and the rest is drawn and justified. Two theories will be empirically equivalent just when every observation statement which is evidence for or against one is, accordingly, evidence for or against the other. We shall assume this is to be the case whenever two theories have the same set of observational consequences, and grasp a palsied justification for this assumption in our ignorance of any organon which would discriminate such theories.

We thus have a crude but usable representation of empirical equivalence. The language of science is a first-order formalized language divided in two by the distinction between observational predicates and the rest. A theory is, at least, a deductively closed collection of sentences in this language, and two theories are empirically equivalent just if they have the same observational consequences. Evidently, this description of empirical equivalence could be given – although not justified – purely syntactically, that is, without using notions of truth, meaning or reference. It is trivial that there can be different deductively closed collections of sentences which contain the same observation sentences, but our obligation is to consider arguments which purport to show that it is nonetheless not true that there are empirically equivalent theories which say different things. Argument A does so by proposing that any two sentences which have the same observational consequences are synonymous, and Argument B reaches the same conclusion by denying that most deductively closed collections of sentences are theories.

The usual semantics for a first-order language gives conditions for the truth of a sentence in a structure for the language, and necessary and sufficient conditions for two sentences of the language to say the same thing. A structure for a language is simply a non-empty set, and for every n-place predicate of the language an n-ary relation on the set. I shall assume your familiarity with what it means for a sentence of the language to be true in a structure, and remind you that two sentences are semantically equivalent just if they are true in exactly the same structures. The semantics 'fits' the logical syntax of the language in a well-known way: a sentence, A, is provable from a sentence, B, just if A is true in every structure in which B is true. It is evident that with the usual semantics there can be empirically equivalent sentences which do not say the same thing.

Reichenbach's remark that empirically equivalent theories are 'logically equivalent' suggests a radical rejection of the usual semantic analysis. It suggests, rather, that we should accept the usual account of truth in a structure only for observation sentences. But, more generally, a sentence will be true in a structure if and only if all of the observation sentences provable from it are true in that structure. Any two sentences, or collections of sentences, from which exactly the same observations sentences are provable will then have the same truth conditions; empirically equivalent theories will be synonymous. This seems a fair account of the tacit semantics in both Reichenbach's and Salmon's accounts of theoretical equivalence, and perhaps also of the semantical views of those philosophers who have been greatly taken with the reaxiomatization theorem of William Craig.

The truth conditions just sketched guarantee the truth of any claim which has no observational consequences other than observational tautologies. This result seems clearly unsatisfactory since there are theories, such as Newton's laws, which we think false but which by themselves have no testable consequences. And there are further difficulties. First order proof theory is a representation of the principles of mathematical reasoning and therefore also of the principles of an important part of scientific reasoning. It is, if you like, an idealized theory of the inference behavior of mathematicians. The importance of the proof theory is evidenced by the natural and enlightening ways in which mathematical theories have been given first-order formalizations. The usual semantics for first-order logic is both an explanation and a partial justification of those principles of inference. If one accepts A and sentence B is provable from A then we accept B, and there is good reason. For if A is true then B is true. Conversely, if B is certainly true when A is true then there is a proof of B from A. This important function of a formal semantics is lost if one takes seriously the new truth-conditions just outlined. The new semantics provides an interpretation of classical logic which is not complete: any formula having only observational tautlogies as consequences is valid but not necessarily provable. Nor is the proposed semantics sound with respect to arguments. Every sentence is provable from a sentence A

and its negation but it is not necessarily the case that every sentence is semantically entailed by such a pair.

Perhaps such considerations only show that classical proof theory is no more tenable than classical semantics. A new account of truth, it might be thought, deserves and requires a new account of proof. Aside from the unlikelihood that a theory of proof agreeing with the proposed semantics would also agree with mathematical inference, there is an important difficulty with such a suggestion. Traditionally, the notion of proof has been required to be effective: there must be a recursive procedure for deciding what strings of formulae are and are not proofs. But the new semantics is not adequate for any proof theory employing an effective notion of proof. A demonstration is given in the appendix.

I shall not pursue argument A any further, for its handicaps seem insuperable. The development of argument B requires conditions for theoretical equivalence which are so framed as to accord with first-order syntax and semantics. For theories expressed within the same language we already have a sufficient condition for theoretical equivalence, namely logical equivalence. But we have heard too much of 'meaning variance' and the like to be comfortable in the assumption that different theories can reasonably be represented in the same language. For generality, then, whenever I mention a pair of theories I will assume their theoretical vocabularies to be disjoint.

The natural necessary condition for the equivalence of two theories framed in different languages is that they be intertranslatable. What that means, at least, is that the two theories have a common definitional extension. For the equivalence of theories T and Q we require a set of definitions D_T of the predicates of Q in terms of open formulas of the language of T, and similarly a set D_Q of definitions of the predicates of T in terms of open formulas of the language of Q. Moreover, the theories and definitions must be such that when D_T is added to T and D_Q to Q the two resultant theories are logically equivalent. Such a criterion guarantees that all and only theorems of T are translated as theorems of Q, and conversely; if T and Q can be axiomatized as single sentences, it is also guaranteed that the translation (into the language of T) of the translation (into the language of Q) of T is logically equivalent to T. Stronger necessary conditions – for example that the composition of translations from the language of T to that of Q and back to T should take every formula into a logically equivalent formula – might conceivably be required, but weaker conditions do not seem likely.

Nonetheless, it is clearly to the advantage of those who like argument B to weaken the necessary conditions for theoretical equivalence; and an argument can be made for weakening the condition just given. Informal number theory is about a specific structure, the natural numbers. But formal number theory has a great many non-standard models; a plethora of statements about the natural numbers which are not provable in formalized number theory are truths of number theory nonetheless. In short, there is more to number theory than its axioms and their models; there is, in addition, an intended model. The same might be said for formalizations of empirical theories; they too may have intended models which are not uniquely characterized by their axioms. But should not two theories with the same intended model be thought equivalent even though they are not intertranslatable?

I grant the objection although I am not sure that I agree with it. Let us take it as sufficient for the equivalence of two theories that they have the same intended model; but to have the same intended model two theories must have at least one model in common, namely the intended one. We must still say what it is for theories in different languages to have a mode in common since the usual notion of a model is relative to a language. Again, however, there is a natural account available. We shall say that two theories, T and Q, have a model in common just if there are models M_T and M_Q of T and Q respectively such that the set D^T of all sentences in the language of T which are true in M_T and the set D^Q of all sentences in the language of Q which are true in M_Q are intertranslatable in the sense given previously. In brief, two theories have a model in common just if they have models whose diagrams have a common definitional extension.[7]

The justification for this account of what it is to have a model in common is straightforward. It follows from the definition that two theories, T and Q, have a model in common just if they have complete consistent extensions which in turn have a common definitional extension, call it TQ. Since TQ is consistent it has a model M, intuitively one of the models which T and Q have in common. The model M is a set with relations corresponding to the predicates in the language of T and relations corresponding to the predicates in the language of Q. The structure M_T, got by dropping the relations corresponding to predicates of Q, is a model of T; similarly, the structure M_Q is a model of Q. But M can be regenerated from either M_T or M_Q by defining the relations which were dropped in terms of the relations which remain.

We have a clear and mathematically useful necessary condition for theoretical equivalence; it is, moreover, a condition which does not beg important questions about the comparability of terms in different theories. Using this condition we have to examine the thesis of argument B, namely that the restrictions on what a collection of sentences must be to be a theory are so strong that all empirically equivalent theories are also equivalent. Since it is trivially possible to give examples of empirically equivalent collections of sentences having no model in common, the claim of B needs arguing. In order for a restriction on what theories are and are not to lend any support to the claim, the restriction must have formal implications. Unless they are of a patently trivial kind, historical, sociological, or psychological restrictions will, of themselves, not limit the variety of structurally different theories which it is possible to construct; neither will material restrictions on the interpretation of the theoretical predicates be of help. It will not advance the case to require, for example, that theories refer only to entities that have a spatio-temporal location, or that for a claim to be a theory it must be possible for someone to believe it, and so on. For such demands, justifiable or not, place no evident restriction on the structure of theories, and if it is to be shown that empirically equivalent theories always have a model in common then some structural restrictions are necessary.

Philosophical papers contain very few formal criteria for a theory, and it is clearly a hopeless task for me to attempt to invent and to study all of the criteria which could conceivably be proposed. What I shall do is to study a criterion which has been explicitly proposed by William Kneale,[8] and tacitly used by a great many others. It is this:

A theory must be finitely axiomatizable, but its collection of observational consequences must not be finitely axiomatizable.

It is not because I think this principle is true that I shall examine it, for it is evidently not true. There are perfectly good theories which appear to have no observational consequences, and there are other, equally good, theories which are not finitely axiomatizable. But the proposed restriction on theories is worth investigating just because, besides being simple, it is too strong in important respects.

The requirement does not suffice to eliminate the possibility of empirically equivalent theories having no models in common. Indeed we have the following result:

If T is any theory in a first-order predicate language (with identity) and every complete, consistent extension of T is decidable, and the collection of observational consequences of T has, up to isomorphism, at most a finite number of finite models, then there is a finitely axiomatizable theory Q which is observationally equivalent to T but has no models in common with T.

A proof of this claim is immediate from work of Kleene, Craig and Vaught and is given in the appendix. Their proofs show, in effect, how to construct such a theory, Q, given the observational consequences of theory T. The theories they construct meet Kneale's criterion whenever the theory T does, but they are perverse theories nonetheless. In effect, the theories which Kleene, Craig and Vaught construct are truth theories for the observation language which say, in addition, that a specified set of observation statements are true. Although Kneale's criterion does not eliminate such systems as theories, it seems entirely reasonable to do so, for they evidently explain nothing. Just what we prohibit by refusing to to regard truth theories as theories is not entirely clear. To show that the requirements still do not eliminate the possibility of empirically equivalent theories, we must, therefore, settle for giving examples where the theories involved are clearly not truth theories. Such examples can easily be given.

Suppose the relevant evidence can all be stated with the identity predicate, and suppose, when all the evidence is in, what it says is that there are an infinite number of objects. Here are two explanations of the evidence which meet all of the required conditions.

T1

$$\forall x \sim L(x, x)$$
$$\forall x, y, z\big((L(x, y) \,\&\, L(y, z)) \rightarrow (x, z)\big)$$
$$\forall x, y\big(L(x, y) \vee (x = y) \vee L(y, x)\big)$$
$$\forall x, y \exists z\big(L(x, y) \rightarrow (L(x, z) \,\&\, L(y, z))\big)$$
$$\forall y \exists x L(x, y)$$
$$\forall y \exists x L(y, x)$$

T2 $$\forall x \sim B(x, x)$$
$$\forall x \exists y B(x, y)$$
$$\exists x \forall y (\sim B(y, x) \,\&\, (\forall z \forall s (\sim B(s, z)) \rightarrow z = x))$$
$$\forall x, y, z ((B(x, y) \,\&\, B(x, z)) \rightarrow z = y)$$
$$\forall x, y, z ((B(x, z) \,\&\, B(y, z)) \rightarrow x = y)$$

*T*1 is the theory of dense order; it is complete and obviously has no finite models.[9] *T*2 is a fragment of the theory of the successor relation;[10] there are no finite models for the theory, and moreover, no finitely axiomatizable extension of *T*2 is complete. It follows immediately that *T*1 and *T*2 have no model in common. Many other examples of a like kind could be given.

I see no possible fault with either of these explanations, aside from the fact that what they explain is rather trivial. Of course, such examples provide no demonstration that empirically equivalent theories having no model in common can always, or even often, be found, but then no such demonstration can be given until philosophers provide a precise account of what is to qualify as a theory and what is not.

Thus far nothing has been said about simplicity, and that is all to the good. Criteria for simplicity are irrelevant to the issue unless they restrict what can be a theory. Thus it might be required that a theory not have eliminable predicates, that is, predicates such that the theory contains an empirically equivalent sub-theory which does not use these predicates. But that is a rather trivial requirement and one which our counter-example already meets. Still, in keeping with Kneale's criterion, one might hope that, for a given non finitely axiomatizable collection of observation statements, there exists a *simplest* explanation. If such a simplest explanation did exist, it might be taken as the one and only *theory* explaining the given body of observation statements. No philosopher has to my knowledge, proposed such a canon of simplicity, nor have I succeeded in divising one. Nonetheless, I shall mention a simplicity criterion which may occur to some, but which does not work.

Let us say that, within a given language, one sentence *A* is simpler than another, *B*, if *A* entails *B* but not conversely. The simplest explanation in a fixed language of a body of observation statements would, then be that sentence entailing the observation statements and entailed by every other sentence entailing the observation statements. Such an account could easily be extended to provide simplicity comparisons of theories with different theoretical vocabularies, e.g., by taking theories to be equi-simple just if they have common definitional extensions. In a fixed language with only one binary predicate besides identity, the simplest explanation of the existence of an infinite number of objects would, then, be that sentence having only infinite models which is entailed by every other sentence having only infinite models. A proof that no such theory exists is given in the appendix. More generally, it is as a reasonable conjecture that given any first order language L, with identity, and any recursively enumerable but not finitely axiomatizable collection Δ of sentences in a proper sub-language of L, there is no sentence A in L such that A entails all sentences in Δ and every sentence B which entails all sentences in Δ entails A.

Kneale's criterion and modifications of it provide no support for the claim of argument B. Neither does there seem any point in working through other possible restrictions on theories one by one, all the while manufacturing toy counter-examples. What is needed in order to lay the *geist* of the doctrine is an example of two *actual* theories which are empirically equivalent but have no model in common. We can lay aside, as well, the dubious premise that there is an observation language. Most will, I hope, admit that physical geometries are actual theories. They are also theories which have been formalized and the metamathematics of which have been studied. Hans Reichenbach has argued, very convincingly I think, that almost any pair of geometries can be interpreted so as to be empirically equivalent.[11] In particular he has argued that, by changing his views about identity, a person who lives in a topologically compact space can consistently maintain a geometry which requires a non-compact topology. The example Reichenbach gives concerns a torus-space interpreted as Euclidean. However, completely analogous arguments can be given for any pair of geometries, one compact the other not, so long as the compact topology is homeomorphic to a quotient topology obtained from the non-compact topology. In particular, Reichenbach's argument can be straightforwardly reproduced for the case of elliptic and Euclidean (or hyperbolic) geometries. The elementary first-order fragments – that is, everything that can be said without set theoretic devices – of elliptic and Euclidean geometries have been formalized and studied. They have no model in common. (See Appendix.)

The argument I have made is not so complete as I should like, nor is its epistemic significance so clear. It is an objection, and a good one, that elementary geometry is only part of geometry not the whole of it. The introduction of set-theoretic devices brings with it, we know, a great explosion in the variety of structures which are models for a geometry. The argument would only be complete if it could be shown that reasonable formalizations of elliptic and Euclidean geometries (or some other pair), based on set theories with geometric points taken as ur-elements, have no model in common. My guess is that the argument can be completed in this way. For those who are realists but not Platonists, the argument is perhaps already good enough.

There are philosophers who find Reichenbach's arguments concerning the empirical equivalence of geometries convincing enough, but only so long as the geometries have the same topology. Professor Grunbaum seems to hold this view, although it is difficult to be sure. For them it would be pleasant to show that elementary Euclidean and hyperbolic geometries have no model in common, but I have no such demonstration. Still, since each of these theories is complete, and therefore has up to elementary equivalence only one model, it would indeed by surprising if they had a model in common.

It may well be that the discussion in this essay presupposes what is false, namely that there is an intelligible notion of empirical equivalence. But if the presupposition is correct, then the admission that there are empirically equivalent theories which are not synonymous seems to entail either that the true theory is sometimes unknowable or that, more simply, even all possible evidence can sometimes have more than one correct explanation.

APPENDIX

1. The claim is that the semantics proposed does not admit a proof theory with an effective notion of proof. To show this it is sufficient to show that the consequence relation generated by the proposed truth conditions is not recursively enumerable (r.e.); thus:

Let L be a first-order predicate language (with identity), V a proper sub-language of L such that there is at least one binary predicate (other than identity) which is in L but not in V. Let **Cn** be a relation on L such that for all sentences A, B in L, **Cn** (A, B) holds if and only if for every sentence C of V, C is provable from A only if C is provable from B. Then $\mathbf{Cn}\ (x, y)$ is not r.e.

Proof: Let T be a sentence of L which is undecidable and such that the consequences of T expressible in V alone are complete in V. A sentence Q of L is then refutable from T just if $\mathbf{Cn}\ (A\ \&\ 7A,\ T\ \&\ Q)$ is true. Again, a sentence Q of L is irrefutable from T just if $\mathbf{Cn}\ (T\ \&\ Q,\ T)$. Assuming that $\mathbf{Cn}$ is r.e., it follows that both the set of sentences refutable from T and the set of all sentences irrefutable from T are r.e. Since each of these sets is the complement of the other, both are recursive. Hence the set of all sentences refutable from T is recursive so T is decidable, which is a contradiction. This argument is essentially due to Richard Grandy.

2. The claim is that, given an axiomatizable theory T in a first-order language L (with identity and a finite number of predicates) with a proper sub-language V, if all of the completions of T are decidable and the set of consequences of T expressible in V alone has at most a finite number of non-isomorphic finite models, then there is a theory Q, in a language $\mathscr{L}$ having V as a proper sub-language but otherwise disjoint from L, which has the same V-consequences as T but no models in common with T.

Proof: By Craig's reaxiomatization theorem, the V-consequences of T are recursively axiomatizable. Craig and Vaught[12] have established the following: Let Δ be a recursively axiomatizable collection of sentences in a language V having at most a finite number of predicates. Further assume that Δ has at most a finite number of non-isomorphic finite models. Then there is a finitely axiomatizable extension Q of Δ in a language $\mathscr{L}$ got by adding a single binary predicate to V. Moreover Q is essentially undecidable. Let T be as in the claim above and Q the Craig-Vaught extension of the V consequences of T. Then Q and T have no model in common. For, supposing the contrary, then there exists a completion T^* of T and a completion Q^* of Q, having a common definitional extension. The intertranslation provides a recursive mapping $\phi : \mathscr{L} \rightarrow L$ such that for every sentence A in $\mathscr{L} Q^* \vdash A$ if and only if $T^* \vdash \phi(\mathrm{A})$. Since T^* is, by hypothesis, decidable, Q^* is therefore also decidable. But this is impossible since Q is essentially undecidable.

3. The claim is that in a language with identity plus a binary predicate there is no sentence which has only infinite models and which is entailed

by every sentence having only infinite models. For suppose there were such a sentence, A. Then if an arbitrary sentence S had only infinite models we could effectively find that it had only infinite models by searching through proofs from S for a proof of A from S. Hence the set of all sentences having only infinite models would be recursively enumerable. By a theorem of Vaught's[13] this set is not r.e. I am indebted to Andrew Adler for this argument.

4. That elementary elliptic and Euclidean geometry have no model in common is an immediate consequence of a theorem due to R. Robinson.[14] Robinson shows that elementary elliptic geometry can be expressed in a language with a single binary predicate, besides identity, whereas elementary Euclidean and hyperbolic geometry cannot be. Since they are complete, to have a model in common elliptic and Euclidean geometry would have to be intertranslatable. Robinson's result is essentially a proof that they are not.

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NOTES

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[1] H. Reichenbach, *Experience and Prediction*, University of Chicago Press, 1961, pp. 374–75.

[2] H. Putnam, 'An Examination of Grünbaum's Philosophy of Geometry' in *Philosophy of Science. The Delaware Seminar*, Vol. 2 (ed. by B. Baumrin), 1963.

[3] W. Salmon, 'Verifiability and Logic' in *Mind, Matter and Method* (ed. by P. Feyerabend and G. Maxwell), 1966.

[4] A. Grünbaum, 'Geometry, Chronometry, and Empiricism', in H. Feigl and G. Maxwell, eds., *Minnesota Studies in the Philosophy of Science*, Vol. III (ed. by H. Feigl and G. Maxwell), University of Minnesota Press, 1962, p. 446.

[5] (Added in proof). In an essay forthcoming in *Philosophy of Science* (December, 1970), Professor Grünbaum gives up the doctrine of equivalent descriptions. Grünbaum distinguishes claims which describe 'intrinsic' facts from those which do not. He now holds that empirically adequate, empirically equivalent theories do not necessarily say the same thing but do say the same thing about the 'intrinsic' facts. If it is the case, and I do not know whether it is, that two theories making different claims about the 'intrinsic' states of affairs cannot be empirically equivalent, then Grünbaum's most recent views do not contradict what I shall say in this essay.

[6] Thus, compare W. V. O. Quine's more recent writings with the views on theoretical equivalence expressed in 'Semantics and Abstract Objects', *Proceedings of the American Academy of Arts and Sciences* **80** (1951). Similarly in *Patterns of Discovery*, N. R. Hanson espoused the view I shall criticize. He later gave it up; see his 'Equivalence: The Paradox of Theoretical Analysis' in Feyerabend and Maxwell, *op. cit.*
[7] An equivalent notion is given in K. de Bouvere 'Synonymous Theories', in *The Theory of Models* (ed. by J. Addison *et al.*), 1965.
[8] W. Kneale, *Probability and Induction*, 1949.
[9] See J. Schoenfield, *Mathematical Logic*, 1969, p. 102.
[10] The example and its properties are due to William Hanf, 'Model Theoretic Methods in the Study of Elementary Logic', in *The Theory of Models* (ed. by J. Addison *et al.*), *op. cit.*
[11] H. Reichenbach, *The Philosophy of Space and Time.*
[12] W. Craig and R. Vaught, 'Finite Axiomatizability Using Additional Predicates', *Journal of Symbolic Logic* **23** (1958).
[3] R. Vaught, 'Sentences True in All Constructive Models', *Journal of Symbolic Logic* **25** (1960).
[14] R. Robinson, 'Binary Relations as Primitive Notions', in *The Axiomatic Method* (ed. by L. Henkin *et al.*), 1959.