23
SCIENTIFIC METHOD

Howard Sankey

Philosophers have long held there to be something special about science that distinguishes it from non-science. Rather than a shared subject-matter, the distinction is usually taken to reside at the methodological level. What sets the sciences apart from non-scientific pursuits is the possession of a characteristic method employed by their practitioners. It is customary to refer to this characteristic method of science as the "scientific method." Those disciplines which employ the scientific method qualify as sciences; those which do not employ the method are considered not to be scientific.

While most philosophers agree that science is to be characterized in methodological terms, they disagree about the nature of this method. Many take the fundamental method of science to be an inductive method. Others belittle induction or deny its use altogether. It was once taken to be virtually axiomatic that the method of science is a fixed and universal method employed throughout the sciences. Yet, at the present time, it is not uncommon to hold that method depends on historical time-period or cultural context, or that it varies from one field of science to another. While it was once widely believed that there is a single scientific method characteristic of all science, it is now more common to hold that the method of science consists of a multi-faceted array of rules, techniques and procedures which broadly govern the practice of science. Indeed, some have concluded that there is, strictly speaking, no such thing as *the* scientific method.

It is possible to distinguish a number of different levels at which methods may be employed in science. At the ground level of data collection and experimental practice, there are methods which govern the proper conduct of an experiment or the correct employment of a piece of equipment. At a slight remove from experimental practice, there are methods of experimental design or test procedure, such as the use of random trials or double-blind tests in clinical trials. At a more remote level are methods for the appraisal, or evaluation, of theories, and possibly theory construction. The methods described in what follows tend, for the most part, to comprise methods of theory appraisal which are designed to provide the warrant for theory choice or theory acceptance. For it is at this level that the bulk of the philosophical debate about scientific method has been conducted.

Philosophers sometimes distinguish between two contexts in which a method might be employed in science. The first context, in which a new idea emerges in the mind of a scientist, has been called the "context of discovery." The second context, in which the idea receives scientific validation, is known as the "context of justification." The bulk of methodological discussion relates to the second context. This reflects the once-dominant view that the process of having a new idea is an inscrutable matter of individual psychology, rather than a matter of logic or method. Contemporary philosophers of science place less weight on this traditional distinction than was previously the case. Indeed, many would be prepared to grant a role to method in the context of discovery.

Naive inductivism

The first view of method I consider is one that is usually presented as part of the common-sense view of science rather than credited to any particular philosopher of science. This is the *naive inductivist* view that the method of science consists simply of inductive inference on the basis of observation. On this naive view, induction is understood in a rudimentary sense as enumerative induction. An inference is inductive in this sense if it proceeds from a limited number of positive instances which have been observed to a generalization that covers all instances whether or not they have been observed.

The naive inductive method may be presented in simplified terms as a two-step procedure for arriving at theories on the basis of observation. Suppose, to begin with, that a specific domain of phenomena is under investigation. The first step in a scientific investigation consists of the collection of empirical data from the domain. Scientists gather empirical data by employing unbiased sense perception to detect observational facts. Only after the collection of empirical data may scientists proceed to the second step, which is the formulation of scientific laws and theories by a process of inductive generalization. Scientists employ inductive reasoning to infer from empirical data to generalizations about the behavior of the items found in the domain under investigation. The generalizations which result constitute empirical laws, which may be conjoined with other such laws to serve as the basis of scientific theories. Induction plays a fundamental role in this method because it is required in order to draw an inference from the limited data provided by observation to the generalizations which apply to items beyond those which have been observed.

This account of method provides both a method of discovery and a method of justification. It provides an account of how scientists arrive at laws and theories, as well as an account of the validation of laws and theories. Armed with the empirical data they have collected, scientists employ inductive generalization to discover laws, which form the basis of theories. At the same time, scientists' use of the inductive method provides the warrant for their acceptance of the laws and theories that result. For the method consists of the use of perception and inductive inference, which are themselves epistemically well-grounded means of belief formation.

Despite its simplicity, the naive inductive method faces a number of serious problems. In the first place, it is not clear that the process of data collection may precede or be independent of theory in the way that the naive inductivist suggests.

For in order to collect data it must already be known which domain of phenomena is the relevant focus of study. Indeed, even to identify data as relevant some prior judgment of the significance of various kinds of data must already have been made. Such judgments depend on previous knowledge, which may include prior theory about the domain under investigation. But this means that science cannot begin with pure observation and only afterwards proceed to the theoretical level. A background of knowledge, which may include theoretical knowledge, must *already* be in place before the work of data collection may even begin.

In the second place, naive inductivism fails to provide an adequate account of scientific theory formation. Scientific theories typically postulate the existence of unobservable theoretical entities (e.g., genes, atoms, electrons) whose behavior underlies the observable phenomena which scientists seek to explain. But while the simple inductive model may have some plausibility as an account of the discovery of low-level empirical laws, it has little plausibility as an account of the formation of theories about the unobservable entities that underlie the observed phenomena. The reason is that theoretical discourse about unobservable entities is typically couched in terms of theoretical vocabulary. Given this, it is not possible for scientists to infer by enumerative induction from premises which are stated in an observational vocabulary to conclusions, stated in a theoretical vocabulary, about unobservable entities. In short, naive inductivism does not have the resources to sustain an inference from observation to theory.

Third, naive inductivism is beset by a range of foundational problems, of which the most significant for present purposes is Hume's *skeptical problem of induction* (though the *paradoxes of confirmation* deserve mention). Since Hume's problem plays such a central role in the philosophy of scientific method, it is important to introduce the problem at this stage in the discussion. The problem is that of providing a rational justification for the use of inductive inference. Because induction is not a form of deductive inference, it is difficult to see how it may be justified on the basis of deductive logic. Nor does it seem possible to justify induction by appeal to the past success of induction, since that would be to use induction to support induction in a circular manner. Neither may induction be grounded in a principle of the uniformity of nature, since such a principle is unable to be justified in an *a priori* manner, and appeal to past uniformity would be circular. As will be seen when I turn to Karl Popper's *falsificationist account of method*, this problem has motivated the search for non-inductivist theories of method.

Before turning to the next theory of method, it is important to emphasize that the naive inductive method presented here is just that. It is a *naive* version of the inductivist method. More refined inductive methods are available. On the one hand, many inductivists favor forms of eliminative induction (e.g., Mill's methods) which take into account negative rather than only positive instances. On the other hand, inductivists have sought to develop an inductive logic and confirmation theory on the basis of the probability calculus. Such technical aspects of the inductive method are dealt with in other contributions to this collection. Rather than explore technical developments, I consider instead a somewhat more sophisticated inductivist theory of method which deals with the first two problems described above.

The hypothetico-deductive method

The second theory of method with which I deal is a more sophisticated inductive method which treats induction solely as a matter of justification. This is the *hypothetico-deductive method*, or, as it is also called, the *method of hypothesis*. The hypothetico-deductive method has enjoyed broad support, from nineteenth-century methodologists such as Jevons and Whewell to logical empiricists such as Hempel and Reichenbach in the twentieth century. According to the hypothetico-deductive view of method, theories are to be evaluated by testing the observational predictions which follow from them as deductive consequences. True predictions confirm a theory; false predictions disconfirm it.

Proponents of the hypothetico-deductive method take induction to serve as a method of *justification* rather than a method of *discovery*. The confirmation which a verified prediction provides for a theory constitutes non-conclusive inductive support for the theory, since the theory will typically have content which extends well beyond the specific prediction which supports it. But while the support provided by such evidence is inductive, there is no requirement that the theory be arrived at by means of an inductive inference. Arriving at a theory is a creative process which may involve intuition, inspired guesswork and imagination, as well as various kinds of deductive and inductive reasoning. What matters, as far as the justification of a theory is concerned, is how the theory fares when its observational consequences are subjected to scrutiny. And the relation of confirmation between verified prediction and theory, which is the only relation of relevance to the justification of a theory according to hypothetico-deductivists, is a relation that is inductive in nature.

The hypothetico-deductive method represents an advance over the naive inductive method with which I began. While it remains subject to foundational problems such as inductive skepticism, it avoids the first two problems with naive inductivism described above. The hypothetico-deductive method does not require that a scientific investigation begin with observation prior to theory. It is entirely possible for scientists who seek to explain a phenomenon to first propose a hypothesis and then to undertake observations in an attempt to verify the predictions entailed by the hypothesis. Nor is there any need for scientists to arrive at theories solely by means of an enumerative induction on the basis of observation. Scientists are free to postulate the existence of unobservable theoretical entities in the context of the development of scientific theories. Theoretical claims about such entities may receive indirect confirmation when the predictive consequences of the theories are subjected to empirical test.

But while the hypothetico-deductive method marks an advance over the naive inductive method, it faces several problems, of which two of the most telling are as follows. The first problem relates to the fact that theories are typically formulated in terms of universal generalizations. But it is impossible to derive a testable prediction from a universal generalization without specification of the initial conditions obtaining in the domain to which the generalization applies. In addition, it is usually the case that a range of further auxiliary hypotheses must also be employed about the objects in the domain, as well as the techniques and apparatus employed to investigate the domain. The result is that theoretical generalizations from which predictions are derived are not capable of being tested in isolation from all other empirical assumptions. The outcome of a prediction may therefore fail either to confirm or disconfirm the theory from which it is derived, since the initial conditions or auxiliary hypotheses might be responsible for the success or failure of the prediction. The ambiguous character of such tests means that the verification of a prediction does not necessarily provide a theory with genuine support. This problem provides an illustration, in the case of the hypothetico-deductive method, of the general problem of the underdetermination of theory by empirical data. In the specific form described here, the problem is known as the Duhem–Quine problem, after Pierre Duhem (1954: 180–200) and W. V. Quine (1953: 41), who brought the problem to the attention of philosophers of science.

While the first problem is an instance of a more general one, the second problem arises specifically with respect to the assumption that theories receive confirmation solely by way of their *predictive content*, as suggested by the hypothetico-deductive method. The problem may be illustrated by considering a scenario in which two or more alternative theories entail exactly the same empirically verified prediction. If the only source of empirical confirmation is by way of the verification of such predictions, then it is difficult to avoid the conclusion that all theories which entail the same predictive consequence receive exactly the same degree of confirmation from that prediction. But, without denying the importance of verified predictions, it should be clear that exclusive reliance on prediction in the confirmation of theories is problematic; for it assumes that there are no other factors of an evidential or methodological nature that might be of relevance to the empirical support of a theory. Yet it seems mistaken to assume, for example, that a coherent and an incoherent theory should be equally supported by the same prediction, or that both a theory and the theory conjoined with an irrelevant proposition should receive equivalent support from the same prediction. At the very least, it should be allowed that success in prediction may convey differential support to various theories in light of relevant differences in the theories and their circumstances. Just which factors should be taken into account is a matter of dispute among philosophers. But factors such as prior probability, fit with background knowledge, and explanatory power are worthy of note.

In recent years, an attempt to modify the hypothetico-deductive method that emphasizes the *explanatory role of hypotheses* has attracted considerable support. If a hypothesis can be shown to be the best available explanation of a set of phenomena, then this fact provides a reason to prefer that hypothesis to alternative hypotheses which provide inferior explanations.

Popper's falsificationist theory of method

The next account of method which we will consider is the *falsificationist theory of method* proposed by Karl Popper. Popper agrees with Hume that induction cannot be justified, and proposes instead a method which makes no use of induction. According to Popper, the method of science is a method of "trial and error – of conjectures and refutations" (Popper 1963: 46). Scientists propose bold, speculative theories in an attempt to explain phenomena which appear problematic in light of background knowledge and expectation. But rather than support such theories by means of experience, scientists seek to disprove theories by means of rigorous tests of the predictions that the theories entail. Those theories which fail such tests are rejected. Those theories which survive all attempts to refute them are then tentatively accepted as the best currently available.

Popper's theory of method may be thought of as an anti-inductivist version of the hypothetico-deductive method. Popper rejects the idea that scientific theories are arrived at by means of induction. Along with advocates of the hypothetico-deductive view of method, he regards the process of theory construction as an imaginative process of discovery incapable of rational reconstruction in terms of the logic or method of science. But, unlike the hypothetico-deductivists, he does not regard the positive outcomes of empirical tests as providing theories with inductive support. For not only does Popper reject induction as a method of theory formation, he rejects it also as a method of confirmation. Indeed, Popper's falsificationist philosophy of science is sometimes called "deductivism" because he rejects induction as a myth, and insists that deduction is all the logic that is needed for the methodology of science.

But while Popper rejects induction, this does not mean that there is no basis on which scientists may accept a theory. According to Popper, a theory receives support of a non-inductive nature as a result of passing empirical tests, and that provides a reason to accept the theory. Popper says that a theory which passes a test is "corroborated" by the test, a term he uses to avoid the inductivist overtones of "confirmation." Corroboration is not just a matter of the number of tests a theory passes. Theories receive greater corroboration the more testable they are. Indeed, Popper argues that the more improbable a theory is, the greater will be the corroboration it receives from a test that it does pass.

Popper's theory of method has itself been the subject of much critical discussion (e.g., Putnam 1974; Grünbaum 1976). Most controversial has been his outright dismissal of induction, which has met with sustained resistance on the part of inductivist philosophers of science. An important example of such resistance may be seen in an objection that is developed in detail by Wesley Salmon in his paper "Rational Prediction" (1981). Salmon focuses attention on the practical case in which one must decide on a course of action on the basis of a theory. Salmon asks how one is to choose between alternative theories which make conflicting predictions as a basis on which to act. According to Popper, the action should be based on the most highly corroborated of the competing theories. But this suggests that corroboration has inductive force. For while corroboration relates to a theory's past success in surviving tests, if it is to serve as a basis for future action then past survival of tests must be of relevance to what will take place in the future. But if corroboration is to be taken into account in determining a future course of action, this amounts to an inductive inference from past success in surviving tests to the likely continuation of such success into the future. It therefore appears that Popper's falsificationist philosophy of science rests at base on an assumption that is inductive in nature.

Another influential line of criticism of Popper derives from consideration of the history of science. Popper's theory of method suggests that theories are to be rejected the moment they entail a false prediction. But such ruthless elimination of theories does not appear to be the norm in actual science. Scientists often retain theories in the face of conflicting evidence. A failed prediction may simply be regarded as a problem for further investigation, rather than grounds for outright rejection of a theory. An established theory may be so thoroughly entrenched in a field of scientific activity that scientists are prepared to tolerate a range of discrepancies between theory and data. Indeed, they may adhere to a theory until a replacement theory has compiled an equally compelling track record and has shown outstanding additional promise. In the face of such behavior, the falsificationist might reply by distinguishing between the actual practice of science and the normative dictates of a theory of scientific method, and noting that actual practice need not always conform to the dictates of method. Alternatively, they might seek to show that resistance to apparent refutation of theories is associated with the introduction of testable modifications of theories, rather than conventionalist stratagems. But those philosophers of science who hold that the actual practice of science is of relevance to the normative methodology of science will be little inclined to adhere to the Popperian picture in the face of historical evidence of anti-falsificationist practice in science.

From paradigms to pluralism

Perhaps the most significant development in twentieth-century philosophy of science was the emergence in the 1960s of a historical approach to the philosophy of science. The influential work of T. S. Kuhn, as well as that of authors such as P. K. Feyerabend and N. R. Hanson, posed a challenge to orthodoxy in the philosophy of science, as represented by the logical empiricists and by Popper. Whereas philosophers had previously sought to characterize science by identifying its special method, the historical philosophers of science tended to see science as an evolving process which takes place in a variety of shifting circumstances. On the more historically attuned conception of science which has subsequently become prevalent, the notion of *a scientific method* plays a less pivotal role than it once did. Indeed, methodological factors are deemed to be of little more than rhetorical significance by practitioners of the sociology of science, which has arisen as one prominent response to the historical movement.

The historical movement in the philosophy of science was characterized by a number of themes in addition to increased sensitivity to the historical character of science. Historical philosophers of science tended to reject a sharp distinction between empirical fact and scientific theory. They argued that neither perceptual experience nor the observation statements prompted by such experience are independent of the scientific theories proposed to explain observed facts. They also emphasized the way in which scientific concepts and vocabulary are developed as part of the process of theory formation, and are subject to variation as theories themselves undergo variation.

Most importantly in the present context, historical philosophers of science challenged the idea of a theory-neutral scientific method that is invariant with regard to historical time-period and scientific discipline. This may be illustrated by means of Kuhn's views about method. In his masterwork *The Structure of Scientific Revolutions*, Kuhn characterized science in terms of periods of routine "normal science" based on an accepted scientific "paradigm," which is broken at intervals by periods of revolution in which the reigning paradigm is replaced by another. He suggested that the rules of scientific method depend on, and therefore vary with, the paradigm that is in place in a scientific community at a given time. However, in later work, Kuhn took the view that there is a set of methodological criteria of theory appraisal which are, by and large, invariant throughout the history of the sciences. The criteria – which include accuracy, consistency, simplicity, breadth, and fertility – are employed by scientists in the comparative choice between alternative theories. Kuhn claimed that the criteria "function not as rules, which determine choice, but as values, which influence it" (1977: 331). But while the criteria may provide scientists with a rational basis for choice of theory, they may enter into conflict in application to particular theories and may also be subject to alternative interpretations. As a result, appeal to the methodological criteria may fail to yield an unequivocal outcome. Scientists may choose to adopt opposing theories even though they adhere to a common set of methodological standards. (For related discussion, see Duhem 1954: 216–18.)

The flexibility of Kuhn's methodological values is complemented by a well-known theme from Feyerabend's "epistemological anarchist" philosophy of science in his book *Against Method*. According to Feyerabend, all methodological rules have limitations, and are therefore defeasible. Although Feyerabend typically expressed this view in more extravagant terms, the main thrust of his claim is simply that there may be particular circumstances in which any given methodological rule ought not to be applied.

In an attempt to restore objectivity to the methodology of science, Imre Lakatos (1970) proposed a synthesis of Popper's falsificationism with Kuhn's model of science. Instead of paradigms, Lakatos spoke of research programs, which are characterized by a "hard core" of laws embedded in a "protective belt" of auxiliary hypotheses. He argued that there is an objective basis for choice between competing research programs, since a progressive program that successfully predicts novel facts is to be preferred to a degenerating one that fails to predict such facts.

Despite their initial opposition to the historical approach, many philosophers of science have taken its central message on board. Whether the rules of method vary with paradigm or remain stable throughout theory change, the view that there is a plurality of methodological rules operative in the sciences is now widespread. Indeed, it seems to represent current orthodoxy. Philosophers who embrace such a pluralist conception of method typically hold that the scientific method does not consist of some single method, such as the hypothetico-deductive or falsificationist method. Rather, the method consists of a plurality of rules which may be employed in the evaluation of scientific theories or in the certification of empirical results. But, while some see such pluralism as being opposed to traditional theories of method, there are others who see in the variety of methodological rules the true nature of the inductive method.

The justification of method

No survey of the philosophy of method would be complete without consideration of the *problem of the justification of method*. The question of how a method, or a rule of method, is to be justified is a meta-level question about the method or the rule of method. It is a question, not of methodology, but of *meta-methodology*. It is at this level that the philosophy of method intersects with the central justificatory concerns of normative epistemology. For it is at this level that questions about the nature of the epistemic warrant of rules and methods must be confronted.

The problem of justification may be illustrated by considering the two major sources of justificatory problems which relate to method. The first source is one that we have already encountered. It is the problem of inductive skepticism, which is the problem of replying to the Humean skeptic by showing that induction may be given a non-circular justification. The second source is the problem of epistemological relativism, which arises from the methodological variation and pluralism highlighted by Kuhn and other historical philosophers of science. For if no single shared method exists, but rather a variety of potentially shifting methodological norms, then it is not clear that there may be any objective, rational basis for scientific theory choice or theory acceptance. Provided only that a theory satisfies standards which happen to be adopted by some scientist or group of scientists, virtually any theory is capable of being accepted on a rational basis. Without a shared method, there would seem to be no genuine difference between right and wrong in matters of theory choice.

Strictly speaking, the problems of skepticism and relativism are different problems. The skeptic denies the existence of objective knowledge or rationally justified belief. By contrast, the relativist allows that knowledge and rational belief exist, but asserts that they are relative to context. But while the problems of skepticism and relativism are distinct problems, both problems raise the question of how a given method is to be provided with a sound rational basis.

This way of looking at the problem of justification suggests that the solution may require a unified approach that addresses both the skeptic and the relativist. The literature on the problem of methodological justification is too vast to summarize here (but for extended coverage, see Nola and Sankey 2000). In the current philosophical climate, however, one particular unified approach is especially worthy of mention.

In recent years, a great many philosophers have embraced a naturalistic approach to philosophical matters. In the context of the problem of the justification of method, an *epistemological naturalist approach* has a great deal to offer. Such a naturalist sees philosophy as continuous with the sciences, so that epistemological matters are to be dealt with in a broadly empirical fashion. On such a naturalistic approach, the challenge of the epistemic skeptic is dissolved by noting that the skeptic sets unrealistically high standards of justification. No higher standards of justification exist over and above those employed in successful scientific practice or in common-sense interaction with the world. Indeed, it may even be possible to respond to the inductive skeptic using an inductive argument from the success of past induction in a manner that avoids vicious circularity (see Papineau 1992).

As for the threat of relativism, the naturalist may simply deny that no distinction may be drawn between right and wrong in relation to methodological matters. For it is possible to subject alternative methods to empirical test in an attempt to determine which methods work and which do not in actual scientific practice. Those methods which pass such tests may be accepted as the normatively correct methods to follow; those which fail such tests are to be rejected as incorrect, and should not be employed. This way of determining the warrant of a method is known as "normative naturalism" (Laudan 1996). It is a form of reliabilist epistemology, since it takes reliable performance as a crucial component in the warrant of a method.

It would be wrong to suggest that the naturalistic meta-methodology just outlined currently enjoys universal assent among philosophers of science (for dissenting views, see Worrall 1999 and Field 2000). Nevertheless, an analysis of the arguments which might be provided for or against such a position will take one straight to the heart of current discussion in the philosophy of method. For the question of whether the problem of justification may be resolved by epistemic naturalism is one of the key questions of concern to contemporary philosophers of scientific method.

See also Bayesianism; Confirmation; Critical rationalism; Evidence; The historical turn in the philosophy of science; Logical empiricism; Naturalism; Social studies of science.

References

Duhem, P. (1954) *The Aim and Structure of Physical Theory*, Princeton, NJ: Princeton University Press.

Feyerabend, P. K. (1993) *Against Method*, 3rd edn, London: Verso.

Field, H. (2000) "Apriority as an Evaluative Notion," in P. Boghossian and C. Peacocke (eds) *New Essays on the A Priori*, Oxford: Clarendon Press, pp. 117–49.

Grünbaum, A. (1976) "Is Falsifiability the Touchstone of Scientific Rationality? Karl Popper versus Inductivism," in R. S. Cohen, P. K. Feyerabend and M. W. Wartofsky (eds) *Essays in Memory of Imre Lakatos*, Dordrecht: Reidel, pp. 213–52.

Kuhn, T. S. (1977) "Objectivity, Value Judgment, and Theory Choice," in T. S. Kuhn, *The Essential Tension*, Chicago: University of Chicago Press, pp. 320–39.

—— (1996) *The Structure of Scientific Revolutions*, 3rd edn, Chicago: University of Chicago Press.

Lakatos, I. (1970) "Falsification and the Methodology of Scientific Research Programmes," in I. Lakatos and A. E. Musgrave (eds) *Criticism and the Growth of Knowledge*, Cambridge: Cambridge University Press, pp. 91–196.

Laudan, L. (1996) *Beyond Positivism and Relativism*, Boulder, CO: Westview Press.

Nola, R. and Sankey, H. (2000) "A Selective Survey of Theories of Scientific Method," in R. Nola and H. Sankey (eds) *After Popper, Kuhn and Feyerabend: Recent Issues in Theories of Scientific Method*, Dordrecht: Kluwer, pp. 1–65.

Papineau, D. (1992) "Reliabilism, Induction and Scepticism," *Philosophical Quarterly* 42: 1–20.

Popper, K. R. (1963) *Conjectures and Refutations*, New York: Routledge & Kegan Paul.

Putnam, H. (1974) "The 'Corroboration' of Theories," in P. A. Schilpp (ed.) *The Philosophy of Karl Popper*, La Salle, IL: Open Court, pp. 221–40.

Quine, W. V. (1953) "Two Dogmas of Empiricism," in W. V. Quine, *From a Logical Point of View*, New York: Harper & Row, pp. 20–46.

Salmon, W. (1981) "Rational Prediction," *British Journal for the Philosophy of Science* 32: 115–25.

Worrall, J. (1999) "Two Cheers for Naturalised Philosophy of Science," *Science and Education* 8: 339–61.

Further reading

Alan Chalmers's *What Is This Thing Called Science?* 3rd rev. edn (St. Lucia: University of Queensland Press, 1999) remains one of the best introductions to leading themes in the methodology of science. C. G. Hempel's *The Philosophy of Natural Science* (Englewood Cliffs, NJ: Prentice-Hall, 1966) is a classic which contains lucid discussion of key topics. Comprehensive coverage of the various theories of scientific method may be found in *Theories of Scientific Method: An Introduction* (Chesham: Acumen, 2007) by Robert Nola and Howard Sankey. The historical origins of the hypothetico-deductive method are explored by Larry Laudan in *Science and Hypothesis* (Dordrecht: Reidel, 1981). Popper's major treatise on method is *The Logic of Scientific Discovery* (London: Routledge, 1959). Criticism of Popper's method may be found in W. C. Salmon and A. Grünbaum (eds) *The Limitations of Deductivism* (Berkeley: University of California Press, 1988). Peter Lipton provides clear discussion of problems with enumerative induction and hypothetico-deductivism as a preliminary to his account of inference to best explanation in *Inference to the Best Explanation*, 2nd rev. edn (London: Routledge, 2004). An important discussion of methodological issues arising from the historical turn is Larry Laudan's *Science and Values* (Berkeley: University of California Press, 1984). Two excellent works on Kuhn are Alexander Bird's *Thomas Kuhn* (Chesham: Acumen, 2000) and Paul Hoyningen-Huene's *Reconstructing Scientific Revolutions: Thomas S. Kuhn's Philosophy of Science* (Chicago: University of Chicago Press, 1993).

THE ROUTLEDGE COMPANION TO PHILOSOPHY OF SCIENCE

Edited by
Stathis Psillos and
Martin Curd

Routledge
Taylor & Francis Group
LONDON AND NEW YORK

First published 2008
by Routledge
2 Park Square, Milton Park, Abingdon, OX14 4RN

Simultaneously published in the USA and Canada
by Routledge
270 Madison Ave, New York, NY 10016

This edition published in the Taylor & Francis e-Library, 2008.

Routledge is an imprint of the Taylor & Francis Group, an informa business

British Library Cataloguing in Publication Data
A catalogue record for this book is available from the British Library

Library of Congress Cataloging in Publication Data
The Routledge companion to philosophy of science / edited by Stathis Psillos and Martin Curd.
p. cm. -- (Routledge philosophy companions)
Includes bibliographical references and index.
1. Science--Philosophy. I. Psillos, Stathis, 1965- II. Curd, Martin.
Q175.R68 2008
501--dc22
2007020000

ISBN 0-203-00050-1 Master e-book ISBN

ISBN10: 0-415-35403-X (hbk)
ISBN10: 0-203-00050-1 (ebk)
ISBN13: 978-0-415-35403-5 (hbk)
ISBN13: 978-0-203-00050-2 (ebk)