THE UNDERDETERMINATION OF THEORY BY DATA

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I. THE REALIST THESIS

Can there be theories which are underdetermined by all actual and possible observations? That is, can there be logically incompatible but empirically equivalent theories? In this paper which falls into three sections it will be argued that a suitably refined version of this question ought to be answered in the affirmative. In the first section of the paper attention will be drawn to an insufficiently appreciated reason for being interested in this question. For most recent discussion related to this question has focused not on the question itself but on Quine's notorious claim that as *all* theories are underdetermined, translation is indeterminate. If we consider the current fashion in the philosophy of science for realism we will be able to discern other reasons for being interested in the question. For as I shall argue giving an affirmative answer to the question is not compatible with realism as it is standardly understood. In section 2 I will consider the arguments that have been or might be advanced for thinking that theories cannot be underdetermined. Having rejected these arguments I will seek to establish that underdetermination can arise by constructing examples. In the final section I develop two different modifications the realist might make in his position in the face of what will be called *the realist's dilemma*. While reasons will be given for favouring one of these responses, it will not be possible to definitely adjudicate between them within the confines of the present paper.

With the exception of some distinguished authors (among whom are Kuhn and Feyerabend) it is fashionable to espouse some form or other of realism. Those who style themselves realists generally see their position as constituted by the following four ingredients. First, scientific theories are either true or false and which a given theory is, it is in virtue of how the world is. In this context a theory is to be thought of as the deductive closure of a set of postulates and to speak the truth or falsity of a theory is to talk of the truth or falsity of the conjunction of the postulates. This will be called the *ontological ingredient* in realism. A second, and arguably not independent ingredient, which will be called the *causal ingredient* is the claim that if a theory is true, the theoretical terms of the theory denote theoretical entities which are causally responsible for the observable phenomenon whose occurrence is evidence for the theory. It would be of small comfort to learn that our theories are indeed either true or false representations of the world if we were precluded from being able to have rationale grounds for believing that any theory is more likely to have one truth-value than the other. And our quite insatiable desire to know manifests itself in the third ingredient which will be cited as the *epistemological ingredient*. This is the claim that we can have warranted beliefs (at least in principle) concerning the truth-values of our theories.

Implicit in the characterization so far developed is the assumption that the goal of the scientific enterprise is the discovery of true explanatory theories. The reasonableness of this goal as stated has seemed to some to be called into question by the fact that the history of science is a grave yard of falsified theories. Indeed, there seems to be evidence to support the meta-induction that any theory is found to be not strictly speaking true within, say, two hundred years of its being produced. Unless one is willing to take the courageous line of arguing that things are now or will be much different, the evidence points to the same fate for our current theories. And, thus, it looks as though the exercise of the epistemological power the realists assumes we possess will always end in a negative verdict. This might incline us to wonder how rational it is to pursue a goal when the evidence is that that goal will never be realised. The realist counter-move is to argue that while all theories are false, some are falser than others. That is, the historically generated sequence of theories of a mature science may well be a sequence of false theories but it is a sequence in which succeeding theories have greater truth-content and less falsity content than their predecessors. This empirical thesis, which will be called the *thesis of convergence*, would, if tenable, render it rational to pursue the goal of truth for we would at least have some assurance that we were getting nearer the unobtainable goal.

The thesis of convergence together with the ontological, causal and epistemological ingredients constitutes a sort of minimal common factor among the wide range of philosophers who in recent

years have advocated a realist construal of scientific theories. These would include Boyd, Harré, Hesse, Popper and Putnam among others.[1] This position needs modifying, I shall argue, for as it stands it presupposes that the underdetermination of theory by data cannot arise.

2. THE UNDERDETERMINATION OF THEORY BY DATA

At this juncture we need to articulate with some greater clarity the thesis of the underdetermination of theory by data. Quine expresses the thesis as follows:

> Consider all the observation sentences of the language: all the occasional sentences that are suited for use in reporting observable events in the external world. Apply dates and positions to them in all combinations, without regard to whether observers were at the place and time. Some of these placed-time sentences will be true and the others false, by virtue simply of the observable though unobserved past and future events in the world. Now my point about physical theory is that physical theory is underdetermined even by all these truths. Theory can still vary though all possible observations be fixed. Physical theories can be at odds with each other and yet compatible with all possible data even in the broadest sense. In a word, they can be logically incompatible and empirically equivalent. This is a point on which I expect wide agreement, if only because the observational criteria of theoretical terms are commonly so flexible and fragmentary.[2]

That is, we have a case of underdetermination if for some subject matter we have two theories, T_1 and T_2, which are 1) incompatible and 2) both compatible with all actual and possible observations. It is essential to bear in mind the force of the reference to actual and *possible* observations. For the thesis is not the uncontentious claim that a situation could arise in which at some moment of time all observations, text, experiments made to date have de facto left two rival theories in the field. For in the context of the sort of underdetermination Quine has in mind the outcome of *any* possible observation would either support both theories equally or count equally against both theories. If there can be such situations, the question as to which (if either) of the theories is true or correct would be *empirically undecidable*.

According to Quine *all* theories are underdetermined by the data; that is, it is held that for any subject matter there are incompatible theories all of which fit the data equally well. I will refer to this thesis of Quine's as the *strong UT thesis*. By the *weak UT thesis* I will mean the thesis that there *can be* cases of underdetermination. It is not clear that Quine has any non-question-begging argument to support this strong contention. Indeed, he tends quite candidly to offer this as something on which general agreement can be expected. Not unexpectedly the response to this contention of Quine's has been one of scepticism. This is not to say that such skepticism has been well-grounded by the production of a multitude of forceful arguments. On the contrary, while there has been considerable discussion concerning Quine's claim that the indeterminancy of translation follows from the underdetermination thesis, this thesis itself has received scant attention. No doubt this is so because most writers have been inclined, not unreasonably, to retort that Quine has provided no reason to think the thesis tenable. In this paper I want to partially rectify this deficiency on Quine's behalf by providing a proof by example of the weak *UT* thesis.

One might wish to object that the thesis as formulated involves the untenable presupposition that there is some viable dichotomy between observational propositions and theoretical propositions. For Quine takes it that a pair of theories are incompatible yet empirically equivalent if and only if they agree on the distribution of truth-values over the set of observational propositions and disagree on the distribution of truth-values over the set of theoretical propositions. Certainly the assumption that there is a difference in kind between observational propositions and theoretical propositions is dubious. But presumably one who denies that there is such a dichotomy will nonetheless agree that propositions vary in the degree to which they are observational or theoretical. In this case there will be a spectrum ranging from the more observational to the more theoretical. That being so we ask of a pair of incompatible theories where along this spectrum they differ in the ascriptions of truth-values. Whether or not the theories are to be regarded as observationally equivalent and theoretically divergent will depend on where along the spectrum one is willing to say we have got theoretical enough. And as, *ex hypothesi*, we are dealing with a difference of degree and not a difference of kind this will be a matter for decision. In light of this we can reformulate the underdetermination thesis as follows: No matter where one fixes the point demarking the observational from the theoretical (so long as one does not put this point at either end of the spectrum) there can be (or, in the case of the strong version of *UT*, must be) theories which agree on the distribution of truth-values to propositions on the observational side of that point and disagree on the ascription of truth-values to some propositions on the theoretical side of that point.

Given that there is no natural bipartite division of the propositions of a scientific language into those that are observational and those that are theoretical, the claim that in a particular case we have underdetermination will have to be relativized to a particular division. Such a claim may be more or less interesting depending on the particular division made. The division implicit in the examples to be given is such as to render interesting the claim that these constitute cases of underdetermination. In what follows it is assumed that some division has been made and the claim that two theories are empirically equivalent is to be understood as the claim that the theories agree on the distribution of truth-values over those propositions deemed observational. The question arises as to whether counterfactual observational propositions should be included. For Putnam[3] has objected to Quine's exclusion of counterfactual observation propositions about what would have been the results of experiments that were not but might have been performed (an exclusion which Quine's horror of propositions would lead him to formulate in terms of sentences). This means that two theories which agreed on the truth-values of non-counterfactual observational propositions while disagreeing on the truth-value of counterfactual observational propositions would be regarded by Quine as empirically equivalent and by Putnam as empirically inequivalent. Except for the following comment this question will not be further discussed for my examples are empirically equivalent on Putnam's more stringent criterion. To see the problem which arises if observational counterfactuals are included let T_1 and T_2 be theories which are empirically equivalent in Quine's sense and let p be a counterfactual observation proposition which is assigned a different truth-value by these theories. Presumably if this is to establish empirical inequivalence for Putnam there must be a matter of fact at stake with regard to p. As will be seen later one response to underdetermination is to conclude that with regard to those propositions responsible for the underdetermination there is no matter of fact at stake. One who makes this response would not think there was a matter of fact at stake with regard to p if the only way to assess the truth-value of p was by reference to these propositions. In view of the fact that the viability of this response is one of the crucial issues about underdetermination it would seem reasonable that in assessing empirical equivalence those counterfactual observations whose truth-value can only be assessed by reference to T_1 and T_2 should be excluded.

Before developing examples of underdetermination it will be fruitful to consider three strategies that have been deployed in arguing against the possibility of underdetermination. First, it is easy to see that on certain views of the nature of theories underdetermination could not arise. This would, for instance, follow trivially given a reductionist construal of theories which treated all theoretical propositions as translatable into observational propositions. And this result follows almost as trivially if one holds that what a theory really is is more perspicuously represented either by its Craig replacement or by its Ramsey replacement.[4] However, in the absence of convincing reasons for thinking of theories along the lines of Craig or Ramsey these results are of no particular significance. Indeed, one might argue that the possibility of underdetermination provides additional support for the claim that Craig and Ramsey's theories cast no particular light on the nature of theories.

A second strategy would involve arguing that considerations other than fit with observational data are relevant in deciding between incompatible theories. An argument of this form is suggested by the following passage from Swinburne's *Space and Time*:

> Compatible with any finite set of phenomena there will always be an infinite number of possible laws, differing in respect of the predictions they make about unobserved phenomena. Between some of these ready experimental tests can be made, but experimental tests between others is less easy and between them we provisionally choose the simplest hypothesis. Evidence that a certain law is simpler than any other is not merely evidence that it is more convenient to hold that suggested law than any other, but evidence that the suggested law is true.[5]

One who thinks with Swinburne that simplicity is a guide to the truth might hope to decide between rival theories which are compatible with all actual and possible observations by comparing the theories as to simplicity. This will, however, be somewhat problematic in view of the notorious difficulties involved in producing a reasonable criterion of relative simplicity. And then assuming we had such a criterion there is no reason to assume that any pair of incompatible empirically equivalent theories will be such that one is simpler than the other. Indeed, I claim that on any reasonable criterion of relative simplicity the examples to be developed involve theories of equal simplicity. In any event there is no reason to think that simplicity is a guide to truth. To have evidence that it is one would have to have evidence that the world was simple. If we suppose that there are two rival theories which can only be chosen between on the grounds of simplicity, we will have already to have shown that the simpler is more likely to be true in order to have evidence that the world is simple in the requisite sense. Thus, it is not clear that there can be any non-question-begging argument to support a principle of simplicity in this context. This is not to say that there are not good reasons for preferring the simpler of two rival theories. For, after all, it may be easier to manipulate such theories. However, we can agree to this without agreeing that its being simpler makes it more likely to be true. Simplicity makes theories more likeable but not more likely to be true. I recognize that this is a contentious and unsubstantiated claim. That need not concern us here if I am correct in claiming that my sample theories do not differ with regard to simplicity.

The third strategy is exemplified in the following remark of Dummett's

> Quine's argument for the indeterminancy in the stronger sense is based on the claim, over which, he says, he expects wide agreement, that there can be empirically equivalent but logically incompatible theories . . . but the claim is absurd, because there could be nothing to prevent our attributing the apparent incompatibility to equivocation.[6]

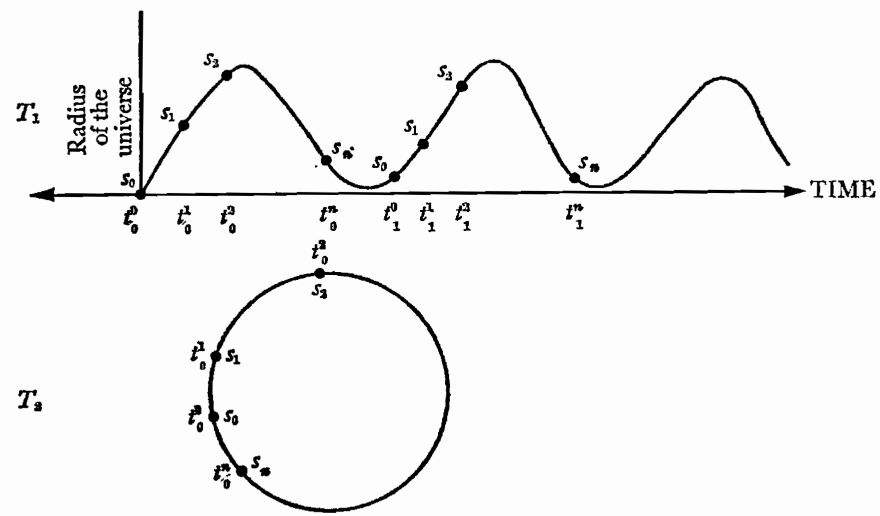
An adequate discussion of this objection would take us far beyond the confines of this present paper. However, we can at the very least show that it is not obvious that the move suggested by Dummett can always be made by constructing examples which it would be unreasonable to treat as cases of equivocation. And perhaps we can do slightly better than this. Suppose we have a pair of theories that appear incompatible but really constitute a case of equivocation. Let T_1 and T_2 be first-order formalizations of these theories (formalizations which stand to the theories as first-order formal arithmetic stands to arithmetic). If we have a case of equivocation, T_1 and T_2 will be more notational variants of one another in the sense that there are definitional extensions of T_1 and T_2, T_1' and T_2', which are such that any theorem of T_1' is a theorem of T_2' and *vice versa*. So if the formalizations of two theories do not have common definitional extension satisfying these conditions, the theories do not consititute a case of equivocation. Given this as a sufficient condition of non-equivocation, the examples to be given are not equivocal.

The crucial assumption on which both examples turn is the assumption that time does not possess whatever structure or topology it does possess as a matter of necessity. That is, while we take time in fact to be linear, continuous, non-ending and non-beginning, there is no logical or conceptual absurdity or inconsistency in the supposition that it should have some other structure. This assumption that just what structure time possesses is something that cannot be discovered by *a priori* reflection but can only be ascertained by empirical investigation cannot be argued for within the confines of this paper. However the theories to be developed do lend support to this assumption.[7]

For the sake of the first example let us begin by thinking of time as linear, non-ending and non-beginning. Let us imagine that we have come to have evidence that a cyclical cosmological model best fits the universe. Thus we are lead to conjecture that the universe is a sort of giant accordion that always has been and always will be oscillating in and out. Let us suppose further that as the evidence comes it tends to support the bolder hypothesis (an hypothesis of greater content in Popper's sense) that the universe during any one period of oscillation is quite similar to the universe during any other period of oscillation. Finally someone casts caution to the winds and conjectures: The universe is at time t_0 in a state of type S_0. At some time t_1 in the future the universe will *again* be in the same type of state S_0. Further, following time t_1 the universe will run through a sequence of states qualitatively identical of states that it runs through between t_0 and t_1. And so on and so on it runs with boring repetition indefinitely into the future. And similarly, it has run

with this lack of novelty for ever in the past. This conjecture, which will be called theory T_1, is the conjecture that *time is linear and history is precisely cyclical.*

Under the supposition that all the observations that can be made support theory T_1 we can construct a rival theory T_2 which will be equally supported by that data. This is the theory that *time is closed.* That is, time has a structure like that of a circle so that there is just *one* occurrence of each type of state. Each occurrence of a particular type of state, however, lies in both the past and the future. We can represent the contrast between theories T_1 and T_2 in the diagram given below—



There are two points about these examples to which explicit reference should be made. First, in the story as told there is no possibility of distinguishing between, say, the times t_0^i and t_1^i by reference to differences in the mental states of observers present at these times. For the states of the universe including the mental states of conscious beings are qualitatively identical at these times. We can either suppose that someone present at t_0^i dies before t_1^i and is replaced by a qualitatively identical counter-part (who, depending on which theory we adopt, is or is not numerically identical to the counter-part person) or we can suppose that the person in question has a continued existence between t_0^i and t_1^i, in which case what he claims to remember at t_0^i is exactly what he claims to remember at t_1^i. Secondly, the picture associated with the closed time theory cannot be construed as a picture of the same time occurring again and again. To run one finger around the circle, so to speak, notionally counting the occurrences of t_0 is to embrace the incoherent notion that the same time could occur again and again. That is, it is to combine illegitimately the notions of closed time and of open time. In closed time each time is present but once. It is simply that what lies in the future lies in the past and *vice versa*. Admittedly this is, to say the very least, a most counter-intuitive picture of time. But as should be familiar from the character of many successful scientific theories of the twentieth century, intuitive, common-sensical judgments may be very bad guides to the truth both in the domain of cosmology and in the domain of quantum mechanics. Perhaps some of the counter-intuitive feeling is removed when it is realized that we could introduce with the help of a conventional stipulation an asymmetrical directed temporal relation which is locally transitive but globally non-transitive. Thus what lies in the local future does not lie in the local past and *vice versa*.

The theory T_2 will commend itself to one who—operating under the *initial* assumption that time is linear—reasons as follows. There is nothing that distinguishes the time t_1^i in the future when the universe will be in state S_0 from the time t_0^i, the present time, at which the universe is in state S_0. For whatever is true of the time t_0^i is true of the time t_1^i. Consequently the best conjecture to make is that the time t_0^i is identical with the time t_1^i. In making this conjecture one is dropping the initial assumption that time is linear and replacing it by the assumption that time is closed.

T_1 and T_2 are clearly incompatible theories. However, the data that support T_1 support T_2 equally. The advocates of T_1 find that the data are exactly what would be expected if T_1 were true. The data in this case consist of support for the conjecture that the universe will in the future be in a state qualitatively identical with the current state. However, these data are exactly what the advocate of T_2 would expect given the truth of T_2. He too expects that there is in the future a state qualitatively identical to the present state. So, in the imagined context, the choice between theories T_1 and T_2 is empirically undecidable. The difference between the theories is shown by the fact that given T_1 the future time at which the universe will be in a state qualitatively identical with the present

state is a time distinct from the present time and, on the other hand, given T_2 this future time at which the universe is in a state qualitatively identical with the state it is in at present is the same time as the present time.

It is important to note that the two theories in question ascribe non-isomorphic structures to time. That is, there is no one-one order preserving map between a structure having the topology of a closed curve and a structure having the topology of an open curve. It is this fact which guarantees that T_1 is not a mere notational variant of T_2.

It might be argued that the choice between these theories can be decided by non-empirical means. For instance, some have held that closed time is incoherent on the grounds that it is true in virtue of what we mean by 'past', 'present' and 'future' that no event can at the same time be past, present and future. In closed time when an event is present, it is then also past and future. However, even granting the claim about what we mean by these worlds this does not show closed time to be incoherent. What the argument in fact shows is that if we are to adequately characterize closed time we need to replace the simple tenses of ordinary language by more complex tenses.[8] It has, on the other hand, been argued[9] that non-empirical considerations could decide the issue in favour of closed time. For instance, some have taken it to be a necessary truth that if whatever is true at time t_0 is true at time t_1, t_1 is the same time as t_0 where the scope of the quantifier is restricted to propositions that can be expressed without reference to the times t_0 and t_1. If that were so, theory T_1 (open time, cyclical history) would be incoherent. However, it will not do in this context to rule out T_1 by claiming that the specialized form of the identity of indiscernibles given above is a necessary truth. For it is just this sort of situation (open time and cyclical history) that tests this claim. Indeed, I would argue that the principle cannot be necessarily true on the grounds that T_1 is an intelligible theory. The two arguments above are but a sample of the considerations that might be introduced in an attempt to show that the choice between T_1 and T_2 can be decided by non-empirical means, given that T_1 and T_2 both fit the observable data. And the acceptability of this as a genuine example of underdetermination is conditional on the assumption which cannot be justified within the confines of this paper that there are no non-empirical factors which would decide the issue.

Before leaving this example I want to introduce an heuristic device for thinking about this situation to which I will return below. To this end consider two possible worlds A and B. We stipulate that in world A time is linear and history precisely cyclical so that a theory of the type T_1 is true of this world. We stipulate that in world B time is closed so that a theory of the type T_2 is true of world B. We assume also that the entire set of states constituting world B is qualitatively identical with the sequence of states in any one oscillation of world A. Imagine that you are to be placed in one of these worlds. You will not be told which world it is. The question then is—just what possible observation could you make to ascertain whether you are in world A or in world B? My suggestion has been that there is nothing you could do. While these are very different kinds of worlds the question as to which world it is that you are in is empirically undecidable.

The theories considered above do serve to make the point that the choice between theories can be empirically undecidable. It must, however, be admitted that these theories are not rich in explanatory content nor are they nor have they ever been regarded as respectable theories about the actual world. Consequently it is of interest to consider whether an empirically equivalent but logically incompatible rival can be generated for a rich and sometime respectible theory such as Newtonian mechanics. To see that this is possible we must first note some salient features of Newtonian mechanics. If one develops a rigorous axiomatization of Newtonian mechanics it is necessary to postulate that space and time are continuous. That is, that the instants of time have the order type of an interval of the real number line as do the points of space along a given direction. This assumption is made as we wish to represent the motion of a particle by a continuous function, the position function, from real numbers (representing time) to triples of real numbers (representing position in space). This guarantees that the differential of the position function is well-defined and licenses us to represent Newtonian mechanics by means of a family of differential equations including for instance

$$F = \frac{m d^2x(t)}{dt^2}$$

If on the contrary, we have assumed space and time to be merely dense and not continuous (*i.e.*, that the instants of time are isomorphic to an interval of rational numbers) we could not adopt Newtonian mechanics for the notion of a differential would not be applicable.

The assumptions of continuity mean that, restricting attention to point particles, we are assuming that the history of the motion of such a particle described in a particular frame of reference is to be represented by a set of ordered four-tuples of real numbers. However, when we directly measure duration or length we will never have occasion to use anything other than rational values. For any technique of measurement has some limit of accuracy and will be accurate only up to some finite number of decimal places. As a terminating decimal can be represented by a fraction we need only the rational numbers in assigning values to a directly measured parameter. There is just no technique one could follow which would lead one to record the results of an actual measurement by means of a non-terminating, non-repeating decimal, *i.e.*, by a non-rational real. In gathering evidence for Newtonian mechanics we will assign a rational value to the time at which the particle has some location which is assigned a triple of rational values. Putting these values into the equations will lead to a prediction about the position of the particle at some later time. The prediction may well generate non-rational values for the parameters. However, on checking the prediction we will assign only rational values to the parameters. If these values approximate to the predicted values this would be regarded as evidence for Newtonian mechanics.

The preceding considerations reveal a sense in which Newtonian mechanics goes beyond all actual and possible data. For while the data consist of assignments of rational values to the parameters in question, it is part of the theory that the parameters take real values which in some cases will be non-rational reals. This prompts the speculation that one could develop a rival theory with the same explanatory power which does not go beyond the data in this regard. Such a theory which will be called Notwen's mechanics can be developed. However, within the confines of this paper I will not be able to do more than provide a programmatic sketch of what would be involved in developing such a theory. We can think of Notwen as someone who shares Newton's general ideas about the relations between force, mass, acceleration *etc.* However, Notwen wishing to have a more parsimonious ontology assumes that space and time are dense but not continuous. Consequently he does not avail himself of differential equations in specifying the theory but characterizes the theory in terms of difference equations which unlike differential equations involve only operations that are closed in the rationals, Notwen's force law will be given as follows:

$$\dot{F} = \frac{m\,[x(t+2h) - 2x\,(t+h) + x(t)]}{h^2}$$

In effect Notwen's mechanics deals with average velocities and accelerations rather than instantaneous velocities and accelerations. The 'h' in the equation above represents a rational interval over which the averages are being taken. By taking h sufficiently small Notwen's equations can be made to approximate the Newtonian ones as closely as one likes. It is important that in specifying his theory, Notwen does not specify a particular value for h. Rather, his claim is that there is some rational value h for which the theory will fit all the data.

To have an adequate theory of mechanics one needs to do more than develop a system of difference equations which mirror the empirical predictions of the system of differential equations which constitute the core of Newtonian mechanics. One needs in addition, for instance, to assume a geometry for space. Notwen cannot avail himself of a differential geometry such as Euclidean geometry for that involves continuity assumptions. From Notwen's point of view it would be nice if one could develop a difference geometry analogous to Euclidean geometry. There are, however, certain technical problems involved in this project. For in *standard* measure theory it is not possible to give non-trivial measures for denumerably infinite sets. And under the density assumptions there will only be a denumerably infinite set of temporal instants and spatial points. I am inclined to regard this as a mathematical problem which it would be interesting to attempt to solve. However, even if this problem should prove intractable, it is not a decisive objection to Notwen. For Notwen could use the full range of mathematical techniques employed by Newton. In this case, in using an interval of the real number line in representing, for instance, the instants of time in an interval of time, Notwen would regard the non-rational reals as specifying ideal elements added for heuristic purposes and not as specifying actual instants of time. Only the rational reals would be regarded as identifying instants of time. That is, Notwen only affirms the existence of some of the items talked about in his theory. Talk of the other items is a convenient fictional device.

Notwen's theory with its postulation of merely dense space and time and Newton's theory with its postulation of continuous space and time are clearly incompatible. However the theories are empirically equivalent in the sense that an observation counts for (against) Newton if and only if it counts for (against) Notwen. Notwen and Newton will test their theories by measuring the values of the parameters and plugging these values into the equations to generate predictions. As was noted, the measured values with which they both begin will be represented by rational numbers. In a world in which Notwen's theory is successful a test of Notwen's theory will involve predictions of rational values for parameters which subsequent measurement supports. In this test Newton may predict the parameter to have a nearby non-rational value. However the subsequently measured rational value will be regarded as supporting the theory in virtue of being an approximation to the true value. On the other hand, if Newton's theory is borne out Notwen can find a value of h which is such that his theory is confirmed by the observations confirming Newton. In a similar vein we can see that observations which are disconfirming of one theory will be disconfirming of the other theory. Thus, the choice between these theories is an empirically undecidable matter.

It may be objected that I have not established that no empirical discovery could decide between these two pairs of theories on the grounds that the history of physics reveals the relevance of certain wider, more general empirical grounds than I have considered for holding one theory to have greater verisimilitude than a rival theory. To see how this objection might be developed consider a situation in which the actual available observational data underdetermine the choice between two rival theories. It might be that there is a more general theory of wider scope in which only one of these theories can be embedded. Given that the wider theory has some degree of empirical success it would be reasonable to opt for the theory compatible with that theory. Zahar has recently argued[10] that in 1905 the available data underdetermined the choice between Special Relativity and the Lorentz ether-drift theory but that only the Special Theory was or could be embedded in a gravitational theory (the General Theory of Relativity), and that this constitutes the empirical grounds for preferring Einstein to Lorentz. This situation might well arise in relation to my first example. For it might be that only one of the two rival theories is compatible with the best total physical theory we can devise where that theory as it turns out does not have, as far as we can tell, an empirically equivalent rival. While this is a possibility, there is no reason to assume *a priori* that the best total physical theory (if there be such a theory) will decide between the rival hypotheses or that there is a unique best total theory as opposed to two empirically equivalent rival total theories one of which favours the closed time hypothesis the other of which favours the open time hypothesis. Consequently I am inclined to concede that I have not established my example to be definitely a genuine case of underdetermination. However, it does serve to establish that there is no reason to assume that such a situation cannot arise. And thus the ball is put back into the court of one who insists that underdetermination is not possible.

In any event this style of objection does not seem to have force against my second example. For it would seem that a more general theory will decide in favour of, say, continuous time only if it involves some continuity postulates or other. And, using the devices I employed, one could construct an empirical equivalent rival of that theory which employed mere density assumptions. This theory would then decide in favour of the dense but not continuous space and time. So regress to more general theory in the case of this example will leave the example as a genuine case of underdetermination.

3. THE REALIST DILEMMA

Given that there can be cases of the underdetermination of theory by data, realism as characterized has to be rejected. For given that all actual and possible data relevant to some subject matter S falsify all theories for S except the incompatible theories T_1 and T_2 both of which fit the data equally well, the ontological ingredient in the realist position leads the realist to hold that there is something in virtue of which either T_1 is true or T_2 is true. However, *ex hypothesi*, nothing is going to count in favour of T_1 over T_2 and *vice versa*. In this context nothing could count as the evidence for thinking that T_1 is more likely to be true than false. This is incompatible with the epistemological ingredient in the realist position which hold out the hope (at least in principle) of having warranted beliefs concerning the truth-value of our theories. To use one of our examples—the realist wants to hold both that the world is such that either time is closed or it is not and that we can come to have evidence concerning which it is. However, we have seen that in some contexts this is not possible.

At this juncture something has to give. One might try, on the one hand, to weaken the ontological ingredient. Or, on the other hand, one might try to weaken the epistemological ingredient. Initially anyone with realist sympathies inclines to respond to the dilemma by weakening the epistemological ingredient. One so inclined will insist that all scientific propositions have determinate truth-values and, while maintaining that in most cases we can (in principle) have warranted beliefs concerning the truth-values of our propositions, it will be conceded that this does not hold for the non-empty class of empirically undecidable propositions. The reason this response seems to be the most plausible, indeed to be the *only* plausible response, lies in the following two factors. First, if one has any sympathy with a realist position one will have adopted a 'correspondence' theory of truth, in the sense that a proposition is true or false in virtue of how the world is. Secondly, we tend to believe in the Law of Bivalence which amounts to the claim that any proposition is either true or false. We cannot abandon a 'correspondence' theory of truth without entirely extinguishing the spirit of realism. And given that there are at most two truth-values, and given that if a proposition has one truth value its negation has the other truth-value, a commitment to bivalence amounts to a commitment to the Law of the Excluded Middle (hereafter cited as LEM). Having a robust sense of common sense we don't see how we can abandon LEM. For it is, after all, one of the *immutables* in virtue of being a law of logic. (If you can't believe that what can you believe?) But these two ingredients basically constitute the ontological ingredient of realism. For example, by appeal to LEM we assert that either time is closed or it is not closed. And by appeal to the correspondence theory of true we conclude that there is something about the world in virtue of which one or other of these alternatives is true. So the only way out of the dilemma seems to involve supposing that there are *facts* concerning which we can have no evidence. Now this response to which we seem driven is not entirely implausible. For surely it might be retorted that it was a piece of not inconsiderable arrogance in the first place on the part of the human intellect to assume that all there is to be known can be known by finite beings such as ourselves. This response, which I will call the *Ignorance Response*, involves maintaining that those propositions responsible for underdetermination are either true or false. It is conceded that with regard to these propositions we could not possibly have evidence concerning their truth-value. As such this response involves embracing the possibility of *inaccessible facts*—facts concerning whose obtaining or non-obtaining we could have no information.

Alternatively a realist might respond to the underdetermination of theories by restricting the scope of his realism in the following sense. Given a context in which some proposition *P* is empirically undecidable, the assumption that either *P* is true or *P* is false is withdrawn. As the realist holds that to be true (false) is to be true (false) in virtue of how the world is, this response involves dropping the assumption that there is something about the world in virtue of which *P* is true or something about the world in virtue of which *P* is false. That is, instead of supposing that there are inaccessible facts in virtue of which *P* is either true or false, we conclude that the world is simply indeterminate with respect to *P*. This response will be called the *arrogance response* for it amounts to holding that if we cannot know about something there is nothing to know about.

Consider the first example of an empirically undecidable proposition in light of these alternative responses. Many have a strong inclination to say of such a possible world that in it time either is closed or it is open—and that is that. Either the future occurrence of some state is a new and different occurrence of that state (*i.e.*, time is open) or it is numerically the same occurrence (*i.e.*, time is closed). Either time is such that it is like a closed curve or it is such that it is like an open curve. In making this response (the ignorance response) we are taking underdetermination as pointing only to our inability to have evidence concerning which of these possibilities actually obtains. That is, we have a case of inaccessible facts. One who makes the arrogance response will regard the heuristic

device I introduced in the discussion of the possible world as inadmissible. The device in question involved us in imagining that we had been placed in one of a pair of possible worlds (in one of which time is closed and in one of which time is open). Such a move would be judged illegitimate by one inclined to make the arrogance response on the grounds that one is just not entitled to make these stipulations. For there is no determinate state of affairs which either obtains or does not obtain whose obtaining would make it true that time is closed. The set of facts constitutive of the one world, it would be claimed, *is the same as* the set of facts constitutive of the other world, and in that set of facts there is no fact answering to the proposition that time is closed and there is no fact answering to the proposition that time is open.

One who makes the arrogance response in the face of an empirically undecidable proposition *P* will not be willing to assert that either it is the case that *P* or it is not the case that *P*, and is thereby committed to denying LEM to have the status of a genuine law of logic. On the other hand, one who makes the ignorance response is likely to invoke the claim that LEM is a genuine truth of logic in attempting to justify his claim that there is a matter of fact at stake with regard to *P*—a matter of fact which is inaccessible. Consequently one inclined to the arrogance response may well wish to avail himself of the interesting arguments Dummett has explored for not asserting LEM.

The consequences of this line of argument strike some as implausible. For there are many cases in which our intuitive inclination is to assert a substitution instance of LEM (*e.g.*, Either a city will be built at the North Pole some day or a city will never be built at the North Pole) where, given the line of argument we explored by Dummett, we would not be entitled to do this. However, there is a weaker strategy which might be deployed by someone to vindicate the arrogance response without embracing these apparently implausible consequences. To develop this strategy we need first to remember that we are dealing with propositions and contexts in which those propositions are empirically undecided in the sense that fixing the truth-value of all observation sentences leaves the truth-value of those propositions open. For instance, the first example of underdetermination provides such a proposition (that time is closed and history is unique) and such a context. One inclined, as I am, to the arrogance response sees no reason to admit in such a context the requisite inaccessible facts. Thus, without asserting that there can be no inaccessible facts, one asks of one making the ignorance response what his grounds are for asserting that there are inaccessible facts which either make it true that time is closed *etc*., or make it true that time is open, *etc*., (we are assuming that all other alternatives have been excluded so that the only way in which time can fail to be closed is if it is open).

It will not do for the advocate of the ignorance response to appeal to LEM. For what is in question is just whether LEM holds for empirically undecidable propositions. Consequently it is not clear what could possibly count as a reason for thinking that there is a matter of fact (a matter of inaccessible fact) at stake here. For there is nothing that would be explained by the supposition that there are such facts. And that being so, we should prefer the ontologically weaker position which does not assert the existence of such facts. This means restricting the scope of LEM to exclude empirically undecidable propositions. If the underdetermination of theory by data is a relatively rare phenomenon this will not mean a very extensive restriction. The limited scope of the restriction arises from the assumption that there is a matter of fact at stake with regard to any observational proposition. Given this assumption we can still assert, for example, that either a city will be built some day at the North Pole or a city will never be built at the North Pole. For a distribution of truth-values over the set of all observational propositions will determine the truth-value of 'There will be a city built some day at the North Pole'. One who denies this assumption will have to embrace a more extensive restriction on LEM.

To opt for the arrogance response means ceasing to regard empirically undecidable propositions as expressing hypotheses about the facts. Consequently the onus is on who so opts to give an account of the rôle those propositions play in the theories that contain them. One possibility is that these propositions should be seen as serving to specify a mode of description or general framework within which the hypotheses about the facts are to be expressed. That is, for instance, one who asserts that time is continuous is not making a guess about the facts but is opting for a particular net for catching the facts. All the facts that there are can be expressed in terms of this framework; or, equally they can be expressed in terms of the rival framework based on the treatment of time as merely dense. This possibility will have to be explored elsewhere. My concern now is only to note the need of some such account.

My primary aim in this paper has been to produce reasons for thinking that the weak *UT* thesis holds. A consequence of this is that the realist construal of scientific theories, as not uncommonly understood by philosophers of science, is untenable. For given *UT* the ontological and the epistemological ingredients in realism cannot be simultaneously satisfied.[11] Two different modifications (the ignorance and the arrogance responses) that the realist might make in the face of this dilemma have been noted and a reason was given for favouring the arrogance response. Consequential modifications are required in the specification of the causal ingredient and in the thesis of convergence as these depend on the ontological and epistemological ingredients. The two modified forms of realism while quite different have much in common including the difficult and pressing problems of providing an analysis of verisimilitude and a defence of the convergence thesis, difficulties whose exploration will have to await another occasion.

NOTES

[1] See in this regard M. Hesse, *The Structure of Scientific Inference* Macmillan, 1974), 290; R. Harré, *The Philosophies of Science* (Oxford University Press, 1972), 90; R. Boyd, "Realism, Underdetermination, and a Causal Theory of Evidence" *Nous* 1973; H. Putman, "What is 'Realism'?" *Pro. Artist. Soc.* 1975/6, pp. 177-194.

[2] W. V. O. Quine, "On the Reasons for the Indeterminancy of Translation" *Journal of Philosophy* 1970, 179.

[3] Putnam, H. "The Refutation of Conventionalism" in his *Mind, Language and Reality* (Cambridge University Press, 1975), 180.

[4] J. English, "Underdetermination: Craig and Ramsey" *Journal of Philosophy* 1973.

[5] R. Swinburne, *Space and Time* (Macmillan, 1968), 51.

[6] M. Dummett, *Frege: The Philosophy of Language* (Duckworth, 1973), 617fn.

[7] This assumption is argued for in my *The Structure of Time* (unpublished).

[8] W. Newton-Smith, "Closed Time" (forthcoming).

[9] A. Grünbaum, *Philosophical Problems of Space and Time* (Reidel, 1973), 197.

[10] E. G. Zahar, "Why did Einstein's Programme Supersede Lorentz's?" *Brit. Journal Phil. Soc.* 1973.

[11] Boyd (*op. cit.*) who has noted the tension between realism and underdetermination argues against the possibility of underdetermination. Reasons for rejecting his arguments are presented in my *The Rationality of Science* (Routledge, forthcoming).