

I The Methodological Roles of Theory in Science

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ABSTRACT

Philosophy of science in this century can be roughly divided into two periods, positivist and postpositivist. Positivists maintained that scientific theories should be evaluated solely on the basis of observational data in accordance with a set of formal methodological rules. According to the positivists, both the data and the methodological rules are known independently of any scientific theories. Data are acquired by sense perception and they provide the empirical foundation for science. Methodological rules are established a priori and these rules provide a universal, permanent framework for the evaluation of scientific theories. One central postpositivist theme is that data and formal rules are not sufficient for evaluating scientific theories because they leave too many options open. Many different generalizations are compatible with any given body of data and, when an observation contradicts a theory, many responses remain possible. Postpositivists argue that, in scientific practice, these options are reduced because established theories take on a methodological role. These theories serve as guiding assumptions that, for a time, are not open to empirical challenge. Instead, these assumptions provide additional criteria for evaluating hypotheses, developing observational procedures, and interpreting the outcomes of these procedures. Still, since guiding assumptions are themselves empirical theories, they can be rejected and replaced with new guiding assumptions as science develops. Such replacements may result in a reconsideration of lower-level theories and a reinterpretation of observational data. The main result of this discussion for present purposes is that scientific theories are seen to provide an important part of the methodology of science, so that the development of methodology is an integral part of the development of science.

The development of philosophy of science in the twentieth century can be roughly divided into two major periods: the 'positivist' period, which has roots in the nineteenth century and dominated the field until the late 1950s, and the 'postpositivist' period, which

began to develop in the 1950s. Positivism was not a single doctrine. There were disputes among positivists on many issues of detail and substantial changes in aspects of the positivist program over time.¹ Still, there were some central doctrines that remained constant and that provided a unified framework for the philosophical analysis of scientific knowledge. These central doctrines include the view that there is a sharp distinction between theories and observational data, and a division of labor between science and philosophy of science. Positivists were strict empiricists with regard to scientific knowledge: scientific claims stand or fall solely on the basis of their ability to explain and predict our sensory experience. Philosophy of science, however, was conceived of as an a priori discipline. Positivists considered the proper subject matter of philosophy to be very narrow, but it included the methodology of science. Methodology provides norms for evaluating empirical claims and these norms, it was argued, must be known independently of any empirical research, and must provide a permanent, universal guide for the practice of science.

Postpositivist philosophy of science has challenged every major positivist theme and there is little unity among postpositivists. One exception concerns the relation between specific scientific theories and methodology. A radical alternative to the positivist view emerged at the beginning of the attack on positivism and has remained a central postpositivist theme. This alternative holds that methodology and science are deeply intertwined and that the evaluation and development of methodology are part of the process by which science develops. In this chapter I will examine the new view of methodology and some of its consequences.

For present purposes I will use the term 'theory' to refer to any generalization that goes beyond available data. Thus theories will range from straightforward universal generalizations, to theories such as those of Newton or Maxwell that involve several interlocked generalizations which cannot be applied or tested individually, to sweeping doctrines that cut across specific sciences, such as that every event has a cause. I will begin with an account of the positivist doctrines that bear on the relation between scientific theories and methodology.

THE POSITIVIST FRAMEWORK

P1

Within science there is a fundamental distinction between data and theory: theories make claims that go beyond available data, and thus yield predictions about what will occur in cases that have not yet been examined. Familiar examples of theoretical claims include: 'All objects fall to the earth with (the same) constant acceleration (provided that we can neglect air resistance),' 'Acceleration is always proportional to force,' 'All energy transfers take place at velocities less than the velocity of light in vacuo,' and 'Information always travels from nucleic acid to proteins, never in the reverse direction.' These examples include some claims that were once held to be universally true but are now rejected, and I have included them to illustrate a point. A major task of scientific research is to establish such theories, but since theories make claims that go beyond available evidence, it is difficult - perhaps impossible - to establish theoretical claims once and for all. Universal claims that have been supported by all the evidence available at a given time

have sometimes turned out to be either quite wrong or to provide only approximately correct results in a limited range of situations - much more limited than had been intended by the scientists who introduced the theory. This poses the problem of assessing when it is appropriate to accept a theory.

To understand the positivist approach to this problem, we should begin by noting that in modern logic universal generalizations are treated as hypothetical statements. The first of the above examples becomes: 'If an object is falling to the earth (and air resistance is insignificant), then the object falls with a constant acceleration.' Empirical support for a universal generalization consists of finding cases that meet the conditions stated in the antecedent and consequent of the generalization. Empirical refutation requires finding a case that meets the condition stated in the antecedent but does not meet the condition of the consequent. Positivists considered empirical refutation to be logically straightforward since refutation requires only a single counterinstance. Thus they focused their attention on confirmation which seemed much more problematic since we can find some supporting cases for virtually any universal generalization. The traditional problem of induction is to determine when we have accumulated enough evidence to be confident in a generalization.

From a historical perspective the confidence with which earlier scientists and philosophers accepted theories now looks somewhat naive since we have many examples of theories that are supported by an enormous body of evidence, but turned out to be wrong. In many case theories failed for reasons that no one had imagined in the period when the theory seemed beyond challenge. For example, the main problem with Newton's second law is that the proportionality constant - the object's mass - turned out not to be a constant after all, but a function of velocity.

P2

The situation I have been discussing concerns *logical relations* between a universal generalization and a body of evidence. I must say a few words about logic because I will be using this term in a narrower and more precise way than it is used in everyday conversation. Logic primarily concerns *relations* between statements, in particular relations between a body of evidence (typically referred to as 'premises') and a conclusion that is supposedly supported by those premises. Please note the following five points.

1. Modern logic is a subdiscipline of mathematics and is thus a formal subject. The point can be illustrated by the rule for finding the derivative of expressions of the form x^n : given any expression of this form, its derivative will be of the form nx^{n-1} . This rule holds independently of what x and n stand for. Whether a particular situation in the world can be correctly described by a law of the form x^n is not the mathematician's concern.

In an analogous fashion, logic is concerned with formal relations between premises and a conclusion. The gathering of evidence and the assessment of whether the description of the evidence is accurate does not concern the logician. Indeed, logicians do not even concern themselves with what the statements are about - just as mathematicians do not care if x^n concerns gravitation, magnetism, or what have you.

2. *Deductive logic* studies a particular relation between premises and conclusion: a *valid* deductive argument is one in which all true premises *guarantee* a true conclusion. Note that this definition concerns a relation that is independent of whether the premises and conclusion are in fact true. This is why logic, like the rest of mathematics, is an a priori

discipline. Logic abstracts from any actual features of the world and studies formal relations between formal expressions. Let me illustrate the idea in another context: in court, an attorney may present an argument to establish a conclusion. The opposing attorney can attack this argument in two ways: the truth of his/her opponent's premises can be questioned or the relevance of these premises to the conclusion can be questioned. Logic is concerned only with relevance relations and deductive logic is concerned with assessing the presence or absence of the strongest possible relevance relation. Few, if any, of the arguments that Sherlock Holmes and Mr Spock refer to as 'deductions' fit this account. It is not that they are misusing the word. In everyday talk 'deduce' is used as a synonym for 'infer'. Logicians and mathematicians use 'deduce' in a narrower, more precise way; our concerns here require that narrower usage.

3. I want to underline some immediate consequences of this account of deductive validity. First, the fact that an argument is invalid does not show that the conclusion is false; invalidity allows us to conclude only that we have failed to prove the conclusion. Here is an example of an obviously invalid argument: $2 + 3 = 5$, therefore smoking causes cancer. If pointing out the invalidity of the argument were sufficient to show that the conclusion is false, life and research would be much simpler than they are. Unfortunately, invalidity is not always so obvious.

By the same token, a valid argument in which one or more of the premises are false shows nothing about the conclusion. In general (i) a valid deductive argument in which (ii) all of the premises are true, guarantees a true conclusion. Given a failure of either condition, we learn nothing from the argument. It is a particularly poignant fact, with direct applications to our attempts to understand the world, that we can validly deduce true conclusions from false premises.²

4. An important consequence of our account of deduction is that if we know we have a valid argument with a false conclusion, we can be sure that the premises are not all true. This result provides the logical basis for testing scientific theories; I will have more to say about its significance shortly. For the moment let me emphasize a picky, but important, point: valid deduction of a false conclusion from a set of premises shows that the premises are not all true; it does not show that the premises are all not true.

5. Now consider *inductive logic*. I will use this term to cover all cases in which a body of evidence is supposed to support a conclusion on the basis of a weaker relation than that of deductive validity. In other words, we are in the inductive realm when we claim that our premises support a conclusion even though it is still possible that all of our premises are true but the conclusion is false. The classic illustration is the claim that all swans are white, a conclusion that turned out to be false even though, at one time, an enormous quantity of evidence available to Europeans uniformly supported this conclusion.

There are many kinds of inductive arguments but I will note only two of them here. I have already mentioned the most familiar type: a number of positive instances are taken to support a simple universal generalization. The second kind occurs when we *deduce* true conclusions from a theory and take those conclusions as evidence for the truth of the theory. While deduction of true conclusions does not guarantee that the theory is true, it does seem to provide support for that theory. It should be clear that scientists are mostly concerned with inductive support although, as the case just mentioned indicates, there is an intimate relation between deduction and induction. Moreover, the second kind of inductive support is fundamental when we are dealing with a theory of any degree of complexity.

P3

There is an important respect in which logic is a *normative* rather than a descriptive enterprise. Logic is concerned with the relations that *ought to hold* between evidence and a conclusion if the evidence is to support that conclusion. The point can be made by considering, once again, more familiar parts of mathematics. The rules for calculating a derivative were not arrived at by conducting an empirical study of the ways in which various people in fact carry out this calculation. The rules specify the correct way to determine a derivative, and anyone who follows different rules is just mistaken. How, you may ask, do we arrive at these norms? For present purposes I will consider only part of the positivists' answer to this question. There is a crucial tie between the claim that our knowledge of mathematics and logic is *a priori* and the claim that these disciplines provide norms. If results in these disciplines were established by gathering evidence about how people in fact calculate or which arguments they actually accept, further data on actual behavior would be relevant to assessing mathematical and logical results. This suggests that one necessary condition for a normative discipline is that it be an *a priori* discipline.

P4

Let us look at these points about logic and norms from a slightly different direction. Suppose one wishes to evaluate a scientific theory on the basis of a body of evidence. The scientists concerned have formulated the theory and gathered the evidence, but one more element is required in order to carry out the evaluation: we still need rules for determining what evaluation this body of evidence confers on the theory. It is these rules of evaluation that logic is supposed to provide and positivists considered these rules to be the core of 'the scientific method'.³ Since this is a much more austere notion than is generally associated with the rubric 'scientific method,' I want to describe two developments that led to the adoption of this view.

The first was the extraordinary development of deductive logic that began in the middle of the nineteenth century. For more than 2000 years logic had been viewed as a finished subject to be taught in essentially a single course; professional logicians concerned themselves with polishing up a few rough edges. At present, logic is a living research subject that has provided (among other things) some rather dramatic surprises, such as Gödel's theorems. One feature of this development has been a reconceiving of the nature of logic. In particular, the view that logic studies 'laws of thought' has been abandoned and replaced by the view that logic studies certain formal structures. Questions about how people actually think is a concern of psychology, an empirical science. Questions about how people ought to think have also been left aside, but the reasons for this require that we consider the second development mentioned above.

An older view of scientific method is that it concerns techniques for making new discoveries. This is the view that we typically trace back to Bacon and Descartes, and it endures well into the nineteenth century. Major reasons for abandoning this view of method come, first of all, from developments within science - especially the unanticipated transformations in our understanding of several domains brought about by the appearance of evolutionary biology, relativity, quantum mechanics, molecular biology, and more.

Looking back on the circumstances under which these developments appeared, there does not seem to have been any single method that the innovators followed. Nor is there any reason for thinking that there is a method that would allow less gifted researchers to have made these discoveries.

This reflection must be tied back to the developments in logic noted above. One consequence of these developments is a much clearer conception of what one is seeking when looking for a method. A notion of method has emerged that is extremely powerful and well-understood. This is the notion of an algorithm - a set of rules that guarantee a solution of a problem in a finite number of steps. From this perspective, a discovery method would be a set of rigorous rules that generate new discoveries. Now there are certain limited domains in which such rules exist. Rules of arithmetic provide routines that allow us to find answers to an enormous number of calculation problems; there is a set of rules that allow us to calculate the derivative of any expression; and, up to a point, there are rules of deductive logic that allow us to determine whether a given argument is valid. Unfortunately, we quickly run out of cases of this sort. There are, for example, no rules we can follow to integrate any expression whatsoever, or even to determine if an expression has an integral. And in deductive logic, once we reach a particular degree of complexity, there are no longer any rules that will allow us to apply an automatic procedure to assess whether a given argument is valid.⁴ Nice examples of this situation are provided by apparently simple mathematical conjectures that long resisted resolution, such as the four-color theorem, Fermat's last theorem, and Goldbach's conjecture. From a logical point of view, each of these is a problem about whether a particular proposition follows validly from a set of premises. Although two of these problems have now been resolved, none were resolved by applying an algorithm to test if the arguments in question are valid.

However, while there are no algorithms for finding answers in these cases, there are algorithms that allow us to answer another question. Given a proposed integral for an expression, the rules of differential calculus provide an algorithm for deciding if the proposal is correct. In a similar way, given a proposed proof of a conclusion from a set of premises, there are algorithms that allow us to evaluate whether that proof is valid.⁵ Algorithms of this latter sort became the focus of positivist studies of scientific method. In other words, the idea that we might find a discovery method - an algorithm - that would solve such problems as the causes of cancer or the origin of the universe came to be seen as utterly implausible (cf. Hempel 1966, pp. 14ff). But the idea that we could find algorithms that would allow us to assess a proposed solution to a problem seemed eminently plausible. In particular, this enterprise seemed well worth pursuing in the case of inductive arguments where we have a theory and a body of evidence, and we want to assess whether, or to what degree, the evidence supports the theory. Since there are no known algorithms for carrying out this project, it seemed that there was work to be done.

For those who are familiar with the jargon, this is what the distinction between *context of discovery* and *context of justification* is all about. Positivist philosophers of science concluded that their proper domain was logic and that as logicians they had nothing to say about discovery; their work was to be confined to establishing rules for assessing logical relations of confirmation. Moreover, since adequate rules for the assessment of deductive arguments were already known, these rules provided the model on which positivists sought

to construct an inductive logic of confirmation. Thus positivists undertook the development of a formal inductive logic - one that would permit assessment of nondeductive evidence relations solely on the basis of formal relations between a formalized theory and a formalized set of evidence reports.

P5

The positivist picture that I have been sketching leads directly to a division of labor between philosophers and scientists. Scientists are in charge of the entire empirical realm. It is their concern to gather evidence and formulate empirical theories. Philosophers maintain control of the a priori realm. For the philosophers I am discussing this is a very limited realm; it concerns only formal logic plus meaning relations that result from definitions - and only the first of these concerns us here. Nevertheless, this a priori realm is especially important because it alone provides the norms that justify scientific procedures. Without these norms we lack any basis for believing that science is a rational enterprise.⁶

POSTPOSITIVISM

I hope that at least the outlines of this picture are familiar and that I have succeeded in filling in some of the details because I now want to consider why, over the past 40 years or so, a number of philosophers, historians, and others who are interested in the overall development of science have argued that this picture is seriously defective.

I will begin with two points about induction. The first point has already been mentioned and I will not dwell on it. So far there is no acceptable set of rules of induction that provide clear criteria for deciding when a theory should be accepted or for determining the probability that a theory is true. To be sure, scientists do accept laws and theories, but not as a result of applying a formal inductive calculus; so far philosophers have not succeeded in providing an inductive logic that can stand alongside deductive logic.

The second point is commonly referred to as 'underdetermination of theory by evidence', although I think it more revealing to describe it as 'underdetermination of theory by evidence plus logic'. An unlimited number of alternative generalizations are equally well supported by any finite body of data. One way of seeing this is to think of data points that have been plotted on a pair of coordinate axes. On formal grounds alone, every curve that passes through these points is supported equally by the data. Observational data and formal logic provide no grounds for preferring one generalization over another - and thus no grounds for preferring one prediction for the unexamined cases over a multitude of competitors.

Reflection on the curve-fitting example will suggest an immediate response: some curves that fit the data are more plausible than others and additional criteria can be invoked for choosing among them. Kuhn (1977) has discussed five such criteria that are commonly used by scientists: a good theory should be *accurate* within its domain, *consistent* both internally and with other accepted theories, have *wide scope* (i.e. consequences that go beyond the data it was introduced to explain), be *simple*, and be *fruitful* as a basis for

further research. Note, however, that this list introduces criteria for evaluating theories that go beyond the resources of observation and formal logic. This point is clear in at least three respects.

1. With one limited exception, there are no known formal rules for applying these criteria, although there have been many attempts to formalize some of them. The exception is consistency. In logic there is a precise sense of the term 'consistent': a set of statements (which may have only one member) is inconsistent if it implies both p and not- p for some statement p . Otherwise, the set is consistent. We can see the point by considering a set that consists of two self-consistent statements: these statements are mutually consistent unless one of them contradicts the other. One consequence of this definition is that two statements that have nothing to do with each other must be mutually consistent. Now the requirement that a theory be consistent with other accepted theories includes the need to avoid formal inconsistency, but it includes more than this. In particular, a new theory must 'fit' with existing views, but there is no formal analysis of what counts as an appropriate fit - there is no formal analysis of consistency in this sense of the term.

In addition, even if we limit ourselves to consistency in the formal sense, the situation is more complex than it might seem at first glance. Internal inconsistency is a defect in a theory that gives grounds for seeking a better theory. Proving that a theory is inconsistent has the same import as showing that it has false empirical consequences: inconsistency guarantees that some part of the theory is false. Unfortunately, this does not require that we throw out the entire theory. Inconsistency only requires some modification in the theory, and logic provides little guidance as to what modification is appropriate. A classic example will illustrate the point.

Special relativity has two key postulates: the laws of physics are the same for all frames of reference moving with constant relative velocities, and the velocity of light is the same in all reference frames. Other physicists besides Einstein contemplated the reconstruction of physics on the basis of these two postulates, but abandoned the idea because it seems easy to show that these postulates are mutually inconsistent. Einstein notes the problem early in his first relativity paper, 'On the electrodynamics of moving bodies', and announces that he will show that these are only apparently inconsistent. He then argues that the inconsistency actually requires a third proposition: whether two events (at a distance from each other) are simultaneous is an objective fact. Before Einstein's analysis, physicists had not considered this claim to be a hypothesis subject to reconsideration, and this is the hypothesis that Einstein rejects in order to eliminate the inconsistency.

There is an additional twist that we should note. An inconsistent theory may be extremely useful if it is deployed with sufficient care. The classic example here is Bohr's theory of the atom which postulates that certain electron orbits are stable, but does so in the context of classical electrodynamics which implies that no orbits are stable. The theory's inconsistency was recognized as a defect and this defect was eliminated by the new quantum theory of Schrödinger and Heisenberg. But inconsistency is not a defect that immediately blocks legitimate application of the theory. In general, inconsistency is a formal defect in a theory, but the discovery that a theory is inconsistent does not require any specific response from theorists.

Simplicity is another example worth considering. There have been many attempts to provide a formal analysis of simplicity and none have withstood criticism. Part of the problem is that there are different senses in which a theory may be called 'simple'. One of

these is psychological, i.e. one theory is easier to understand and use than another. But psychological simplicity is not a formal property of a theory. Psychological simplicity varies among individuals and among historical periods: which theory is easiest to use can depend on features of an individual's psychology and on just how one was trained.

Another kind of simplicity requires limiting the number of independent premises in a theory. Note, however, that this may compete with psychological simplicity. In addition, it is not always clear how to count the premises. Purely logical manipulations can alter the apparent number of assumptions involved in a theory.⁷

A third approach is captured in Occam's razor: postulate as few entities as possible. But again we have a kind of simplicity that can compete with other versions, and historical experience should make us wonder if the theory that postulates the smallest number of entities is more likely to be true than one which postulates a larger set of entities. For example, fundamental particle physics has now faced several generations of proliferating basic entities followed by a move to a deeper level that reduced the number of basic entities, followed by another proliferation. The particles at the current deepest level are tied together by an elegant mathematical structure - but this may involve yet another kind of simplicity that I will not pursue in detail.

The last example suggests another interesting point about simplicity: if we apply induction to the history of science, there does not seem to be any good reason for believing that the simplest theories are the most likely to prevail. The progression in planetary theory from circles to ellipses to perturbed ellipses that do not fit any simple curve is a progression of, at least, increasing mathematical and psychological complexity. The move from Newton's gravitational theory to Einstein's involves an increase from one gravitational potential to 10; there are other respects in which the new theory is arguably more complex, and others in which it is arguably simpler, than the older theory. Of course, these changes were not made arbitrarily. At the very least they were made in the pursuit of greater accuracy - which brings me to the second point that was mentioned some while back.

2. As Kuhn points out, all five of these criteria can compete with each other. I have just cited cases in which accuracy and simplicity compete, but it would be a mistake to jump to the conclusion that accuracy always prevails. Sometimes theories that are accurate but out of harmony with established approaches will have a hard time getting a hearing.⁸ There is no reason for assuming that we will eventually find formal rules for deciding how these criteria should be applied - and there is no reason for thinking that scientists have been impotent in the absence of such criteria. Assessment of which criteria should prevail in a particular case requires reflection and judgment that must be based on a detailed understanding of the situation in the field. Such scientific judgment lacks the certainty of results arrived at by means of formal rules, but this does not make judgment arbitrary or utterly unreliable.

3. Now consider why these five criteria are appropriate. Kuhn's response is that historical research shows them to be widely used in science and the position I will develop below suggests that this is the right way to go about finding the relevant criteria. For the moment I want to stress that these criteria are themselves nonformal and that the process of recognizing the appropriate criteria also requires considerations that go beyond the application of a formal calculus.

POPPER

Before we leave the topic of formal methodological rules, I want to back up historically and consider Popper's attempt to preserve the idea that evaluations of theories are solely a matter of evidence and formal logic. Popper's starting point was his conviction that there is no such thing as inductive support for a theory - a result that he maintained had already been demonstrated by Hume.⁹ There is, according to Hume, a psychological process that results in our believing a generalization as we encounter confirmations, but there is no basis in logic for this process. As a result, Popper concluded, the logical basis for the evaluation of scientific theories must be found in deductive logic. Popper built his account of scientific method on a feature of deduction that we have already noted: the deduction of a false conclusion from a set of premises proves unequivocally that something is wrong with those premises. If we focus our attention on falsification, rather than on confirmation, the only logic we require is deductive logic, whose status is not in doubt. This leaves us with the central characteristic of Popper's philosophy of science: the defining character of scientific propositions is that they are empirically falsifiable - not that they are empirically confirmable - and the proper method of scientific research is to propound hypotheses and then attempt to falsify them.

I want to emphasize a point that is often missed by scientists who turn to Popper for an account of the nature of science. Popper insists that there is no such thing as induction and that there is no process by which the testing of a scientific theory yields reasons to believe that theory. It is alright to use a theory that has passed severe tests, but we ought also to be continually attempting to disprove that theory - keeping in mind that one false consequence provides sufficient grounds for rejecting the theory while no run of correct predictions provides any grounds for believing the theory to be true. The only time we learn something definitive from the scientific process is when we discover that we are wrong.

There are many problems with the Popperian approach, but I will focus on those that generate further forms of underdetermination. Recall that while the deduction of a false consequence from a theory proves that something is wrong with that theory, it does not pick out what is wrong or even how many revisions we must make. The only clear result is that at least one revision is required. Consider a realistic example of the deduction of an observable result in Newtonian mechanics: the prediction of the location of Uranus in its orbit at a specific time. This prediction requires Newton's laws of motion and gravitation plus a large number of initial conditions: the masses of Uranus and the sun, the distance between them at some point in time, Uranus' tangential velocity, and its location in its orbit at some point in time. If we want a really accurate calculation, we will need values for the masses of other planets and their distances from Uranus.¹⁰ In addition, any instrumentation that is required to gather the observational data will depend on other scientific results that - on Popperian grounds - have not been established as correct. When we encounter an incorrect prediction, logic tells us that at least one member of our total set of assumptions is mistaken, but logic does not provide a clue as to which should be challenged.

Moreover, even if we leave aside questions raised by the theoretical basis of our instruments, Popper holds that observational data are part of the body of science and thus falsifiable. The upshot is that we should consider the complex consisting of a scientific theory, the initial and boundary conditions needed to arrive at a testable prediction, any theories required by the test procedure, and the observational outcome of the test as a

single set of claims. The discovery that the observational result contradicts the prediction amounts to the discovery that this set is inconsistent. From a Popperian perspective, empirical refutation and the discovery of an internal inconsistency are variations on a single theme. In either case, once we discover an inconsistency, logic has done all it can for us, and we do not have a clue as to the source of the inconsistency. The contemporary solar neutrino problem will underline the point that this is a genuine difficulty. The anomalous result of the first solar neutrino experiment was announced almost 30 years ago and it shows that there is a genuine problem somewhere in a large body of science. There have been dozens of proposals as to the source of the problem and research aimed at narrowing down the choices continues, but methodology, as understood by positivists and Popperians, has little guidance to offer (see Bahcall 1989 for a comprehensive, although already somewhat out of date, review).

Popper was aware of this problem and attempted to solve it by introducing a number of additional methodological rules, such as that one should always reject the most general propositions in a set (see Popper 1968, especially sections 11 and 20, and Ch. 5). There is much interesting material here, and perhaps a few surprises for the reader, but the outcome is to underline again the limits of methodology when one attempts to capture this subject wholly within the confines of formal logic.

METHODOLOGY RECONSIDERED

Please note, I am not claiming that observation and logic are unimportant for scientific method, only that they are not sufficient because at every key juncture they leave too many options open. The point is nicely stated in a recent essay by Antony (1993, p. 211): 'The problem of paring down the alternatives is the defining feature of the human epistemic condition.' The number of alternatives to be explored at a given time must be limited if effective research is to take place. This is also one of the key problems that Kuhn (1970) addressed in *The Structure of Scientific Revolutions* and the answer that Antony offers is essentially the same one that Kuhn proposed: at any period in the development of knowledge there are theories that are generally accepted by the relevant community and are taken as fixed. These fixed theories guide research by providing criteria for deciding what questions are worth asking, what observations are worth making, what phenomena are problematic, and what counts as a legitimate solution to a problem. Theories that play this role function as part of the methodology of a discipline. I will call theories that function as methodology 'guiding assumptions'.¹¹

Guiding assumptions have two key features that pull in opposite directions and that must be balanced against each other. On one hand, methodology is supposed to provide a stable set of rules for the pursuit of science. The idea that methodology is known a priori makes it not just stable, but permanent, by taking it completely outside the realm of empirical evaluation. Guiding assumptions share this feature of the a priori in that they are immune to being undermined by empirical results - but only temporarily. For guiding assumptions are still theories that can be challenged empirically and replaced under appropriate circumstances. Guiding assumptions are the locus of 'the essential tension' between conservatism and innovation that Kuhn maintains is central to scientific research.

The idea that science requires guiding assumptions originates with Kant's notion of synthetic a priori propositions, although Kant thought that he had identified a set of propositions that necessarily provide the framework for science and thus that can never be challenged. Kant's two most important examples - Euclidean geometry and the causal principle - illustrate how guiding assumptions that endure for substantial periods of time can eventually be challenged by empirical results. These two examples, along with Einstein's treatment of simultaneity, also illustrate the way in which scientists can rely on guiding assumptions without realizing that they are assumptions. Hopefully, work since the 1950s in the history and philosophy of science has placed us in a somewhat more reflective situation with respect to the role of guiding assumptions in science. Nevertheless, I think it will be worthwhile to examine a few examples.

Perhaps the most widely discussed example is the response to discrepancies between the calculated and observed orbits of Mercury and Uranus that emerged in the nineteenth century. I will not tell this story yet again, but I will note the nice way in which it illustrates the two key features of guiding assumptions. In the case of Uranus, Adams and Leverrier assumed that Newtonian mechanics was correct and used the resources of this theory to solve the problem; the outcome was the discovery of Neptune. In the case of Mercury, Leverrier's attempt to overcome the anomaly by introducing yet another planet failed, and the anomaly was not resolved until Einstein introduced his new theory of gravitation. I want to add three more examples that will illustrate various aspects of the role of guiding assumptions in scientific research.

During the nineteenth century a great deal of careful empirical research by highly skilled chemists was guided by Prout's thesis that the weight of every chemical element is an integral multiple of the weight of hydrogen. When a sample presented an anomaly it was assumed that the sample was insufficiently pure and that further laboratory refinement was required. After the discovery of isotopes, weights that were previously considered anomalous were no longer considered problematic and this entire line of research was no longer significant. Frederick Soddy, who won a Nobel Prize in chemistry for his work on isotopes, summed up the situation this way (1932, p. 50):

There is something, surely, akin to if not transcending tragedy in the fate that has overtaken the life work of that distinguished galaxy of nineteenth-century chemists, rightly revered by their contemporaries as representing the crown and perfection of accurate scientific measurement. Their hard-won results, for the *moment at least* [italics added], appears as of as little interest and significance as the determination of the average weight of a collection of bottles, some of them full and some of them more or less empty.

I want to suggest, however, that it is a mistake to describe this outcome as tragic. Rather, it is a normal outcome of research given human epistemic conditions. This is the same fate that befell Ptolemaic astronomers, phlogiston chemists, Leverrier's attempt to account for the orbit of Mercury, Einstein's 30-year quest for a theory to unify gravitation and electromagnetism, and many others. It is also a fate that may befall many contemporaries if their guiding assumptions are undermined. The key point is that we must get beyond the view that valuable scientific work consists only in the production of true results. In many cases, the most important outcome of scientific work is found in the contributions it makes to undermining the guiding assumptions on which it is based.

As a more recent example, consider the use of red-shift data in determinations of the recession velocities of celestial objects. There is much to say about the theoretical elements involved in breaking light up into a spectrum and interpreting an unfamiliar spectrum as a red-shifted version of a familiar spectrum, but I want to focus on a different point. Once it has been decided that we are dealing with a Doppler shift, we need a formula to convert that shift into a recession velocity; Newtonian physics and relativity give different formulas for this conversion. Which one should we use? Obviously, since relativity is the currently accepted theory, we use the relativistic formula - with its built-in guarantee that no recession can achieve the velocity of light.¹² One might reflect on how different current cosmology would be if we still used the Newtonian formula. There is no guarantee that future developments will never lead to another revision in this formula along with a reinterpretation of masses of data already collected. But, at the present time, the methodological role that was once played by Newtonian mechanics is being played by relativity. The theory is used to interpret data rather than being subjected to evaluation on the basis of that data.

For my final example I turn to the solar neutrino experiment. The idea of measuring the neutrino flux from the sun in order to test the standard model of stellar energy production is a good example of the way in which even the best established of theories may be subjected to empirical evaluation when a new kind of test becomes available. Davis proposed the experiment in the early 1960s, only a few years after the ability to detect neutrinos was established.¹³ Still, there was considerable debate over whether the test was worth the expenditure of limited resources given the absence of any serious doubt about the standard theory. It is pretty likely that the experiment - which is not very expensive by contemporary standards - would not have been done if its cost had been significantly higher. (The relevant history is reviewed in Bahcall and Davis 1989.) Bahcall (1989, p. 5) also describes the central role of the standard model as a source of guiding assumptions

that astronomers use everyday in their research. In interpreting astronomical observations (made by detecting photons), and in constructing astronomical theories, we use the theory of stellar evolution to determine the ages of the stars, to interpret their compositions, to infer the evolution of galaxies, and to place limits on the chemical composition of the primordial material of the universe. Each of these basic astronomical industries is called into question by at least one of the proposed solutions of the solar neutrino problem.

A large number of additional theories are implicated in the design of the experiment and the interpretation of its results. The key instrument in Davis's experiment is a large tank of cleaning fluid in a South Dakota gold mine. The tank was placed about a mile underground because contemporary physics indicates that neutrinos are the only particles of astronomical origin that could penetrate the earth and reach the tank. The material in the tank is C_2Cl_4 and the relevant reaction is reverse beta decay: occasionally a neutrino will transform a chlorine atom into argon which appears in the tank as a dissolved gas that can be periodically removed. Since the resulting isotope of argon is radioactive with a known half-life, the amount recovered can be measured by studying its decay. All of the rather complex chemistry involved in extracting the minute amounts of argon produced, plus the physics involved in measuring how much has been extracted, are included in the complex of guiding assumptions required for this experiment. From the point of view of formal logic alone, any of these theories can be viewed as having been challenged by the anom-

alous result. But at no point are logic and the data sufficient to settle the source of the anomaly; at each stage of evaluation, productive research requires that a substantial number of these theories are not being actively questioned. Indeed, one must accept a large set of theories (e.g. the existence of inverse beta decay, our understanding of radioactive decay and of the operation of a proportional counter) for there to be an anomaly in the first place.¹⁴ Consider, also, the role that guiding assumptions play in turning a tank of cleaning fluid into a sensitive scientific instrument.

I want to return now to a previous topic, the relation between methodology and discovery. The positivists held that there is no connection between these at all. However new ideas appear, the role of methodology is to guide the process of sorting out which proposals are to be accepted and which rejected. Now if one identifies methodology with formal logic, and identifies a rational discovery process with the application of an algorithm, then there is indeed no rational basis for pursuing scientific discovery. But once we expand our notion of methodology to include established theories, then it becomes clear that the process of introducing hypotheses worth taking seriously receives considerable methodological guidance. No further comment should be needed to make this point with regard to the role of Newtonian theory in guiding the search for reasonable explanations of the anomalies in the orbits of Uranus and Mercury, nor with regard to a large body of physics in the development of the solar neutrino experiment. But consider the case of general relativity, which involved significant departures from previous physics. Many features of Einstein's program are continuous with developments in physics over the previous three centuries. For example, Einstein was pursuing the ideal of a single theory that would encompass as wide a range of physical phenomena as possible - an approach that had achieved several notable successes since Galileo. In addition, Einstein relied on the standard mathematics that had been so successful in physics (although he introduced the use of tensors, which were not familiar to physicists) and on familiar observational results (although special relativity had already required reinterpretation of the Michelson-Morley experiment). In other words, neither Einstein nor any of the other scientists who initiated scientific revolutions, worked in a methodological vacuum even when their results overturned key features of previously accepted guiding assumptions. But the methodology they pursued came from accepted science not from formal logic.

Still, the case of scientific revolutions does raise additional questions about the role of methodology in science. Revolutions involve challenges to established guiding assumptions. As a result, the set of alternatives that can be seriously considered expands as previously accepted criteria for limiting the range of acceptable alternatives are challenged. This raises serious questions about the existence of sufficient methodological guidance to resolve revolutionary disputes. Within the limits of this chapter I can only indicate the way that this question should be approached. Note, first, that while guiding assumptions are challenged in revolutionary situations, they are not completely eliminated; only a particular subset is challenged in a specific case. The range of alternatives is increased, but the research community is not in the position of considering any proposal whatsoever. Second, recall our earlier discussion of Kuhn's five criteria for evaluating theories. Whatever guidance these criteria give, they do not provide algorithms. The way they are to be applied, and their relative weighting, depend on the professional judgment of the scientists concerned - which is why Kuhn (1977, p. 331) says that these criteria 'function not as rules, which determine choice, but as values, which influence it'. It is this scientific

judgment, operating within the framework of a scientific community, that provides the ultimate court of appeal in the development of science. This court of appeal is far from infallible, but it is also far from arbitrary.¹⁵

This leads me to the final topic I wish to discuss. If methodology is fallible and subject to change, what happens if scientists spend their entire careers working on the basis of guiding assumptions that turn out to be false? Does this not make their work utterly worthless? The short answer is, 'No,' for the reasons I indicated in response to Soddy's remarks. To see why, we must keep in mind that scientists, like other human beings, are fallible cognitive agents - which does not mean that they are without cognitive resources. One of the most powerful of these resources is the ability to develop and test scientific theories, and it is through this process that we discover mistakes and develop new proposals. Most scientists make their contributions through this process and none can expect to bring any important part of this process to its completion. Consider some of our examples once again. The anomalies in the orbits of Uranus and Mercury were discovered as a result of generations of scientific work. This work included the development of improved telescopes, better calculation techniques, and long-term accumulation of observations. In the case of Mercury, the anomaly was identified only after the telescopic observation of hundreds of orbits. Everyone now alive will be dead long before a single orbit of Pluto has been observed.

An even more sobering thought for those who think that valuable scientific work requires definitive solutions to problems in their lifetime is provided by the more recent discovery of deterministic chaos. We now know of the existence of previously unnoticed chaotic solutions to fairly simple equations that have been in use for substantial periods of time. It is far from clear how much this discovery will alter our view of nature and of scientific method. But the exploration of chaos - and other nonlinear phenomena - is vitally dependent on modern computing power, so that if the discovery had come much earlier (Poincaré saw at least the outlines), not much could have been done with it.

CONCLUSION

I will end this discussion by stressing one central contrast between the approach with which I began and that at which I have arrived. Positivists and Popper, along with the mainstream empiricist tradition in philosophy, held that we have no a priori insight into nature. Whatever we know about the world must be learned through experience. But they also held that the empirical study of nature must be guided by a methodology that is known a priori. The view I have defended here is a more radical form of empiricism which denies that we have a priori insight into how nature should be studied. One of the things that we must learn is how to learn.¹⁶ We have had to learn, for example, that observation is not just a matter of using our senses. The world is full of entities and processes that we cannot sense but that we can still study by building appropriate instruments. We have also learned that items outside the range of our senses are often more important for understanding nature than are the items we can detect with our unaided senses. We have learned the power of controlled experiments and of how to achieve the virtues of controlled experiments in realms where literal control is not possible (consider, yet again, the solar neutrino experiment). We have learned the power of statistical methods and found applications for mathe-

matics that is so abstract that it once seemed clear it would always be just an intellectual game. From the perspective I have been presenting, all of these and many other developments are improvements in the methodology, as well as in the content, of science. In other words, instead of the older picture of scientists exploring nature within a fixed methodology, we have arrived at the conclusion that improving methodology is a central part of the scientific process.

Finally, if scientists create their own methodology, where does this leave the philosopher? I hope that this chapter has provided an illustration of one major task for the philosophy of science: to attempt to understand the scientific process by looking at science from a broader logical and historical perspective than is required for work within a single scientific discipline. This, too, is an empirical endeavor that works in terms of guiding assumptions that are subject to challenge when science goes off in a new direction, as well as from internal philosophical considerations. Given the empirical nature of this study, it is important to extend its data base in another way. Positivists took physics as the paradigmatic science and - since the thesis that methodology is the same for all sciences was one of their guiding assumptions - they did not see any need to examine other sciences. In recent decades, however, we have seen philosophers paying increasing attention to other scientific fields. From the perspective of this chapter, the present interaction between philosophy of science and geomorphology is one from which both subjects should benefit.¹⁷

NOTES

1. See Scheffler (1963) and Brown (1979) for discussions of the development of positivism from, respectively, a positivist and a postpositivist point of view.
2. Here is a trivial example: all dogs are guppies, all guppies are mammals, therefore all dogs are mammals.
3. That theories are to be evaluated empirically is also part of scientific method, but this was not considered problematic, and it dropped into the background of actual discussions among positivists.
4. This point is reached when we have existential quantifiers trapped inside universal quantifiers. See Quine (1982, especially Parts II and III), for a systematic discussion of the limits of algorithms in assessing validity.
5. Those familiar with discussions of the recent proof of Fermat's last theorem will recognize that even here the situation is more complex than positivists imagined.
6. Coffa (1976, p. 207), writing specifically about Carnap, offers the following description: 'the linguistic world of knowledge was, for Carnap, an Upstairs-Downstairs world. Upstairs, in the object language, live the masters, the scientists, those whose task it is to find out the facts about the world. Downstairs, in the metalanguage, live the servants, the philosophers, whose task it is to keep the house of knowledge clean and to set an occasional Upstairs novice straight on matters of taste and manners.'
7. Consider another kind of problem: the introduction of vector and tensor notation allows us to capture sets of equations in a single equation. Does this reduce the number of independent premises?
8. See, for example, Polanyi's (1969) discussion of the reasons why his potential theory of adsorption was largely ignored for several decades - along with his defense of the practices that led to his own theory being ignored at first.

9. The original German-language edition of Popper (1968) appeared in 1934, well before the problem of choosing between equally well-supported theories came to prominence. However, Popper's complete rejection of any notion of inductive support makes the latter problem moot.
10. If we pursue this degree of accuracy we also move into a realm in which a rigorous deduction from a list of all of these premises is not possible. Approximations must be introduced and these approximations provide additional reasons why a prediction may have been incorrect. Newton faced this problem in calculating the orbit of moon. For one key parameter he got a result that was twice the observed value (Newton 1971, p. 147). Clairaut initially thought that the problem should be solved by adding an inverse fourth-power term to the gravitation law. 'But before proceeding far on his new theory he took the precaution to carry his calculation, using Newton's law, to a higher degree of approximation, and, behold, in 1749 he reached results agreeing with observation' (Cajori 1971, p. 650).
11. This term was introduced in Laudan et al. (1986) to cover this central theme that is shared by many postpositivist writers. See also Donovan et al. (1988). In his 'Postscript' Kuhn (1970, p.182) notes that 'Scientists themselves would say they share a theory or a set of theories, and I shall be glad if the term can ultimately be recaptured for this use.' Kuhn's hesitation to use 'theory' was generated by the narrow use of this term that was standard in the late stages of positivism.
12. For a measured red-shift of S, the relativistic formula is $P = (S^2 - 1)/(S^2 + 1)$, where is the recession velocity expressed as a fraction of the velocity of light *in vacuo*. The Newtonian formula is $\beta = S - 1$. Although it is mathematically impossible for β to exceed one in the relativistic formula, the use of this formula in an observation procedure does not automatically prevent that procedure from yielding a result that can contradict the relativistic limitation on velocities. See Brown (1993, 1994a) for discussion.
13. The first experimental detection of neutrinos was reported in 1953, but the experimenters were not fully confident of their result until 1956. See Reines and Cowan (1953), Cowan et al. (1956), Reines (1979).
14. See Bahcall (1989) for these and many other details. For discussion of the implications of this experiment for our understanding of scientific observation see Brown (1987, 1995) and Shapere (1982).
15. I have developed this view at greater length in Brown (1988) and have improved on several aspects of that discussion in Brown (1994b).
16. The idea that we must learn how to study nature has been a consistent theme of naturalistic epistemology. See, for example, Hooker (1987), Laudan (1987), and Shapere (1984).
17. For some examples from geomorphology of the view of methodology I have proposed, see Rhoads and Thorn (1996).

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