Explanations, Tests, Unity and Necessity

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The fact that a scientific hypothesis explains a phenomenon is supposed to be a reason for believing the hypothesis, and, indeed, at least sometimes a special reason over and above considerations of empirical adequacy.[1] Copernicans may have claimed that their theory gave a more accurate accounting of the motions of the planets than did the traditional Ptolemaic astronomy, but the real weight of their argument came from the claim that, even if both systems could save the phenomena, the Copernican theory offered the better explanations.[2] If explanation sometimes provides a special reason for belief in a theory, over and above empirical adequacy, then explanations must, it seems, do something more to the phenomena, or say something more about the phenomena, than merely entail their description. My first question is what more it is that explanations do. There is no single answer because there is no single extra thing that explanations accomplish; shortly, I will try to describe two of the things that I think some explanations do, and in virtue of which we give credence to theories: they eliminate contingency and they unify.

Explanations are sometimes contrasted with tests. A phenomenon that a theory explains is not always counted as a test of the theory; no one is surprised to read criticisms of psychoanalysis, for example, which admit that the theory explains things, but denies that the things explained constitute tests of psychoanalytic hypotheses. A test of an hypothesis is at least some occurrence that could have been otherwise, and that, had it been otherwise, would have been reason not to believe the hypothesis. In this minimal sense of "test," traditional confirmation theories—the theories we have from Carnap,[3] from Hempel,[4] and the many varieties of hypothetico-deductivism—are theories of testing. They differ, of course, in their assessment of the relations required between theory and evidence in order for the evidence to be a test of the theory, but they are at one in sensing a distinctive kind of reason for belief provided by tests, and in attempting to analyze that kind of reason. So we have, *prima facie*, two different kinds of reasons for belief in scientific theories: reasons provided by the explanations the theories have provided, and reasons provided by the tests the theories have survived. Very often, perhaps typically, we expect that these sorts of reasons go together and that our theories will explain the outcomes of tests of them. One would like, then, to know what the connection is between explanations and tests, and why they often but not always accompany one another. The question can only be answered if one has in hand an adequate theory of explanation and an adequate theory of confirmation, and it can only be answered in part if one has at least parts of such theories. I will try to answer it, on the one hand, with regard to the two patterns of explanation mentioned in the first paragraph, and, on the other hand, with regard to my own views about confirmation.[5]

One way in which our wonderment about a phenomenon can be relieved is through a demonstration that it is necessary, that it could not be otherwise. One way, perhaps the most complete way, to explain the ideal gas law is to show that it just is not possible for a gas to have pressure, volume and temperature other than as the gas law requires. Philosophers of science nowadays sometimes recognize the explanatory force of eliminating the appearance of contingency, and even trace it back to the Aristotelian notion of "knowledge of the reasoned fact", but the modern philosophical temptation is to change the subject and to treat explanation not as a demonstration of the necessity of the phenomenon, but instead as a demonstration that a description of the phenomenon is a necessary consequence of some hypothesis. I think modern science often explains regularities by showing that they are necessary, not just that they are necessary consequences of theories. My idea is that certain regularities are explained by identifying the properties they concern with other properties in such a way that a statement of the original regularity is transformed into a logical or mathematical truth. The statements identifying properties are, if true, necessarily true,[6] and so the original, apparently contingent regularity is transformed by necessary truths into a necessary truth.

A simple example comes from electrodynamics. Two of the Maxwell-Lorentz equations concern the relations between electric and magnetic fields: these "internal" field equations run

(1) $\text{div } B = 0$

(2) $\text{curl } E = -\frac{1}{c}\frac{\partial H}{\partial t}$

If the scalar potential ϕ and the vector potential A are introduced and it is claimed that the electric field intensity E and the magnetic field intensity H just *are*, respectively,

(3) $E = -\text{ grad } \phi - \frac{1}{c}\frac{\partial A}{\partial t}$

(4) $B = \text{curl } A$

then by substituting for the quantity B the quantity curl A in equation (1) we obtain

(1*) $\text{div curl } A = 0.$

Now the divergence of a curl is identically zero; that is, equation (1*) is a mathematical identity. Again, if we substitute for the quantity E in equation (2) the quantity $-\text{grad } \phi - \frac{1}{c}\frac{\partial A}{\partial t}$ and we again substitute curl A for B we find that equation (2) becomes

(2*) $-\text{ curl grad } \phi - \frac{1}{c}\text{ curl}\frac{\partial A}{\partial t} = -\frac{1}{c}\text{ curl}\frac{\partial A}{\partial t}.$

Equation (2*) is also a mathematical identity since the curl of a gradient is identically zero.

The general pattern should be clear. An apparently contingent truth T makes reference to properties P, Q and R. T is explained by identifying P with P′, Q with Q′, R with R′ and so on, where P′, Q′ and R′ will not necessarily be single quantities but may instead be combinations of new quantities. On substituting "P′" for "P", and "Q′" for "Q", and so in in T, T is transformed into a logical or mathematical truth, T′. T is shown to be a necessary consequence of necessary truths, and so is necessary itself. Explanations of this form are especially complete and satisfying, since they leave behind only philosophical questions about the nature of necessity and of logical truth. They leave no further questions that ordinarily fall within the compass of science. Compare such explanations with the explanations of Kepler's laws in Newtonian celestial mechanics. The latter explanations only raise further questions about why Newtonian celestial mechanics is true, since its laws are not obviously necessary truths. But when an explanation proceeds by the pattern illustrated, then there is nothing about the hypotheses used in the explanation that requires explanation in turn. Mathematical and logical truths require no scientific explanation, and neither do claims asserting the identity of properties. One can ask, for example, why temperature and mean kinetic energy have concommitant values for gases, and imaginable answers provide explanations for the concommitance. One might postulate a Rube Goldberg apparatus that serves to keep the mean kinetic energy of a gas in tow with its temperature. But if one gives a Rube Goldberg explanation of the concommitance we can still ask for an explanation of features of the apparatus: what makes the bald man slip on the banana peel? By contrast, if the temperature of gases just *is* their mean kinetic energy then the explaining is over. There may be arguments to the conclusion that temperature is mean kinetic energy, but there are no explanations of the fact.

There are many examples of the sort of explanation I have described. I think that part of the renewed appeal that general relativity gave to W. K. Clifford's idea that matter just is curved space derived from the fact that if matter just *is* curved space-time then if general relativity is true then it is necessarily true. For then the field equation of general relativity states an identity of properties, and so if true is necessarily so, and the equation of motion of the theory, since it is a consequence of the field equation, is also necessary.[7] Indeed the general relativistic field equations, construed as an identification of quantities, affords an explanation of the general relativistic equation of motion exactly in the way that equations (3) and (4) above afford an explanation of the internal Maxwell equations. Again, if one considers the kinetic explanations of the ideal gas law, it seems that the gas law is transformed, by the appropriate identifications of properties, into a mathematical truth. Examples are only limited by my ignorance; I am sure they are to be found in virtually every subject in the exact sciences. There is one more example that I must give in some detail, both because it illustrates some points about confirmation that I will talk about below, and because it concerns the difference between the sort of explanation in question and mere empirical adequacy.[8]

Ptolemy discovered a useful but curious regularity about the motion of the superior planets, a regularity which he did not claim to be able to explain. A solar year is the period required for the sun to move 360 degrees along the ecliptic; that is, the period between its successively taking up the same position with respect to the fixed stars. A revolution of longitude is the analogous period for a superior planet. Superior planets are sometimes in opposition from the sun; that is, they appear 180 degrees, or half-way round the celestial sphere, from the sun. A cycle of anomaly is the period between two successive oppositions of the same superior planet. The regularity that Ptolemy presents is as follows:

> If a superior planet goes through a whole number of revolutions of longitude while going through a whole number of cycles of anomaly in a whole number of solar years, then the number of solar years is equal to the number of revolutions of longitude plus the number of oppositions.

Copernicus had a neat explanation of this regularity, though Ptolemy had none. According to Copernican theory, the apparent movement of the sun along the ecliptic really is just the projection of the Earth's motion in a closed path about the sun, and a solar year is just the period of one revolution of the Earth about the sun. A superior planet is one which moves in a closed orbit about the sun, wholly containing the earth's orbit, and a revolution of longitude is just the period required for the planet to make one revolution in orbit. An opposition just is a collineation of the Sun, Earth, and the superior planet, with the Earth between the other two bodies. With these identifications, Ptolemy's regularity becomes:

> If a body moving in a closed path, wholly containing the Earth's closed path about the Sun, goes through a whole number of revolutions in that path while going through a whole number of collineations with the Earth, and a fixed point inside the Earth's orbit—the position of the sun—while the Earth goes through a whole number of revolutions in its path, then the number of revolutions of the Earth is equal to the number of revolutions of the body plus the number of collineations.

Now this claim is not a mathematical truth. It is a mathematical truth, however, if we add to the antecedent the claim that the orbital period of the superior body is larger than the Earth's orbital period. The latter claim is equivalent in Copernican theory to the proposition that the period of a revolution of longitude is greater than a solar year. So the Copernican theory does not strictly reduce the Ptolemaic regularity to a mathematical truth by means of its identifications, but it does so reduce a consequence of that regularity, namely:

> If a superior planet has a period of revolution of longitude greater than a solar year, then if the planet goes through a whole number of revolutions of longitude while going through a whole number of oppositions in a whole number of solar years, then the number of solar years is equal to the number of revolutions of longitude plus the number of oppositions.

I am in no position to argue that the Copernicans saw the explanatory reduction I have sketched as part of their accomplishment, and I do not even have any opinion on the matter. I do think that the scheme provides a natural way to view the case, and accounts correctly for the sense that Copernican theory explained the regularity, whereas Ptolemaic theory, though it could be made to logically entail the regularity by proper adjustment of parameters, did not explain it.

Another form of understanding is the recognition of a pattern, and another kind of explanation—or another kind of explanatory virtue—is the demonstration that diverse phenomena are of a kind, and exhibit a common pattern. Newton's theory of dynamics and gravitation ties together the law of fall, the law of the pendulum, Kepler's laws, and the infinity of diverse theorems of celestial mechanics; Maxwell's electrodynamics bound up various laws of electrostatics, of magnetostatics and of electrodynamics. Copernican theory, it is often remarked, unified the description of the motions of the planets, both in the sense that it linked the parameters of the orbits of different planets to one another, and in providing a common mechanism for the periodicities of the several planets.

How does Newtonian celestial mechanics, for example, unify anything? By generating diverse regularities from a single scheme, in this case the scheme that specifies the acceleration of any body in a system of n point particles, subject only to mutual gravitational attraction, in terms of the masses of the particles and their mutual distances. It is, of course, a scheme which gives upon specification of the value of n a linear second order differential equation; each such differential equation has an infinity of solutions corresponding to the infinity of possible momentary geometrical configurations and velocities of the n particles. The many theorems of celestial mechanics are the result of applying this scheme to various values of n and to classes of initial conditions of n-particle systems. The single scheme gives, for each value of n greater than 1, an infinity of exact and approximate solutions.

It might be said that both Ptolemaic and Copernican astronomy unify the motions of the planets, for each theory provides a scheme which generates a description of the motions of the planets. One has, for any planet, only to specify the appropriate parameters and the scheme generates the planet's motion; for the Ptolemaic theory the parameters include the size and period of the deferent, the same for the epicycle, the location of the epicenter on the deferent, the location of the planet on the epicycle, at some time, and so on. For Copernican theory one needs the radius of the orbit, the orbital period, the position of the planet on the orbit at some time, and so on. But the unification provided by the Copernican theory is greater than that provided by the Ptolemaic theory since, at least at the qualitative level of accuracy, the Copernican theory requires fewer parameters to generate the motion of a planet. Very roughly, the fewer the features needed to generate a description of a system from a theory, the more alike the theory regards different systems with which it is concerned. Extra parameters help: Ptolemaic theory is more accurate than Copernican. It is a striking feature of scientific reasoning that, other things being equal, we are willing to sacrifice a bit of empirical accuracy for a gain in explanatory unification.

The importance of unification to scientific explanation has recently been emphasized by Michael Friedman,[9] who proposed that unity is *the* essential thing in scientific explanation. Friedman attempted to account for the notion of explanatory unification in terms of the notion of independent acceptability. The idea is that Graham's law and Boyle's law and Kepler's laws and Galileo's law can all be accepted independently of one another: there can be evidence sufficient for any one of them that is not evidence sufficient for any of the others. The acceptance of the conjunction of them is equivalent to the acceptance of each of them, on independent grounds. By contrast, the Newtonian theory from which all of these laws (or approximations of them) may be deduced can only be subdivided into a few hypotheses acceptable independently of one another, and the smallest number of Newtonian hypotheses that are each acceptable independently of accepting the whole of Newton's theory, and which are jointly logically equivalent to the whole of Newton's theory, is smaller than the number of independently acceptable laws that the theory entails. Friedman's attempt to account for explanation in these terms ran into technical difficulties.[10] The chief point, however, is that the kind of explanatory unification involved in finding a common pattern among diverse laws is somehow connected with our judgments about the bearing of evidence: for a collection of diverse laws the evidence for one law has no bearing on the evidence for the others. What Friedman's approach points to is not so much explanatory unification itself as an epistemic correlate of explanation, and that brings us back to confirmation and testing.

The fundamental question about confirmation, I believe, is one of relevance: what makes a particular piece of evidence count as evidence for or against any particular hypothesis? The question is particularly difficult when, as is often the case, the hypothesis is about properties and entities with which the evidence is not explicitly concerned; or, to abandon the material mode, when the hypothesis is stated in terms different from those in which the evidence is stated. One thing is clear, and that is that every working scientist is equipped with an enormous array of *beliefs* about what is relevant to what. Some of these beliefs are very general, while others are specific to a particular subject matter. If a microscopic theory entails claims about macroscopic properties, then the facts about those properties count for or against the microscopic theory. Moreover, microscopic theories or systems of a given kind are *supposed* to entail the macroscopic regularities shown by such systems, and this is so in every subject matter. Every specialist knows, besides such general principles about relevance, many others particular to his subject; thus gravitational theories are supposed to account for the motions of the planets, and a gravitational theory which does not entail regularities of planetary motion is counted defective. It is this abundance of belief, both general and specific, which gives the appearance that the bearing of evidence is determined more or less in a hypothetico-deductive way. We see descriptions of the evidence deduced from theory, and claims that the evidence bears on the theory, and we are tempted to conclude, erroneously, that it is *because* the theory entails the evidence, or because the theory entails the evidence in the right way, that the evidence is relevant. But in fact, without the background of belief about relevance, the deduction itself would not establish any bearing at all. Fifty years of failed attempts to characterize evidential relevance in hypothetico-deductive ways should have taught us that.

It cannot be, however, that all questions of the relevance of evidence are settled by appeal to established beliefs about the relevance of evidence. Our knowledge of what is relevant to what derives from our general knowledge of the way things work. When novel theories occur that break new territory, that either rather completely overthrow parts of our conception of how things work, or that provide an account of a range of things about whose working we had, before, no very definite idea, judgments of evidential relevance cannot derive from established beliefs about relevance. The same thing is often true, I think, when rather fine grained questions about the bearing of evidence arise. When one wants to know whether or not a novel phenomenon predicted by a complex new theory bears on a particular equation in that theory, very often established beliefs will not suffice. In all of these cases, the matter of the relevance of evidence must be established by some kind of structural connection; we know the connection cannot be hypothetico-deductive and it cannot be a connection established simply by a body of substantive beliefs.

I hold that there is a structural relation that obtains among evidence, hypothesis and theory, and that the relation can be seen pretty explicitly in the history of arguments over the bearing of evidence in many of those contexts where we should expect something of the sort: for example, in Newton's argument for universal gravitation, in the 19th century debates over the atomic theory, in arguments over the bearing of the classical tests of general relativity, in some contemporary social science, and doubtless in many other places besides. The basic idea is very simple.

Evidence for our theories is often stated in terms different from those of the theory itself and concerns only some of the properties the theory postulates. Kepler's laws are about the features of planetary orbits; Newton's law of gravitation, for which they are evidence, is about forces; the law of definite proportions is about the combining weights of chemical reactants; the atomic theory, for which it is evidence, is about atoms and how they combine. And on and on. I believe that the fundamental confirmation relation is by means of instances: the evidence somehow establishes an instance of the hypothesis evidenced. How can this be, if the theory and the evidence are cast in different terms? My answer (the answer), which is less banal than it sounds, is that confirmation is not a relation between a piece of evidence and an hypothesis, but is instead a relation among evidence, hypothesis and some theory or other. What the theory does is to relate the terms in the evidence to the terms in the hypothesis. Thus Kepler's laws describe features of the motion of the planets in their orbits about the sun, and Newton took these features to be evidence for his gravitational force law. He related these features of motion to features of *forces* by means of consequences of his own second law of motion—for example the consequence that says that if a body moves in a closed curve about a primary body so that the line from the body to the primary sweeps out equal areas in equal times, then the body is subject to a *force* directed towards the primary body. The crucial thing is that one is not deducing the evidence from the hypothesis with the help of the theory. One is, instead, doing almost the opposite: one deduces an instance of the hypothesis from the evidence, using the theory. Newton's argument in the *Principia* is *not* that Kepler's laws are logical consequences of Newton's dynamical theory together with the hypothesis of universal gravitation. His argument is that evidence about various systems—the primary planets, the satellites of Jupiter and Saturn, the Moon—evidence, that is, that says that each of these systems satisfies Kepler's laws, leads to instances of the law of universal gravitation.[11]

The deduction of an instance of an hypothesis from a piece of evidence by means of a theory must be done in the *right way* in order for there to be any confirmation or disconfirmation established thereby. Saying what the right way is is a little complicated, but again the basic idea is simple enough.

An instance obtained from the evidence by means of the theory counts as confirming the hypothesis with respect to the theory only if the instance can be viewed as the result of a test of the hypothesis with respect to the theory. A test of an hypothesis is something done which could turn out one way or another; if it turns out one way it provides a reason to believe the hypothesis, and if it turns out the other it provides a reason not to believe the hypothesis. In other words, the deduction of the instance of the hypothesis must be such that if the evidence had been different then the same form of argument would have led to a counter-instance of the hypothesis. The matter is simplest if we think in terms of quantitative hypotheses expressed as equations, with the evidence consisting of values of measured quantities. If the computed values satisfy the hypothesis, and if possible alternative values of the measured quantities would have led, by computations of the same kind, to values of the unmeasured quantities that would not satisfy the hypothesis, then the actual evidence confirms the hypothesis with respect to the theory. To return to Newton, from the actual evidence about features of the orbits of planets and satellites, Newton calculates, using his theory, the forces acting on the planets and satellites and finds them to be inverse square and directed towards the primary bodies, and to be in proportion to the product of the masses; had the motions of the planets been different—had the planets moved on third degree curves rather than second degree curves—the inverse square law of gravitation would not have been satisfied, and the forces calculated from the motions would not have provided instances of the law.

The notion of the possible values of the quantities that occur in our evidence is not one that I can make precise. The notion of two computations being of the same form or of the same kind can be made precise, but I will not do so here.[9] The fundamental idea is that confirmation is obtained only when instances of the hypothesis to be confirmed are obtained. Such instances are obtained by deducing them from the evidence by means of a theory; the instances only count as confirming instances if alternative evidence would, by the same form of argument, lead to counter-instances to the hypothesis. In confirmation, a theory is pulled up by its own bootstraps. The idea can be made precise enough either for theories and hypotheses viewed as first-order sentences, or for hypotheses understood as equations and theories understood as systems of equations. However one does it, as soon as one looks at things in this way, certain structural features of obvious methodological importance become apparent. Holism becomes importantly qualified. If we fix a theory and fix a body of evidence, then in general it will turn out that, applying the procedure just described, certain hypotheses can be tested with respect to the theory by a particular part of the evidence, and others cannot be. If we look at the claims of the theory itself, it turns out in general that a particular piece of evidence will test some claims of the theory with respect to the theory itself, but not other pieces. Considerations about simplicity also result, virtually automatically: the inclusion of what scientists (according to field) variously call physically meaningless quantities, or unobservable quantities or non-identifiable quantities within a theory will result in some of the theory's hypotheses not being testable with respect to the theory. The strategy of testing likewise provides a natural and familiar basis for much (but by no means all) of the scientific concern with variety of evidence. The evidence for a theory must be various both because no individual pieces of the evidence may suffice to test all of the hypotheses in the theory and also because we want, in so far as possible, independent tests of the various hypotheses in the theory—that is, for every pair of hypotheses A and B of the theory, we want, if possible, a test of A that does not use B (to compute values of unmeasured quantities) and likewise a test of B that does not use A. These are only the most obvious methodological results of looking at confirmation in the upside down way I have described. I will mention some deeper ones shortly.

If considerations of explanatory power are to be correlated with those of testing, one needs some criteria for comparative confirmation of theories based on the analysis of the the latter notion. The bootstrap strategy permits some obvious dimensions of comparison, but provides no ordering of them. (Although in restricted contexts an ordering can be developed plausibly enough.)[12] In comparing theories with respect to a given body of evidence, we are comparing the confirmation of one theory with respect to itself with the confirmation of another theory with respect to itself. It is better that all of the hypotheses of a theory be tested rather than only some of them, or at least that a body of hypotheses sufficient to entail the entire theory be tested. More tests are preferable to fewer. Not all hypotheses within a theory are equal, and for various reasons some are more important than others; if a piece of evidence tests a central hypothesis of one theory but only peripheral hypotheses of another, then it is better evidence for the first than for the second. We prefer that the evidence provide a variety of tests, that it permit one to test the various hypotheses of a theory independently of one another.

How well a theory is tested by a body of evidence depends on how tight the linkages are, according to the theory, among the properties with which the theory is concerned. So, too, a theory succeeds in explaining an empirical regularity by reducing it to a necessary truth only if there is a special intimacy postulated among the properties concerned. Now the two kinds of closeness—the one required for confirmation and the one required for the elimination of contingency—are not the same, and neither one is a sub-species of the other. They can diverge, but they more often go together. How they go together is best seen by reconsidering an example, the Copernican explanation of the Ptolemaic claim that if a superior planet goes through a number of cycles of anomaly while going through a number of revolutions in longitude in a number of solar years, then the number of solar years is equal to the number of oppositions plus the number of revolutions of longitude. Jupiter, for example, goes through 65 cycles of anomaly and 6 revolutions of longitude in 71 solar years. We have already seen that, construed as identifications, the following three Copernican principles reduce the Ptolemaic regularity to a necessary truth:

1. A solar year is the period of the Earth's orbit.
2. A cycle of anomaly (the period between oppositions) of a superior planet is the period between two successive collineations of the planet, the Earth and the sun, with the planet and the Earth on the same side of the sun (i.e., the period between two successive overtakings of the planet by the Earth).
3. The [average] period required for a revolution in longitude of a superior planet is the orbital period of the superior planet.

These claims are at the heart of Copernican theory, and their confirmation is of the first importance. Each of these claims is tested with respect to the other two by measurement of the number of oppositions and revolutions of longitude through which a superior planet passes in a number of solar years; that is, using the other two Copernican claims, an instance of any one of these claims can be deduced from such measurements. For example, from the fact that Jupiter has gone through 65 cycles of anomaly, the second Copernican hypothesis entails that Jupiter has been overtaken 65 times by the Earth. Because the planet in its orbit has been overtaken 65 times while 71 solar years have passed, and from the first hypothesis a solar year is the period of the Earth's orbit, it follows that in this same period Jupiter has made 6 revolutions about the sun. We have then that the period required for Jupiter to make 6 revolutions in its orbit is the same as the period required for Jupiter to make 6 revolutions of longitude, and so we obtain an instance of the third Copernican hypothesis. Moreover, it is an instance that tests the hypothesis, for had the relations among solar years, cycles of anomaly, and revolutions in longitude been different—that is, had the Ptolemaic regularity been otherwise—the same computations would have resulted in an instance contrary to hypothesis 3. If, for example, Jupiter had gone through 65 cycles of anomaly and 10 revolutions of longitude in 70 solar years, the Copernican hypothesis would have been disconfirmed with respect to its fellows.

If we compare the Ptolemaic theory, we find that not only does the theory fail to explain the relation between solar years and cycles of anomaly and revolutions of longitude, but also that instances of that relation provide no test of any common Ptolemaic hypothesis. All that the facts about Jupiter test, for instance, are the Ptolemaic parameters for Jupiter. Some of the features which enable the Copernican theory to explain the regularity are also features which make the regularity a test of Copernican hypotheses. The fact that the observed features of the planets are set equal in value to "theoretical" features by means of general principles is crucial to both explanation and confirmation; and the fact that the three identifications transform the Ptolemaic regularity into mathematical identity means that each identification is tested with respect to the other two, without the need of any further theoretical principles for the computations.

Not every feature of the elimination of contingency is important for confirmation, and not every explanation which proceeds by reducing the apparently contingent to the necessary is correlated with a test of the hypotheses used in the explanation. In the first place, the fact that the theoretical principles are *identifications*—a fact which is crucial to their explanatory role—is incidental to confirmation. The Copernican hypotheses would be tested with respect to one another by the measurements of positional astronomy even if they were not identifications, even if they were only unvarying correlations. In the second place, the theoretical quantities may not have values calculable from the properties they are postulated to explain. The internal Maxwell equations provide a vivid example; although they are explained by the identification of the electric and magnetic fields with functions of a scalar and vector potential, those identifications are not tested by the Maxwell equations, because the values of the scalar and vector potentials cannot be determined from values of the electric and magnetic fields. The demands for theories that explain by eliminating contingency and the demand for theories that are tested by instances can diverge and conflict. When a theory provides powerful explanations but does so in terms of quantities that cannot be determined—even using principles of the theory—from the phenomena explained, doubts often arise as to the reality of the properties postulated to explain the phenomena. Exactly this sort of doubt has been common enough about the electromagnetic potentials. A conflict of this kind was, I think, the central one in the first fifty years or so of the modern atomic theory. Dalton's theory did not reduce regularities such as the law of definite proportions to mathematical truths, but it did reduce the law of definite proportions to the additivity of mass, and somehow (I do not know how)[13] the additivity of mass must have seemed dramatically less contingent than did definite proportions. At the same time, the properties in virtue of which this explanation was accomplished—atomic and molecular weights—were not determinable other than by procedures that seemed arbitrary and without warrant. Fifty years and more of scientific work and argument were devoted to expanding the atomic theory to include a body of hypotheses that could be tested against one another and that sufficed to determine the weights of atoms.[14]

Explanatory unification is accompanied by something like the sort of unification Friedman describes—a reduction of the number of "independently acceptable" hypotheses. But, more exactly, explanatory unification takes regularities which provide no evidence for one another, and explains them by an hypothesis or hypotheses for which the several regularities *are* evidence. Consider a simple example, nearly Friedman's own. Newtonian gravitational theory explains such diverse regularities as Galileo's law of falling bodies, the law of the pendulum, and Kepler's laws. Consider just the body of regularities alone. Would observations that test the law of the pendulum also test Kepler's laws or Galileo's law? Apparently not, whether we apply intuition or philosophical theory. On the account of confirmation described earlier, consider the "theory" that consists just in the set of empirical laws mentioned (and, of course, their consequences). Then measurements that test the law of the pendulum will not test any of Kepler's laws with respect to this theory, and observations that test Kepler's third law will not necessarily test Kepler's other laws or Galileo's law or the law of the pendulum with respect to this theory. From the period of a pendulum one cannot compute the period of a planetary orbit. What is tested by what depends on the tightness of the theory, and the theory that consists of the conjunction of these empirical regularities is particularly loose. The evidence for the conjunction of Kepler's laws, Galileo's law, and the law of the pendulum is just the evidence for the first two of Kepler's laws, for the third of Kepler's laws, for Galileo's law and for the law of the pendulum. The laws afford one another no mutual support in the context of the theory consisting merely of their conjunction. Consider what happens when we introduce Newtonian gravitational theory. Kepler's second and third laws and the law of the pendulum provide evidence for the fundamental postulates of the theory—instances of those regularities test the gravitational law with respect to Newton's second and third laws; likewise, they also test the second and third of Newton's laws with respect to the gravitational force law. Similar results should obtain if we use the instances of

Galileo's law in conjunction with Kepler's second and third laws, or if we use Kepler's first law instead of Kepler's second and third laws. Various pieces of the evidence test a common set of theoretical principles—test them, that is, with respect to the very theory consisting of these principles and their consequences. If we consider what else Newtonian gravitational theory might explain, the case is even more dramatic. Consider the infinity of independent claims of the kind: "Under such and such initial conditions, a system of *n* bodies subject only to mutual gravitational attraction will exhibit property P." P may be any property describable without infinitary operations (differentialtion, infinite series). The number of such results derivable in Newtonian celestial mechanics is unlimited. Some give algebraic expressions for the trajectories of the bodies under special initial conditions, some give qualitative characteristics of the trajectories under initial conditions of a general kind, and so on. With respect to the "theory" consisting just of all of the claims of this kind that are consequences of Newtonian celestial mechanics itself (that is, the "theory" in question does not include the differential equations of Newtonian celestial mechanics itself), most of these claims have no bearing on one another, they provide one another no mutual support. For example, unless we pass through the differential equations of celestial mechanics, instances of the claim that each body in every system of two bodies moves on a second degree curve need provide no confirmation of the claim that if two bodies have, initally, collinear velocities then they will collide. Nor will results about two-body systems provide evidence for results about three-body systems, and so on. Yet many of these claims provide evidence for the fundamental principle of Newtonian celestial mechanics that specifies the differential equation satisfied by every system of *n* bodies subject only to mutual gravitational attraction.

The unity of Copernican theory is reflected in the power of observations of the planetary motion in testing Copernican hypotheses.[15] There are many examples, but consider simply the fact that, in Ptolemaic theory, the motions of the inferior planets have no bearing on hypotheses about the superior planets, but in Copernican theory they do. For example, observations of the inferior planets provide a test, with respect to Copernican theory, of the claim that the greater the distance a planet is from the sun, the smaller is its angular velocity (and so the greater is its orbital period). The principle that, for the superior planets, the greater the period required for a revolution in longitude, the greater the mean distance of the planet from the Earth, was common to both Ptolemaic and Copernican astornomical views. On the Ptolemaic theory, it cannot be tested, because the distances of the planets cannot be determined. On the Copernican theory, not only is the principle tested by observations of the superior planets themselves, it is besides a special case of the principle about the inverse correlation of planetary distances (from the sun) and their orbital periods, a principle tested by observations of the inferior planets. In binding the observations together, Copernican theory permits those observations to test its fundamental theoretical assumptions more completely, more often and with greater variety than does the Ptolemaic theory.

When we see a common pattern, what we see is the applicability of a common set of principles to diverse circumstances. In scientific contexts, that application ordinarily results in tests of those principles in diverse ways, with the result that disparate regularities, which have alone no mutual bearing, in common support a theory which entails and explains them all. Thus it happens that a finite body of observational consequences of a theory can provide better evidence for that theory, with respect to the theory itself, than that same finite body of observational consequences provides for the set of observational consequences of the theory with respect to the set of observational consequences of the theory. The examples discussed already suggest that this may be so, and one can give formalized theories that illustrate the point with respect to a formalized version of the confirmation theory.[16]

I have discussed two ways in which theories explain, and how those patterns of explanation are linked with confirmation I don't mean to suggest that these two patterns of explanation are all there are, or are the only patterns that have intimate connections to confirmation. Quite the contrary. Wesley Salmon[17] has emphasized the importance of patterns of explanation which proceed by attributing, in special circumstances, the correlation between two or more quantities to the action of a common cause, and which explain events by locating their place in a causal network. I believe a variety of patterns of causal explanation are closely connected with considerations of confirmation and testing. No doubt there are, besides, still other patterns of explanation, still other explanatory virtues, with other connections to testing, and they need to be described. The only great mistake would be a jejune generality that insists that explanation is one thing alone. Explanation is not the description of causal connections, or unification, or the elimination of contingency. It is any of those things, or others.

Successful tests and satisfying explanations are two different sorts of reasons to believe a theory; both make demands of theories far beyond the demand of empirical adequacy alone, and both are virtues of theories which may override small defects of empirical adequacy. The two kinds of reasons often go together, but they need not: one can have explanations without tests, and tests without explanatory virtues. I do not think we can understand the way of scientific reasoning and argument unless we understand how the variety of explanatory virtues and the many considerations of confirmation can conflict with one another and support one another and how, together, they function to select what is to be believed from the infinity of bizarre hypotheses that would save the phenomena.

Notes

[1]A claim which is forcefully denied in Bas van Fraassen, *The Scientific Image*, Cambridge University Press, forthcoming. A manuscript version of Professor van Fraassen's book was the stimulus for the present essay.

[2]See, for example, T. S. Kuhn, *The Copernican Revolution* (Cambridge, Mass.: Harvard University Press, 1957).

[3]R. Carnap, "Testability and Meaning," *Philosophy of Science 3* (1936): 419-71 and *4* (1937): 1-40.

[9]1[8]4C.Hempel, "Studies in the Logic of Confirmation," in *Aspects of Scientific Explanation* (Glencoe, Ill.: The Free Press, 1965).

[5]See C. Glymour, *Theory and Evidence* (Princeton: Princeton University Press, 1980).

[6]The thesis that such identities are, if true, necessarily true, has been developed in S. Kripke, "Identity and Necessity" in S. Schwartz, *Naming, Necessity and Natural Kinds* (Ithaca: Cornell University Press, 1977).

[7]Some of the very earliest interpretations of the general theory of relativity were of this kind. For example, Eddington wrote that "Matter does not cause the curvature of space-time, it *is* the curvature. Just as light does not cause electromagnetic oscillations; it *is* the oscillation." A. S. Eddington, *Report on the Relativity Theory of Gravitation*, 1920.

[8]The discussion of Ptolemaic and Copernican astronomies given below is pursued in more detail in chapter six of *Theory and Evidence*, op. cit.

[9]M. Friedman, "Explanation and Scientific Understanding," *Journal of Philosophy* LXXI(1974): 5-19.

[10]See P. Kitcher, "Explanation Conjunction and Unification," Journal of Philosophy LXXIII(1976): 207-12.

[11]Newton's argument for universal gravitation is analyzed in more detail and with more historical faithfulness in chapter six of *Theory and Evidence*, op. cit.

[12]See, for example, *ibid.*, chapter 7.

[13]A theory of degrees of natural necessity is presented in B. Skyrms, *Causal Necessity*, Yale University Press, forthcoming.

[14]An analysis of this history partly in terms of the bootstrap strategy of confirmation is presented in M. Gardner, "Realism and Instrumentalism in 19th Century Atomism," *Philosophy of Science 46*(1979): 1-34.

[15]Again, a much more detailed discussion will be found in chapter 6 of *Theory and Evidence*.

[16]See *ibid*, chapter 5, for example.

[17]W. Salmon, "Theoretical Explanation" in S. Korner, ed., *Explanation*, (Oxford: Blackwell, 1975): 118-145, and "Why Ask, "Why?"?" *Proceedings and Addresses of the American Philosophical Association 51*(1978): 683-705.