The Epistemology of Geometry

CLARK GLYMOUR

THE UNIVERSITY OF OKLAHOMA

There is a philosophical tradition, going back at least to Poincaré ([9]), which argues that the geometrical features of the universe are underdetermined by all possible evidence, by all of the actual or possible coincidences and trajectories of material things, whatever they may be. Many different geometrical and physical theories can encompass the phenomena, can account for the motions of things. Poincaré supported his view with a parable, Reichenbach ([11]) with a sort of recipe for writing down alternative but empirically equivalent theories; later authors have repeated their arguments or given very similar ones. In his admirable book on space, time, and spacetime ([13]), Lawrence Sklar has tried to catalogue the possible philosophical attitudes towards the underdetermination arguments put forward by Poincaré, Reichenbach and others: One can simply be sceptical about the possibility of knowing geometrical truths; one can maintain that in so far as there are any such, they are truths by convention; one can contend that certain theories are *a priori* more plausible than others and so should win any ties based on empirical evidence; one can insist that despite appearances all empirically equivalent theories say the same thing; or one can deny that there is any coherent notion of empirical equivalence and so lay the entire question aside. What one cannot do, if this catalogue of options is complete, is to admit the notion of empirical equivalence, admit an account of sameness of meaning which permits that different theories may save the phenomena, deny that there are available *a priori* principles about what is most likely true, and still insist that arguments such as Poincaré and Reichenbach give do not establish the underdetermination of geometry. Of the options, listed and unlisted, I think this uncatalogued view is the one closest to the truth.

I will concentrate on Reichenbach's argument, for it is more explicit and more general. My thesis is that even when it is sympathetically developed, Reichenbach's sort of argument does not establish the underdetermination of geometry or of anything else. The same is true for any other arguments for the underdetermination of geometry which use devices like those employed by Reichenbach to generate alternative theories. Since I do not deny that Reichenbach showed how to construct different but empirically equivalent theories, my thesis is perhaps a little puzzling. My idea is that the body of evidence which distinct theories hold in common, the phenomena which both theories save, may nonetheless provide differing support for the two theories, more reason to believe one than the other, more confirmation of one than the other. This is so not because one theory is *a priori* more plausible or probable than the other but, roughly, because one theory is better tested than the other by the body of evidence in question. I think this is the case with most of the putative examples of underdetermination in geometry. My view, then, is that the arguments for the underdetermination of geometry fail because they succeed only in producing different theories which are empirically equivalent but which the imagined body of evidence does not equally well test or support. The particular view that evidence may discriminate among empirically adequate hypotheses without being inconsistent with any of them and without supposing that some of them are *a priori* likelier than others is not unprecedented. It is a view shared, I think, by Popper and by many Popperians; no doubt they would have little use for the rest of what I shall have to say about confirmation.

To address the question of the underdetermination of geometry requires several things. It requires a framework for the statement of alternative theories; it requires at least some necessary conditions for the synonymy of theories so expressed; it requires a characterization, however rough, of the states of affairs, actual or possible, which will serve as possible evidence; and it requires a theory of confirmation which will provide criteria for comparing and assessing competing theories. Most discussions of the underdetermination of geometry omit the specification of some of these elements; I shall try to make it clear, if not convincing, what I am supposing about these matters.

Space-Time Theories. Since arguments for underdetermination include relativistic theories, and since they involve geometry in the context of physical theory, I will just assume we are concerned with the underdetermination of features of space or of space-time in the context of space-time theories, whether classical or relativistic. I will assume that all such theories are formulated covariantly using a differentiable manifold and geometrical objects on that manifold, in order to state field equations relating sources to field quantities (such as the metric in general relativity or the gravitational potential in Newtonian theory) and to state equations of motion for material systems of various kinds. In addition there may be equations not containing source terms but which put restrictions on geometrical features of space-time. Further, the theories may contain principles not stated as equations which establish boundary conditions in certain situations or which establish symmetry properties (e.g., symmetric sources have symmetric fields) and so on. Such formulations are natural in the sense that actual theories are sometimes stated that way, they are reasonably clear, and we know how to write down a great many theories in such terms.[1] While any theory may imply, given a coordinate system, various coordinate dependent equations, and may sometimes be more easily tested in such a form, it is essential that the theory be stated covariantly; otherwise we become enormously confused about what each theory claims, about the synonymy of theories, and so on.

Synonomy. In dealing with formalized first-order theories there is a natural necessary condition for synonymy. First, identify any two theories if one can be obtained from the other simply by adding a predicate, but no new axioms, to its language. Second, make a lexicographic change in the theories so that no two theories considered have any non-logical vocabulary in common. Then count two theories as synonymous only if their transmogrifications have a common definitional extention. That is, we can add a set of sentences of definitional form to one theory (or rather to its transmog) and another set of sentences of definitional form to the other theory (or rather its transmog) and the two results are logically equivalent. This condition not only catches the intuition that synonymous theories are those that with proper substitutions say the same thing, it also satisfies natural conditions about translation.[2] A much weaker condition is that the theories have at least one model in common: For at least one model of theory 1 it is possible to define in this model the quantities of theory 2 so as to form a model of theory 2, and, conversely, from the model of theory 2 so formed it is possible to define the quantities of the original model of theory 1.

I propose to use the best analogs I can for these first-order requirements, and especially for the second one. For the synonymy of theories formulated as covariant equations we will require at least that for any manifold and set of geometrical objects on that manifold which constitute a solution to the equations of one theory there be covariantly definable from the geometrical objects of this solution another set of geometrical objects which, together with the manifold, constitute a solution to the equations of the other theory, and symmetrically. In other words, if theory 1 is synonymous with theory 2 then given geometrical objects A, B, C which are distributed on manifold M so as to satisfy the equation of theory 1, we can write a covariant set of equations in appropriate variables, such that for A, B, C as values of some of the variables there are determined on M a unique set of geometrical objects, X, Y, Z as values for the other variables, and X, Y, Z satisfy the equations of theory 2. Furthermore, the equations which constitute the implicit definitions must contain only variables and operations occurring in one of the two theories. The only regard in which this condition might seem in the least too strong is the demand that the equations used to get one set of objects from another set be covariant; but this demand is no less plausible than the demand of covariance in general. To insist that non-covariant equations may establish the determination of one set of objects from another set is in effect to suppose that each of the theories ascribes "true" coordinates in the world. Charity forbids.

Evidence. There are a variety of objections to the supposition that talk of empirical equivalence makes sense at all. In discussions of space-time theories the states of affairs, actual or possible, that are ordinarily cited as the fundamental evidence for theoretical claims are coincidences of material bodies and the trajectories of bodies in space-time. The first are but a special feature of the second. It can be objected that these states of affairs are not "observational", that theoretical principles of various kinds are involved somehow in their determination. That is probably true, but irrelevant nonetheless. It is not claimed that the body of phenomena to be saved comprise some epistemic rock bottom, nor need it be. It is claimed only that there are a collection of states of affairs that can be ascertained, at least approximately, independently of the assumption of the truth or falsity of any of the space-time theories in question, and that these states of affairs are those for which such theories must account. The other sort of criticism apt to be made is that the supposed evidence is the wrong kind of thing to be evidence. Theories, it is said, tell us nothing or next to nothing about the trajectories of bodies subject to specified forces, or subject to specified fields. Thus classical physics says nothing about the motions of bodies but only about the motions of bodies if subjected to various kinds of forces and to no others. But, the objection continues, we cannot infer the forces from the motions, and the forces are most definitely theoretical quantities. So there is no theory-independent body of evidence that can adjudicate between space-time theories.

To reply: First of all, in special circumstances we *can* infer the forces from the motions; Newton did so and so did a number of later Newtonians. Second, there are built into our theories various principles which establish presumptions as to the forces acting in various situations; for example, in Newtonian theory there is built the presumption that the only significant force determining the trajectories of the major bodies of the solar system is gravity. Such presumptions are not, and perhaps cannot be, laws, but they are an essential part of our theories nonetheless.

What is at issue is something like this. Bodies of various identifiable kinds move in certain ways; Newtonian theory, say, accounts for these motions by supposing a limited class of forces acting on the various bodies, and providing principles which serve as guides to which forces are working on which bodies. We wish to know whether these motions, in turn, provide grounds for believing Newtonian theory as against other possible space-time theories, and to answer the question we ask whether there are other space-time theories, which may suppose whatever forces and force-laws may be imagined, that account both for the actual motions and for any other motions Newtonian theory could account for. Now Newtonian theory must explain any motion as the result of the action of some finite number of kinds of fields, and any of its competitors, if we imagine them to be field theories, must do the same. If then, for some competitor it can be shown that for every possible combination of Newtonian fields there is a combination of the fields of some competing theory such that the respective combinations of fields determine the same motions, we will be well along in demonstrating the existence of empirically equivalent alternatives. If, furthermore, the alternative theories are such that Newtonian principles for determining the forces (or fields) acting in any situation can be parodied by principles of the alternatives, there will be nothing remaining to show. Such a parody will be possible generally only when there is some systematic connection between the fields of the alternative theory and the fields of Newtonian theory. However, for the theories to be genuinely distinct, the connection must not be so systematic that values of the Newtonian quantities are uniquely determined by values of the non-Newtonian quantities, and conversely. Newtonian theory, of course, is just an example; the issue is the same for any other space-time theory.

Confirmation. I propose to look at confirmation in the following way.[3] An hypothesis in a theory is tested positively by producing an instance of the hypothesis by a procedure that does not guarantee than an instance rather than a counter-instance will result. Since the quantities or states of affairs (hereafter, simply quantities) that can be determined without use of the theory are not all of the quantities of the theory, many hypotheses will contain quantities whose values are not found amongst the empirical data. Values for such quantities are obtained by *using* other hypotheses of the theory to compute from them values of the empirically accessible quantities. Any hypotheses in the theory may be used in such a capacity. So a test of an hypothesis consists of a set of values of empirically available quantities, a set of hypotheses of the theory which determine from this empirical data a set of values for various quantities of the theory, and an hypothesis of the theory for which these values constitute either an instance or a counter-instance. To give a trivial example, suppose we have a theory consisting of Newton's Second Law and Hooke's Law, and suppose we can measure, independently of this theory, length, acceleration and mass. Then we can test, say, the Second Law by measuring the mass, extention and acceleration of a spring, using Hooke's Law to determine the force on the spring, and then seeing whether or not the values of mass, acceleration and force so determined provide an instance of the Second Law. Equally trivial but more realistic examples of this strategy are provided by the use of data to determine arbitrary parameters in relations such as the gas laws and then using further data plus the parameter values to provide instances of the relations. It is essential that the computations of theoretical quantities from empirical ones not be so constructed that an instance of the hypothesis to be tested would result whatever the values of the empirical quantities might be. This does not prevent one from using the very hypothesis to be tested to determine values of certain theoretical quantities (one does that, legitimately, in curve-fitting), but it does prohibit using the hypothesis itself in certain ways.

The essentials of the strategy are ordinary enough that many people have hit upon them, but generally only to conclude that something must be wrong. Sneed (cf. [14]) and, following him, Stegmüller, for example, describe something very close to this strategy but claim that it cannot be correct because it is circular. The strategy is not circular at all, though it is a bit of a bootstrap operation. One claims that if certain principles of the theory are true, then certain empirical data in fact determine an instance of some theoretical relation, and moreover if the data had been otherwise a counter-instance of that relation would have been obtained. This is some reason to believe the hypothesis tested, but a reason with assumptions. Of course it is possible that the assumptions—the hypotheses used to determine values of theoretical quantities—are false and a positive instance of the hypothesis tested is therefore spurious, or a negative instance equally spurious. But this does not mean that the test is circular or of no account; it does not mean that the strategy can have no part in genuine scientific method. On the contrary, it is just this feature of the strategy which explains part of an important element of method, the demand for a variety of evidence. The hypotheses used in testing an hypothesis may themselves be in error, so that the instances or counter-instances obtained with them are spurious. The only means we have to guard against such error is to test the auxillary hypotheses used in computing theoretical quantities and to test the original hypothesis using a different combination of auxillary hypotheses to determine values of theoretical quantities. It is this need that determines part of what counts as a variety of evidence. One can see this aspect of evidential variety explicitly in some experimental research programs, for example in Jean Perrin's series of experiments to test the kinetic theory.[4]

The strategy is holistic in some ways and not in others. Given a definite theory, and some particular pieces of data, it will in general be possible to test some hypotheses of the theory from the data but not other hypotheses. That is because the structure of the theory may be such that there is no way to compute values of certain theoretical quantities from the data in such a way as to test some hypotheses of the theory. For example, from data about the positions of a single planet, Mars say, one can test, relative to the theory consisting of Kepler's three laws, Kepler's First and Second Laws, but one cannot test his Third Law. For using this theory, from data about one planet it is impossible to compute, independently, the period and orbital diameter of any *other* planet. Different hypotheses, then, may be tested by different data. It may even be that for certain bodies of possible data some hypotheses of a theory are not tested at all. It is exactly because of this non-holistic aspect of the strategy that the explanation just given of the demand for a variety of evidence makes sense. But the *assessment* of the hypotheses of a theory may be nearly holistic insofar as deciding to accept an hypothesis on the basis of the instances of it obtained involves making a decision about the truth of the auxillary hypotheses used in testing it, which truth in turn . . . . Still, one can easily construct theories which contain hypotheses that are not tested at all and that are not needed to test other hypotheses of the theory.

There are a number of features that we can use in an inexact way to compare how well theories are supported by evidence. I will mention the most important and obvious ones.

First, it is better that the hypotheses of the theory be confirmed rather than disconfirmed; if one theory contains hypotheses disconfirmed by a given body of evidence, while another does not, then other things being equal that is a reason for preferring the latter.

Second, one theory may contain untested hypotheses whereas its competitor does not; or in some appropriate sense one theory may contain more untested hypotheses than another. As a special case, two theories may share a common hypothesis which is tested by the evidence with respect to one theory but not tested by that same evidence with respect to the other theory. (Such is the case, for instance, with Copernican and Ptolemaic astronomies.)

Third, the evidence may be more various for one theory than for another. One theory may, for example, contain pairs of hypotheses, A and B, such that every test of B must use A (or hypotheses that imply A) and every test of A must use B (or hypotheses that imply B), whereas the competing theory does not have hypotheses so interdependent. Again, there are real examples. Before 1680, tests of Kepler's First Law had to use his Second, and tests of his Second Law had to use his First, and astronomers thought this a difficulty with his laws (cf. [17]).

Fourth, some or all of the evidence may have the following feature: It tests one hypothesis of one theory repeatedly, whereas in the other theory it provides fewer tests for a larger number of hypotheses. Informally, the body of evidence may be explained in a uniform way in one theory but have to be explained in several different ways in the second theory. We prefer the first.

Fifth, not all hypotheses in a theory are of equal importance—some are central, others peripheral. There is often enough an historical distinction of this kind, the peripheral hypotheses being those which have resulted from a process of modification to fit the data, the central hypotheses being those which are applied to the data to produce the modifications. In many cases we might expect the historical distinction to correspond to one or more logical distinctions. (Try, for example, this one: Central hypotheses of the theory are the members of the smallest deductively closed set S of hypotheses such that all hypotheses of the theory, or all confirming instances of hypotheses that can be obtained from the data are logically entailed by S together with the set of data statements.) In any case, if a large part of the evidence tests hypotheses that are peripheral to one theory but tests hypotheses that are central to another competing theory, that is reason to prefer the second theory.

These grounds for discriminating among theories could in principle lead us to prefer one from among several competing theories on the basis of a body of evidence explained by all of the theories in the group. Such preferences are not founded, or rather need not be founded, on *a priori* conceptions about how the world is or likely is; they are founded on the preference for better tested theories, and the various modes of comparison are only aspects of that preference. It is true that the principles of comparison are vague, and further that there is no principle given that determines which of these considerations take precedence should they conflict, or how they are to be weighted. (I doubt that there are any principles of this kind which are both natural and explain pervasive features of scientific practice.) I think there is rigor enough, however, to distinguish unambiguously among candidates that are offered in demonstration of the underdetermination of geometry.

One of the puzzling things about the literature on the underdetermination of space-time theories is how little notice its authors have given to the matter of how such theories have been tested. Testing of space-time theories has typically (though not exclusively) proceeded through planetary theory, and the bootstrap strategy, while it can be found throughout science, is pre-eminently a strategy for planetary theory. The differences between Ptolemaic and Copernican theory, and the relative advantages of the latter, are made evident by the strategy. Many of Kepler's arguments seem to involve it, and such elementary facts as that his evidence for the Second Law is founded almost entirely on observations of one planet, Mars, whereas his evidence for the third law is founded on observations of many planets, are explained by it. The difficulties which post-Keplerian and pre-Newtonian astronomers found in testing his First and Second Laws are unintelligible without it. Newton's argument for universal gravitation, certainly the most fundamental non-mathematical argument of the *Principia*, employs the strategy as a central component.[5] The differing values placed on the classical tests of general relativity are difficult to understand without the strategy. The strategy I have described is, I maintain, not only a good strategy, it is one that has predominated in the testing of space-time theories.

We have the elements for an assessment of arguments for the underdetermination of geometry. To make the criteria for theory comparison more definite and at the same time to illustrate why alternative theories of the kind that Reichenbach envisioned are not as good as the theories they are supposed to undermine, imagine a situation. Suppose you find yourself teaching high school physics, Newtonian mechanics in fact. Suppose further that a bright and articulate student named Hans one day announces that he has an alternative theory which is absolutely as good as Newtonian theory, and there is no reason to prefer Newton's theory to his. According to his theory, there are two distinct quantities, gorce and morce; the sum of gorce and morce acts exactly as Newtonian force does. Thus the sum of the gorce and morce acting on a body is equal to the mass of the body times its acceleration, and so on. Hans demands to know why there is not quite as much reason to believe his theory as to believe Newton's. What do you answer?

I should tell him something like this. His theory is merely an extension of Newton's. If he admits that an algebraic combination of quantities is a quantity, then his theory is committed to the existence of a quantity, the sum of gorce and morce, which has all of the features of Newtonian force, and for which there is exactly the evidence there is for Newtonian forces. But in addition his theory claims that this quantity is the sum of two distinct quantities, gorce and morce. However, there is no evidence at all for this additional hypothesis, and Newton's theory is therefore to be preferred. That is roughly what I should say, and I believe it is a natural thing to say; but then I am, I admit, in the grip of a philosophical theory.

The gorce plus morce theory is obtained by replacing "force" whereever it occurs in Newtonian hypotheses by "gorce plus morce", and by further claiming that gorce and morce are distinct quantities neither of which is always zero. In general, a test of Newtonian hypotheses—for example the simple test of Newton's Second Law using Hooke's Law described earlier—will not be a test of the corresponding gorce plus morce hypothesis

That is because the computations which give values for force will not give values either for gorce or for morce, but only for the sum of gorce and morce. Indeed, in general if we have a set of simultaneous equations such that using these equations, values for some of the variables in the equations may be determined from values of other variables, if each of the former variable are replaced systematically throughout the equations with an algebraic combination of two or more new variables, then values for the new variables will not be determined. If to the gorce plus morce theory we add the hypothesis that force is equal to the sum of gorce plus morce, then the theory, with this addition, entails Newtonian theory and every test of Newtonian theory is a test of the identical fragment of the expanded gorce plus morce theory. But there are no tests of the hypothesis that force equals the sum of gorce plus morce, nor are there any tests of those hypotheses that contain "gorce plus morce" in place of "force". The bootstrap strategy, then, gives formally what I should say informally. No surprise there.

My thesis is that the theories advanced to demonstrate the underdetermination of geometry bear a relation to ordinary theories very much like the relation the gorce plus morce theory bears to ordinary Newtonian theory, and are inferior for much the same reason. Implicit in the discussion is a certain articulation of the principle that we prefer a theory with fewer untested hypotheses to one with more untested hypotheses. Suppose there are two theories, T and Q, such that there are a set of axioms A of definitional form and T&A entail Q but there is no set of axioms B of definitional form such that Q&B entail T. Further suppose that every test of T&A from some body of evidence is a test only of hypotheses in Q. Then Q is better tested by that body of evidence than is T. Intuitively, T has whatever untested stuff Q has plus some more. This principle, though rather weak and applicable to only a very limited number of cases, enables us to see how some apparent cases of underdetermination are not that at all. Let us consider a case that might be taken to illustrate the underdetermination of affine geometry in the context of classical physics.

One formulation of Newtonian gravitational theory uses as geometrical objects two scalar fields, the mass density ρ and the gravitational potential ϕ. In addition, there is a scalar field t, the absolute time, and a (2, 0) singular tensor field representing the metric, and, finally, an affine connection compatible with the metric. The field equations in component form are

1) $R^{i}_{jkl} = 0$

2) $t_{i;k} = 0$ where $t_i = \dfrac{2t}{2x^i}$

3) $g^{ir}_{;k \cdot k} = 0$

4) $g^{ik} t_i t_k = 0$

5) $g^{ik} \phi_{;i;k} = 4\pi\kappa\rho$

where the semi-colon signifies covariant differentiation with respect to the index following it and, as usual, repeated indices are understood to be summed over. The equation of motion is

6) $$\frac{d^2 x^i}{dt^2} + \Gamma^{i}_{jk} \frac{dx^j}{dt} \frac{dx^k}{dt} = - g^{ir} \phi_{;r}$$

where the Γ^{i}_{jk} are the Christoffel symbols of the connection.

It is easy to prove that there are inertial coordinates in which the time scalar functions as one coordinate, and the components of the metric tensor are constant and the matrix of components of the tensor are $(g^{ij}) = \text{diag}\ (0, 1, 1, 1)$—see [2]. In such coordinates, equation 5 becomes just Poisson's equation:

$$\nabla^2 \phi = 4\pi\kappa\rho$$

Consider how how well Newtonian theory, so formulated, is tested by the kind of evidence generally in mind. Assume then, as part of the theory, that ordinary rigid rods determine congruences according to the metric and that mechanical clocks measure the absolute time function at least approximately. So we may take as data such congruences, time intervals and the trajectories of freely falling bodies. The question is what parts of Newtonian theory are tested by such data. On the account of testing given earlier, the answer depends on what other quantities can be determined uniquely from such data by means of the theory itself, and in what ways such determinations can be carried out. By a model of Newtonian gravitational theory let us mean a tuple $\langle M, g, t, \rho, \phi, \Gamma, F \rangle$ where M is a four dimensional differentiable manifold, g a (2, 0) metric field, t, ρ, and ϕ scalar fields, Γ an affine connection and F a family of time-like trajectories on the manifold, such that these objects satisfy the equations of the theory if F is taken as the collection of free falls. Our question can be answered in part by asking whether such quantities as the affine connection and gravitational potential are uniquely determined in some models or in every model by g, t, ρ and the family of trajectories. The answer is that they are not so determined, not in any model. Let $\langle M, g, t, \rho, \phi, \Gamma, F\rangle$ be a model of the theory. Choose three linearly independent constant vector fields ${}_k\mathsf{U}^a$ $(k = 1, 2, 3)$ such that ${}_k\mathsf{U}^a t_a = 0$ and let $f_k(t)$ $(K = 1, 2, 3)$ be any three scalar fields which are constant on each constant time hypersurface. Denoting $g^{ar}\phi_{;r}$ by ϕ^a, define

$$\psi^a = \phi^a + f^k(t)_k \mathsf{U}^a$$

$$°\Gamma^a_{bc} = \Gamma^a_{bc} - f^k(t)_k \mathsf{U}^a t_b t_c$$

Then Trautman (see [16]) has shown that $\langle M, g, t, \rho, °\Gamma, \psi, F\rangle$ is a model of the theory.

The connection and the potential are not determined by the other quantities in the theory. Although an ordinary Riemannian connection is determined by an ordinary Riemannian metric, the metric in this case is singular and so fails to determine a unique compatible connection. Another way to put this indeterminacy is that the metric, time and trajectories of free falls do not determine the class of inertial frames.

The upshot is that because the affine connection cannot be determined from the phenomena, not even using the theory, and functions of the connection, such as the curvature tensor, cannot be determined either, many of the equations of Newtonian theory cannot be instantiated in a way that tests them; the theory can be tested in hypothetico-deductive fashion, but that is a fashion different from the one described above. It may occur to some that the theory contains undeterminable quantities only because it is incomplete. Perhaps the indeterminacy arises because, lacking boundary conditions, we cannot get a unique solution of Poisson's equation. If one adds to the theory the natural condition that at infinite distances from sources the gravitational potential vanishes, then it seems the potential and hence the connection will be uniquely determined (because, at arbitrarily large distances from sources, ψ^a must vanish, so $f^k(t)$ must vanish, but $f^k(t)$ is constant throughout space). Even then, however, the most one can say is that in *some* models of the theory the potential is determined. "Full" models, those in which there is an upper bound on the distance between particles, will not determine the potential; in particular every non-empty model of the theory in which the constant time hypersurfaces are compact will leave the potential and connection undetermined.

Consider now another theory, one which permits the very same trajectories as does the Newtonian theory. Save for the absence of the gravitational potential, the new theory has the same kinds of objects as does Newtonian theory, but the field equations and equations of motion are different and some of the objects, the connection in particular, behave differently.

1*) $t_{\{p} {}^{o}R^{i}{}_{j\}kl} = 0$

2*) $g^{ip}\, {}^{o}R^{j}{}_{kpl} = g^{jp}\, {}^{o}R^{i}{}_{lpk}$

3*) $t_{i;k} = 0$

4*) $g^{il}{}_{;k} = 0$

5*) $g^{il} t_i t_l = 0$

6*) ${}^{o}R_{lk} = -4\pi\rho t_l t_k$

are the field equations, and the equation of motion is

7*) $$\frac{d^2x^i}{dt^2} = {}^{\circ}\Gamma^{i}_{lk} \frac{dx^l}{dt}\frac{dx^k}{dt} = 0$$

${}^{\circ}R$ and ${}^{\circ}\Gamma$ signify the curvature and Christoffel symbols of the connection. The brackets indicate antisymmetrization with respect to the indices between them. In this theory, the affine connection is a dynamical object determined by the distribution of matter through equation 6* which is just the analogue of Poisson's equation. The equation of motion, 7*, says that the trajectories of free falls are geodesics of the connection. The Newtonian gravitational potential has, in effect, been geometrized away.[13]

Consider how well the new theory is tested by the same data we considered before. Equations 3*, 4*, 5*, like their Newtonian analogues, imply the existence of inertial coordinates in which spatial components of the connection vanish. So those equations expressed by 7* which have $i \neq 0$ can be tested by determining features of trajectories and congruences. Further, equation 7* says that free falls are geodesics and that the time is an affine parameter. The geodesic spray of a connection and an affine parameter uniquely determine the connection (cf. [1]). The connection can therefore be determined from trajectories by using 7* and thus various of the field equations (e.g., equations 6* and 1*) can be instantiated in a way that tests them.

Informally, it seems clear that the confirmation principles described earlier imply that the second theory is better tested than is the first. But we can here apply the more precise principle developed in the gorce plus morce example. To the first theory we add axioms of definitional form so that the second theory is entailed. But the procedure is not symmetrical. The only object of the second theory that behaves differently from its Newtonian analogue is the connection. If to the equations of Newtonian theory we add

$$^{\circ}\Gamma^{i}_{lk} = \Gamma^{i}_{lk} + \phi_{;r} t_l t_k$$

then all of the equations of the second theory follow. It does not work in the other direction. We can show that given $\langle M, g, t, {}^{\circ}\Gamma, \rho, F\rangle$ satisfying the second theory, there exists a scalar ϕ and connection Γ such that $\langle M, g, t, \Gamma, \rho, \phi, F\rangle$ is a model of the first theory, but we cannot *define* Γ or ϕ from $\langle M, g, t, {}^{\circ}\Gamma, \rho, F\rangle$. The two theories do not say the same thing. The relations between the Newtonian theory and the alternative theory with a dynamical connection are exactly like the relations between the gorce plus morce theory and the force theory. Only this time we are dealing with rather more realistic examples. While both theories account for the imagined phenomena, the testing strategy described earlier provides clear reasons for preferring one of these theories to the other on the basis of that body of phenomena.

There are other examples of this kind of relation between competing theories, or of something very close to it. There are many special relativistic theories of gravitation, theories which ascribe to space-time the Minkowski metric, unaffected by the distribution of matter, energy, or momentum, and which treat gravity as a field distinct from the metric field. For various reasons, only those special relativistic gravitational theories which treat the gravitational field as a tensor field have a hope of being empirically adequate. To exaggerate slightly, what many of these flat space-time theories of gravitation do is to divide the dynamical metric field of general relativity into a fixed Minkowski metric and a gravitational field tensor which, of course, is not fixed but dynamical, that is, dependent on the distribution of matter and radiation. If that were exactly what they did, then the relation between such theories and general relativity would be just like the relation between the gorce plus morce theory and the force theory. In fact, the situation is usually a little more complicated. Such theories may, for example, take the metric field of a particular solution or class of solutions of the field equations of general relativity, divide the metric into a Minkowski metric and a gravitational field tensor, and write down new field equations satisfied by these objects. That is just what happens in a flat space-time gravitational theory due to W. Thirring ([15]). So far as testing is concerned, the results are generally very much as in the Newtonian case already considered; the special relativistic metric and the gravitational field tensor cannot be determined uniquely, and the field equations, unlike their general relativistic analogues, cannot be instantiated. Thus the authors of a relativity textbook say of Thirring's theory:

> ... there exists a transformation of the potential which leaves all observable quantities unchanged, but which changes the rate of flow of time and the rates of clocks as expressed in terms of "absolute time" ....
>
> Thus a fully developed RTGFS (Relativistic Theory of Gravitation in Flat Space) which agrees with GTR (General Theory of Relativity) in the first corrections to Newtonian theory, in order to explain the universality of the action of gravity, is forced to employ the unphysical hypotheses of an unobservable "absolute" time, and of the influence of unobservable quantities—e.g., the gravitational potential—upon all physical processes. ([18]: 69-70).

For "unobservable" in this passage, read "undeterminable". Thirring himself says much the same thing.

R. Sexl ([12]) has claimed that general relativity says the same thing as Thirring's theory. The sole basis for this claim is that a tensor satisfying Einstein's equations for the metric tensor can be defined from quantities in Thirring's theory. But this is far from sufficient, since the special relativistic metric of

Thirring's theory cannot be defined from general relativistic quantities. A quantity satisfying Einstein's equations might be definable from Thirring's theory even if Thirring's theory were inconsistent. (In fact, Thirring's theory *is* inconsistent. Cf. [7]: 186.)

Reichenbach's argument for the general underdetermination of geometry goes like this. Suppose you have an empirically adequate physical theory in which the metric tensor is g and bodies subject to no forces move (or would move) on geodesics of g. Let the theory postulate whatever other fields are necessary, and let g be measured in whatever ways are appropriate, e.g., by congruences of rigid rods. Form an alternative theory as follows. Replace g by any metric you like, call it h, so long as h meets certain topological constraints. Introduce a "universal force" U such that h plus U equals g, and specify that every body is subject, always, to the universal force U, so that bodies subject to no other forces or fields would move on geodesics of the tensor h plus U. Let the other fields and forces, and the criteria for determining where they are acting, be just as in the first theory. Then the two theories should be empirically equivalent and the choice between them underdetermined by all possible evidence.

It is not entirely clear whether Reichenbach meant to be arguing in the context of classical physics, relativistic physics, or both. I believe his arguments have been widely understood to apply to both contexts, and to show that the geometry of space in classical theory and the geometry of space-time in relativistic theory are equally underdetermined. In relativistic contexts, the "universal force" must be a "universal field" but other than that, the argument is most clear for relativity. It tells us, for example, that to obtain a theory equally as good as general relativity we need only replace the general relativistic metric by any other metric we choose and add a "universal field", i.e., a gravitational field tensor. We have seen already, however, that this is exactly the strategy pursued in certain flat space-time theories, and that the result is *not* a theory as well tested as general relativity. In its most direct application, Reichenbach's argument simply fails if we employ the account of confirmation discussed earlier.

What about classical physics? In the context of Newtonian theories it is less clear what a universal force might be, but we can work backwards to arrive at an account. In Newtonian theory with ordinary Euclidean geometry, particles subject to no forces, were there any, would have to move on geodesics of the connection, that is, subject to the equation

$$\frac{d^2x^i}{dt^2} + \Gamma^i_{lk}\frac{dx^l}{dt}\frac{dx^k}{dt} = 0$$

With a non-Euclidean geometry, the motion of particles must be such that if, *per impossible*, they were not subject to a universal force they would move on geodesics of the connection. The actual covariant acceleration then, that is

$$\frac{d^2x^i}{dt^2} + {}^*\Gamma^i_{lk}\frac{dx^l}{dt}\frac{dx^k}{dt}$$

should be equal to the universal force F^i acting on them. So the equation of motion should be

$$\frac{d^2x^i}{dt^2} + {}^*\Gamma^i_{lk}\frac{dx^l}{dt}\frac{dx^k}{dt} = F^i$$

Hence the universal force must be

$$F^i = ({}^*\Gamma^i_{lk} - \Gamma^i_{lk})\frac{dx^l}{dt}\frac{dx^k}{dt}$$

A universal force, then, is feature of the difference in the affine properties of two geometries, and need not involve the metrics directly at all. (Cf. [7]: 186.)

Looking back at the relation between the version of Newtonian theory with a fixed affine connection and the second version, that with a dynamical connection and no potential, it is clear that the former theory is just the latter *with a universal force*. The universal force in that case is $-g^{ir}\phi_{;r}$, i.e., just the gravitational force that enters the equation of motion of the first version. And again, we have already seen that while these two theories may equally save the phenomena, they are not equally well tested or supported by the phenomena.

The examples we have already considered show that Reichenbach's argument is invalid, and that his strategy for generating alternative theories need not result in pairs of theories equally well-tested by the phenomena imagined. Considering classical theories which have, besides different connections, different metrics, does not change things at all. Thus if the alternative theory postulates, say, a hyperbolic metric we should expect it to contain field equations such as the following

$$h^{ik} t_i t_k = 0$$
$$h^{ik}_{;l} = 0$$
$$t^i_{;l} = 0$$
$$R_{abcd} = K(Y_{ad} Y_{bc} - Y_{ac} Y_{bd})$$
$$K = -1$$

where h is the singular metric tensor and Y the metric induced on any constant time hypersurface. For the equation of motion of particles subject to no "differential forces" we should have

$$\frac{d^2 x^i}{dt^2} = \overline{\Gamma}^i_{lk} \frac{dx^l}{dt} \frac{dx^k}{dt} = F^i$$

where F^i *is the universal force. The relation between this theory and Newtonian theory is very much the kind we have seen* before. Although the Euclidean metric cannot be defined from this theory, the standard connection, Γ, can be by

$$\Gamma^i_{lk} = \boldsymbol{\Gamma}^i_{lk} - F^i \frac{\partial t}{\partial x^l} \frac{\partial t}{\partial x^k}$$

and with this definition (and defining time by time, etc.) the geodesic equation of motion and those field equations of Newtonian theory that do not involve the metric all follow.

An obvious reply to the criticisms I have offered is that the alternative theories described are incomplete, and that more complete versions of them will be equally well tested by the phenomena. The cause of the underdetermination of geometry, allegedly, is that while geometry is supposed to deal with properties of space or of space-time itself, the evidence for a geometry must always be provided by what is material; different assumptions about the connections between geometrical quantities and material systems lead to different geometrical theories. What the theories described so far have left out, the reply continues, is the new assumptions connecting universal force with material systems, or the new metric with material systems, assumptions which would make the universal force term occurring in a theory determinable, and which would, therefore, permit the testing of various equations in such theories.

I think the reply fails, and does so because it does not take the testing strategy described earlier seriously enough. In the first place, if the demand of covariance be satisfied, the kinds of principles envisioned require that the theories be made rather more complex. Whereas ordinary rods measure, at least approximately, Euclidean congruences or distances, there are no natural relations of material things that can be used to measure, even approximately, distances according to an arbitrary metric. The usual way philosophers have described an alternative metric, therefore, is in terms of functions of some particular set of coordinates, generally Cartesian coordinates such that coordinate differences equal Euclidean distances; the procedure in mind, apparently, for determining the non-Euclidean metric is that one uses material systems to set up Cartesian coordinates and then evaluates the metric as a function of the coordinate description of position. I have no objection to this method of determining a non-Euclidean metric, for it is often the case that we must use some system of coordinates to test hypotheses. But to make the *statement* of the theory independent of coordinates one must in the theory introduce suitable scalar fields (essentially the spatial coordinate fields) and, furthermore, one must introduce the hypothesis that the metric is a suitable function of these scalar fields. And one must claim that material systems, e.g., rigid rods, measure these scalar fields or some feature of them—just as one claims that clocks measure a feature of the time scalar field. With these additions an alternative theory such as the hyperbolic theory described above is covariant and its metric is determinable.

Is such a theory really as good as the ordinary one with ordinary geometry? Introducing a set of hypotheses that link the metric with material systems may make testable various equations, some of the field equations for example, involving the metric. But the equation of motion, since it involves a universal force term, will still not be tested, nor will any equation that involves covariant differentiation. To test these hypotheses, the theory must be expanded still further, and in such a way as to make the universal force term determinable. Perhaps the universal force can be specified as some function of the (coordinate) scalar fields, very much like the metric. The result, one expects, must still be a theory that is less well tested than is the ordinary theory. For while all of the equations written down earlier should be testable in this expanded theory, to test them we will have had to introduce an enormous body of claims in order to permit the determination of the metric, connection and universal force; these claims will either be untested, or else the claims involved in the determination of the metric, say, will only be testable by using the claims that are involved in the determination of the connection and universal force. The theory will be inferior, then, either by reason of untested hypotheses, or by reason of an insufficient variety of evidence.

Suppose it is said that, after all, there is the same kind of interdependence of hypotheses in the ordinary theory. The ordinary theory claims that clocks measure a function of the time scalar, and that rigid rods measure the congruences of the metric—aren't these the same kind of "coordination principles" that the non-Euclidean theory uses? The answer is that they are, but the non-Euclidean theory requires a lot more of them and at best will permit one of them to be tested only by using a lot of others. The case of time is common to all theories we are considering in a Newtonian context. The Euclidean theory must claim that rods (or something) measure congruences; the non-Euclidean theory must claim that there are various scalar fields, that rods (or something) measure functions of these fields, that the metric is a certain function of these fields and, furthermore, that the universal force is another function of these same fields. Nothing less will do. These are all claims which we want tested, and tests of any of them will necessarily involve using all of the others.

It might be said, though not quite accurately, that all I have done is to argue that non-standard theories must have more theoretical content than standard ones, that non-standard theories go farther beyond the data. But if that is an objection to the non-standard theories in comparison to the standard theory, why isn't it equally an objection to the descriptions of actual or possible trajectories and so on? Why not just say: "The possible trajectories are such and such"? Why have theories at all? The answer, I believe, is that actual data on trajectories, etc., can provide better tests of, and better support for, a *theory* of the possible trajectories than it does for the simple claim that the possible trajectories are those the theory claims to be possible. I imagine the latter "simple" claim to be a set of hypotheses of the form: "With such and such initial conditions, the motions satisfy the equation ____ " where the blank is to be filled by some non-differential equation in coordinate variables and time, giving the position of a body in the system as a function of time. Clearly, the set of hypotheses of this kind is not going to be finitely axiomatizable. Regard this enormous set of claims as a theory to be tested like any other, and to be compared in particular with the dynamical theory that gives these trajectories as the possible ones. The data, we may suppose, consist of statements as to the locations of various bodies at various times. The data may very well test the dynamical theory better than it tests the set of claims about the trajectories, and for several reasons. Various principles of the dynamical theory may be tested over and over by different pieces of data, whereas data incorporating different sets of initial conditions will generally test but one of the hypotheses explicitly about trajectories. On any finite body of evidence, it will be the case that most of the trajectory hypotheses are untested, but that same data, while not testing every hypothesis of the dynamical theory, may nonetheless test a body of hypotheses of the dynamical theory sufficient to entail every hypothesis of the theory. We may, then have sufficient grounds to accept the dynamical theory even when we would not have anything like sufficient grounds to accept merely that the possible trajectories are those the theory permits.

What if we consider not statements about trajectories but rather just the statements of the data itself. "Body B was at place P at time t." Is not the body of such data claims, whatever they may be, better warranted than the theory, and so do not the principles I used to reject the non-Euclidean theory require that I reject all theories? Clearly not. First, because the data claims are not a theory and cannot serve the purposes of theories, in particular cannot serve for prediction or retrodiction. Second, because while the data as a whole may be better warranted; once we admit that what we call data is no incorrigible body of claims but only claims obtained by methods, whatever they may be, that we believe reliable, it becomes possible that a theory be better supported, better warranted, than some particular piece of evidence for or against it. If data are corrigible, the theory tested and confirmed by many pieces of evidence may sometimes reasonably be preferred to a datum that conflicts with it. Curve-fitters make such decisions every day.

I think, then, that the criteria by which I have denigrated non-standard theories of space or of space-time do not require me to denigrate all theories. The sorts of theories Reichenbach and many others have suggested, whether they be understood in classical or in relativistic contexts, are just not as good as the theories they are supposed to prove to be underdetermined. It is still possible, of course, that space-time theories are as radically underdetermined as Poincaré, Reichenbach and others have believed, but that the alternative theories are simply very different from the kind Reichenbach and subsequent writers envisioned[6]. Even if it is true that our space-time theories are not radically underdetermined, there may still be features of space-time that are underdetermined. There may be quantities or properties which for whatever reasons we believe our theories rightly ascribe to space-time, but which we cannot determine. I have suggested elsewhere ([3]) that in certain cosmological models the global topology of space-time may be such a feature. Perhaps there are others as well; whatever may turn up, it is bound to be more palatable than is the radical underdetermination of geometry.

REFERENCES

[1] R. Bishop and S. Goldberg, *Tensor Analysis on Manifolds* (New York: Macmillan, 1968).

[2] Michael Friedman, "Foundations of Space-Time Theories" 1972 PhD Dissertation, Princeton University.

[3] Clark Glymour, "Indistinguishable Space-Times and the Fundamental Group," in *Minnesota Studies in the Philosophy of Science*, Vol. 8, edited by J. Earman, C. Glymour, and J. Stachel (Minneapolis: University of Minnesota Press, forthcoming).

[4] ——, "Relevant Evidence," *The Journal of Philosophy*, LXII (1975).

[5] ——, "Physics and Evidence," in *Pittsburgh Series in Philosophy of Science*, ed. by L. Laudan, forthcoming.

[6] ———, "Theoretical Realism and Theoretical Equivalence," *Boston Studies in the Philosophy of Science*, Vol. 8, edited by R. Buck (Dordrecht: D. Reidel, 1971).
[7] C. Misner, K. Thorne, and J. Wheeler, *Gravitation* (San Francisco: W. H. Freeman, 1973).
[8] M. J. Nye, *Molecular Reality* (American Elsevier: 1972).
[9] H. Poincaré, *Science and Hypothesis* (New York: Science Press, 1905).
[10] H. Putnam, "The Refutation of Conventionalism," NOÛS VIII(1974): 25-40.
[11] H. Reichenbach, *The Philosophy of Space and Time* (New York: Dover, 1953).
[12] R. Sexl, "Universal Conventionalism and Space-Time," *General Relativity and Gravitation*, Vol. 1 (1970).
[13] L. Sklar, *Space, Time and Space-Time* (Berkeley: University of California Press, 1974).
[14] J. Sneed, *The Logical Structure of Mathematical Physics* (Dordrecht: D. Reidel, 1971).
[15] W. Thirring, "An Alternative Approach to the Theory of Gravitation," *Annals of Physics*, Vol. XVI (1961): 96-117.
[16] A. Trautman, "Foundations and Current Problems in General Relativity," in *Brandeis Summer Institute in Theoretical Physics, 1964* (New York: Prentice-Hall, 1965).
[17] C. Wilson, "From Kepler's Laws, So-called, to Universal Gravitation," *Archive for the History of Exact Sciences*, Vol. VI (1969): 89-170.
[18] Y. Zeldovich and I. Novikov, *Relativistic Astrophysics* (Chicago: University of Chicago Press, 1971).

NOTES

*This research was supported, in part, by NSF Grant GS41764.

[1] An excellent discussion of covariant formulations of space-time theories is available in [2].

[2] An elaboration and defense of these conditions is given in [6].

[3] The ideas about confirmation sketched here are given in more detail in [4].

[4] An excellent history of Perrin's work is given in [8].

[5] A detailed discussion of how the strategy applies to Ptolemaic and Copernican theories and to Newton's argument for universal gravitation is given in [5].

[6] In a preprint of [10], Hilary Putnam argues that non-standard theories can be generated without using "universal forces" by positing an appropriate "interaction force" between differential forces. I do not see how this suggestion can work unless the theory to be undermined already contains two universal forces which are like gravitation in entering the equation of motion without parameters. But a theory with two such forces would already be objectionable for reasons discussed.