Philosophy of Science

The phrase "philosophy of science" can be used most broadly to describe two different, though related, sorts of inquiry. On the one hand it can be used to describe the philosophy of particular sciences, such as the philosophy of physics, biology, or economics. On the other hand, it can be used to describe the study of epistemological issues in science more generally. Although an increasing majority of work in the philosophy of science is being done in the philosophy of particular sciences, it is this latter construal of the philosophy of science that remains the heart of the field and is the focus of this entry.

Scientific methodology

In a tradition that can be traced back to John Stuart Mill (1806–1873) and Francis Bacon (1561–1626), many have taken the scientific method to be inductive. An inductive inference is *ampliative* (i.e., the content of the conclusion goes beyond the content of the premises) and *nondemonstrative* (i.e., all true premises do not guarantee a true conclusion; at best they render the conclusion more probable). For example, suppose that one has observed a large number of mammals and every kind of mammal that one has observed has teeth; from this evidence one might make the inductive generalization that all mammals have teeth. It is possible, however, that the next mammal one observes (say, an anteater) might turn out not to have teeth. The fallibility of inductive inferences is often referred to as Hume's problem of induction, after the philosopher David Hume (1711–1776).

Karl Hempel (1905–1997) argues that the scientific method begins not with observations but with hypotheses. According to this hypothetico-deductive method one deduces certain observational predictions from the hypothesis and then rigorously tests them through further observation and experimentation. If the predictions are borne out, then the hypothesis is confirmed. Thus Hempel's method is still broadly inductive. Although the conclusion of an inductive argument is not certain, one would like to determine quantitatively how probable the conclusion is, given its premises (the evidence). The logical positivist Rudolf Carnap (1891–1970) sought to develop such a logic of confirmation. Other models of confirmation, such as Bayesian and bootstrapping models, are reviewed in John Earman's *Testing Scientific Theories* (1983).

Karl Popper (1902–1994) insists that the scientific method is deductive, not inductive. Observation always requires a prior point of view or problem. Like Hempel, Popper believes science begins with a bold hypothesis or conjecture. The way in which the scientist comes to the hypothesis (context of discovery) is irrelevant (e.g., it could come to the scientist in a dream); all that matters is the way in which it is tested (context of justification). Unlike Hempel, Popper does not think that hypotheses can be confirmed. If the observational prediction is borne out, deductively the scientist is unable to conclude anything (to conclude that the hypothesis is confirmed is to commit the deductive fallacy of affirming the consequent). If, however, the predictions are falsified, then, by the valid deductive inference *modus tollens* if p then q, not q, therefore not p) one can conclude that the hypothesis is falsified. Hence, Popper's method is known as falsificationism. According to Popper, the scientist should not seek to confirm theories but rather, refute them. A theory that has survived repeated attempts of falsification—especially in those cases where it has made risky predictions — has been corroborated, though not confirmed. On this view, a theory is demarcated as scientific if there are observational conditions under which one would be willing to reject the theory as falsified.

As a matter of historical fact, however, scientists typically do not abandon their theories in the face of falsifying evidence. Furthermore, in many cases it turns out to be sound scientific judgment to continue developing and modifying a theory in the face of recalcitrant evidence. In response to these sorts of difficulties, Popper's student, Imre Lakatos (1922–1974), developed a sophisticated falsificationism known as the "methodology of scientific research programs." For Lakatos, instead of evaluating an individual theory or modification of a theory as scientific or ad hoc, one should evaluate a whole series of theories developed over time. This series, called a *research program,* consists of a *hard core,* which defines the research program and is taken to be irrefutable, and a *protective belt,* which consists of auxiliary hypotheses and background assumptions to be modified in the face of falsifying data, thereby protecting the hard core. According to Lakatos, a research program is demarcated as scientific if it is progressive — that is, it continues to make new predictions that become corroborated. Once a research program ceases to make new corroborated predictions it becomes degenerative and its hard core should be abandoned.

Paul Feyerabend (1924–1994) was a close friend of Lakatos and also a student of Popper's. In his book *Against Method* (1978) he denies that there is such thing as the scientific method. He writes, "the idea of a fixed method, or a fixed theory of rationality, rests on too naïve a view . . . there is only *one* principle that can be defended under *all* circumstances.... It is the principle: *anything goes* " (pp. 27–28). Feyerabend's view is known as *epistemological anarchism.*

Scientific rationality and theory change

Beginning in the early 1960s there was a shift away from concerns about scientific methodology towards concerns about scientific change. This shift was in large part due to the publication in 1962 of Thomas Kuhn's (1922–1996) *The Structure of Scientific Revolutions.* Kuhn argues that the philosophy of science ought to be the product of a careful examination of the history of science. This involves recognizing the integrity of the science within its own time and not simply viewing it in relation to one's contemporary perspective. This new historiographical approach leads Kuhn to reject much of traditional philosophy of science: the confirmationist and falsificationist accounts of theory evaluation, the view that science is cumulative, the distinction between context of discovery and context of justification, and the idea of a crucial experiment.

Kuhn argues that science is characterized by three sorts of phases: pre-paradigm science, normal science, and revolutionary science. Central to understanding these phases is his notion of a *paradigm,* which he uses in two primary ways. First, he means an exemplar, a concrete problem solution or scientific achievement that serves as a model for solving other scientific problems (e.g., the planetary dynamics laid out in Isaac Newton's *Principia*). Second, and more broadly, he means by paradigm a disciplinary matrix, which includes not only exemplars, but laws, definitions, metaphysical assumptions, and values (e.g., Newton's dynamical laws, the definitions of mass and space, and the mechanical philosophy). The paradigm determines what is to count as an acceptable scientific problem and an acceptable scientific solution. In the process of normal science, anomalies emerge that resist solution within the framework of the paradigm; if these anomalies persist and proliferate, they can lead to a state of crisis. Revolutionary science is described as "those noncumulative developmental episodes in which an older paradigm is replaced in whole or part by an incompatible new one" (p. 92). Kuhn refers to the pre- and post-revolutionary periods of normal science as incommensurable, and says that there is a sense in which scientists from different paradigms work in different worlds. Kuhn polemically refers to the conversion from the old to the new paradigm as being analogous to a Gestalt switch or religious conversion. Ian Barbour draws analogies between Kuhnian paradigms and religious paradigms in *Religion and Science* (1997).

In the 1969 postscript to *The Structure of Scientific Revolutions* and in the article "Objectivity, Value Judgment, and Theory Choice" (1977) Kuhn responds to charges that his account of science makes science irrational and leads to relativism. Against the charge of irrationality, Kuhn notes that values (such as predictive accuracy, simplicity, internal consistency, and coherence with neighboring theories) provide scientists with a shared basis for theory choice. Against the charge of relativism, Kuhn notes that ultimately paradigms are to be evaluated by their ability to set up and solve "puzzles." In this sense Kuhn does believe that there is objective progress in science: Newton solves more puzzles than Aristotle, and Albert Einstein more puzzles than Newton. What Kuhn rejects is realism, which claims that there is a coherent direction of ontological development and that science is getting closer to the truth.

Subsequent philosophers of science influenced by Kuhn developed different strands of his thought in different directions. Feyerabend, who developed an incommensurability thesis around the same time as Kuhn, came to later embrace the label of relativist. Others, such as Larry Laudan, sought to preserve the rationality of science against the threat perceived in Kuhn's holist picture of scientific change. According to Laudan, a closer look at the history of science shows not a wholesale exchange of one paradigm for another, but rather the components of the disciplinary matrix (e.g., methods, values, and ontology) being negotiated individually. Regarding theory choice, he writes, "there is enough common ground between the rivals to engender hope of finding an 'Archimedean standpoint' which can rationally mediate the choice" (1984, p. 75). He calls this alternative view the reticulated model of scientific change.

Scientific realism versus antirealism

The labels *realism* and *antirealism* are each used to cover a wide spectrum of views. The main positions can be roughly distinguished by their answers to three questions: (1) Is there a mind-independent world? (2) What is our epistemic access to that world? (3) What is the aim of science? Realists (along with many antirealists) accept the existence of a mind-independent world. Those antirealists who deny this advocate some form of idealism. While realists tend to be optimistic about epistemic access to the world, antirealists argue in various ways that this optimism is unwarranted. Realists typically see the aim of science being truth, whereas antirealists argue the aim is something less.

At one end of the realist spectrum is *naïve realism* — the view that science is a perfect, undistorted mirror of the mind-independent world and that scientific theories are literally true. More sophisticated versions of realism, such as the view of Ernan McMullin, hold that realism means the long-term success of a scientific theory gives reason to believe that something like the entities and structures postulated by the theory actually exist (p. 26). According to McMullin, an important part of the aim of science is the development of fruitful metaphors. Many have argued for realism on the grounds that it provides the best explanation for the success of science; the widespread success of science would be "miracle" if scientific theories were not at least approximately true (Boyd 1984, Putnam 1975). Others argue that the proper question for realism is not whether some theory is true or approximately true, but whether some entity exists. According to Ian Hacking's entity realism, one can conclude, for example, that electrons exist because researchers experimentally build devices that use electrons to investigate other parts of nature. Between theory realism and entity realism is another view known as structural realism. This view, which John Worrall attributes to Henri Poincaré(1854–1912), affirms a mind-independent world but takes epistemic access to that world to be limited to its structural features. Thus, there is a continuity of structure across theory change despite radical changes in ontology. Although what is meant by *structure* is not entirely clear, in the physical sciences it is typically taken to be the structures expressed in the mathematical formalism of the theory.

Challenges to realism come from many sources and have led to a variety of antirealist views. Both Kuhn (1962) and Laudan (1981) argue that the history of science undermines realism. Kuhn's view can be classified as a form of instrumentalism, according to which scientific theories are merely useful instruments for making predictions and solving problems. Other antirealist views, such as Bas van Fraassen's constructive empiricism, come out of the empiricist tradition. According to van Fraassen (1980), science only aims to give theories that are empirically adequate and a theory is empirically adequate if what it says about observable things is true, that is, if it saves the phenomena (p. 12). On this view, one is not compelled to accept the existence of unobservable entities, such as electrons.

A third strand of antirealism, known as *social constructivism,* comes from sociology. The social constructivist seeks to understand scientific practice in the laboratory in a manner similar to an anthropologist seeking to understand a foreign culture. Social constructivists, such as David Bloor of the Edinburgh School, reject the philosophical understanding of knowledge as justified true belief, and instead take knowledge to be whatever is collectively endorsed by a particular group of people at a particular time (p. 5). This makes social constructivism a form of relativism. It is called *constructivism* because it takes scientific knowledge and facts to be constructed rather than discovered. Stronger and weaker versions of this view are obtained depending on whether this process of constructing scientific knowledge is, or is not, taken to be purely social. Arthur Fine, who argues that social constructivism has important methodological lessons for the philosophy of science (1996), himself rejects both realism and antirealism. Instead Fine advocates a minimalist position he calls the *natural ontological attitude,* which prescribes accepting the claims of science in the same way that one accepts the evidence from one's senses, without adding any additional claims such as "and it is really true" or "and it is only a useful fiction" (1986, p. 127).

Feminist philosophies of science

Since the 1970s, many feminist philosophers, historians, and scientists have been asking why there have traditionally been so few women in science and whether certain sexist, racist, or nationalist biases have shaped the practice and content of science. Detailed case studies, a representative sample of which can be found in Janet Kourany's *The Gender of Science* (2001), reveal many ways in which such biases have affected science. In response to these findings, many feminists sought to develop a new epistemology or philosophy of science. Following Sandra Harding (1986), feminist philosophies of science can be roughly divided into three traditions: feminist empiricism, feminist standpoint theory, and feminist postmodernism.

Helen Longino, whose work falls largely within the feminist empiricist tradition, introduces a view known as *contextual empiricism.* Longino sees empirical data as constraining, but nonetheless underdetermining, theory choice. This gap between theory and evidence is bridged by value-laden background assumptions belonging to a particular context. These contextual assumptions are one way biases can enter science. Longino criticizes traditional portrayals of the scientific method as individualistic. Instead she sees the objectivity of science being secured by its social character (e.g., peer review, replication of experiments, and an openness and responsiveness to criticism). She argues that diversity in science is important for making these often invisible assumptions explicit and open to criticism.

Harding takes this point about diversity in science a step further in her feminist *standpoint theory.* In contrast to empiricism, standpoint theory argues that the legitimacy of the knowledge claim depends on the social identity of the knower. Harding writes, "women's subjugated position provides the possibility of more complete and less perverse understandings. Feminism . . . can transform the perspective of women into a 'standpoint'—a morally and scientifically preferable grounding for our interpretations and explanations of nature" (p. 26). One standard criticism that Harding considers is whether there is such a thing as a "feminist standpoint" that cuts across all classes, races, and cultures.

Donna Haraway's work *Simians, Cyborgs, and Women* (1991) exemplifies the feminist postmodernism tradition. Haraway rejects the idea of single feminist standpoint and instead argues that all knowledge is locally situated. Like Longino, Haraway offers an alternative account of objectivity. She writes, "Feminist objectivity is about limited location and situated knowledge, not about transcendence and splitting of subject and object. In this way we might become answerable for what we learn how to see" (p. 190). Although Haraway's view shares some affinities with social constructivism, she explicitly rejects the label "relativist." She explains, "the alternative to relativism is not totalization and single vision.... [Rather, it is] partial, locatable, critical knowledges. . .. Relativism is a way of being nowhere while claiming to be everywhere equally. The 'equality' of positioning is a denial of responsibility and critical enquiry" (p. 191). Underlying many of these feminist philosophies is a central concern for the social and ethical implications of science.

Scientific explanations and laws

The most influential account of scientific explanation is Hempel's *covering law model.* On this model, explanations are understood as arguments in which the explanandum (the event, feature, or law to be explained) appears as the conclusion of an argument. The premises of the argument must contain at least one universal or statistical law used essentially in the derivation, and empirically verifiable statements describing particular facts or initial conditions. If the argument is deductive and involves a universal law, it is called a *deductive nomological explanation*; if the argument is inductive and involves a statistical law, then it is called an *inductive-statistical explanation.* For example, suppose one wants to understand why an ice skater's angular velocity increases as she draws her arms in during a spin. The explanation would show that this event can be logically deduced from premises involving the law of conservation of angular momentum and statements such as her initial angular momentum was nonzero and her moment of inertia was reduced by drawing her arms in.

Several philosophers and historians have objected that Hempel's conditions are neither necessary nor sufficient for a scientific explanation. The most famous counterexamples fall into the categories of either irrelevance (although the event follows from the premises, as a matter of fact those premises are irrelevant to the explanation of the event) or symmetry (if the law involves a biconditional or equation then one can switch one of the premises with the conclusion and "explain" things such as why a flagpole is a certain height in terms of the length of its shadow). These sorts of problems have led philosophers largely to abandon Hempel's model and propose new alternatives. To handle the problems of irrelevance and symmetry, Wesley Salmon (1925–2001) introduces a causal model explanation, whereby to explain an event is to identify the causes of that event. Alternatively, van Fraassen in his pragmatic account of explanation embraces the possibility that the length of a shadow may explain the height of a pole. For van Fraassen (1980), an explanation is always relative to a particular context. Yet another model of explanation is provided by Philip Kitcher (1981), who understands explanation to be a unification of diverse phenomena by means of a common underlying structure or small number of processes. He sees Charles Darwin's theory of evolution as illustrating this model of explanation. The link between explanation and unification is challenged by Margaret Morrison in her book *Unifying Scientific Theories* (2000).

From reductionism to theoretical pluralism

Reductionism can be construed as a thesis about ontologies, laws, theories, linguistic expressions, or some combination of these. Considered as a relationship between scientific theories, it can be taken as a synchronic relation between two concurrent theories belonging to different levels of description or a diachronic relation between a historical predecessor theory and its successor. The classic formulation of theory reduction is due to the logical empiricist Ernest Nagel (1901–1985), who takes it to involve the logical derivation of one theory from another. More specifically, "a reduction is effected when the experimental laws of the secondary science . . . are shown to be the logical consequences of the theoretical assumptions . . . of the primary science" (p. 352). The standard example is the reduction of thermodynamics (secondary science) to statistical mechanics (primary science). In the physical sciences, reductionism is more often taken to be a correspondence between two theories under certain conditions, typically characterized by the limit of some quantity. As Thomas Nickles notes, this view is "best described by 'inverting' the usual concept of reduction, so that successors are said to reduce to their predecessors . . . under limiting operations" (p. 181). For example, special relativity is said to reduce to Newtonian mechanics in the limit of small velocities.

Challenges to reductionism have come from detailed case studies of the relations between particular scientific theories. One recurring challenge is known as the problem of *multiple realizability*. For example, in reducing Mendelian genetics to molecular biology, as Alexander Rosenberg points out in his 1989 "From Reductionism to Instrumentalism?”, one discovers that a single Mendelian trait can be realized by a variety of molecular mechanisms, and furthermore, the same molecular mechanism can produce different Mendelian characteristics. Another set of challenges arises when the reducing theory is statistical (such as statistical mechanics or quantum mechanics) and the reduced theory is not as Lawrence Sklar indicates in this 1999 essay, "The Reduction (?) of Thermal Dynamics to Statistical Mechanics." These sorts of difficulties have led many to reject reductionism and instead argue for theoretical pluralism, or the so-called disunity of science. According to pluralism, each scientific theory has its own proper domain of applicability. In her book, *The Dappled World* (1999), Nancy Cartwright raises the possibility that "nature is governed in different domains by different systems of laws not necessarily related to each other in any systematic or uniform way" (p. 31). This view has been criticized on the grounds that it forfeits the benefits that come from examining inter-theoretic relations. The question of the unity or disunity of science remains a controversial topic.

See also Explanation; Philosophy of Science, History of; Positivism, Logical

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