PHILOSOPHY OF SCIENCE: WHAT ONE NEEDS TO KNOW

by Philip Clayton

Abstract. This introduction to the philosophy of science offers an overview of the major concepts and developments in contemporary theories of science. Strengths and weaknesses of deductive, inductive, and falsificationist models of science are considered. The "Received View" in the theory of science is contrasted with Kuhn's paradigms and Feyerabend's "anything goes," leading to an examination of the merits of a research program–based approach. After touching on the sociology of science, postmodernism, and the feminist critique, the article concludes with a summary, in six theses, of the implications for religion/science.

Keywords: deductive models; falsification; inductive models; paradigms; philosophy of science; religion/science parallels; research programs; theories of rationality.

Philosophy of science means nothing more esoteric than reflection on the nature of scientific theories and scientific practice. Such reflection does not *control* scientists: scientific activity is by and large self-correcting, and scientists certainly do not need philosophers to tell them what to do! Nonetheless, the philosophy of science does help to *describe* what it is that scientists do. Theories of science may also help in the social critique of science, since they can strongly affect our ideas about the authority of scientists and their theories. In the debate between science and religion it is especially essential that we stop to think about what constitutes scientific activity and what the status of scientific theories is.

Let me state my thesis clearly at the outset. In what follows I shall try to show that arguments that dichotomize between scientific theories and religious theories on the one hand (the so-called two-worlds approach) and arguments that seek to identify them on the other are based on a false understanding of the scientific enterprise and its results. Although I shall

Philip Clayton is Associate Professor of Philosophy, California State University, Sonoma, Rohnert Park, CA 94928 He may be reached via E-mail at claytonp@sonoma.edu.

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not stop to spell out the details, it should become clear by implication that the theoretical activity of scientists reveals crucial parallels with that of theologians and religious believers—even though we cannot identify the two activities completely.

MAJOR SCHOOLS IN THE PHILOSOPHY OF SCIENCE

Before the advent of carefully controlled empirical science, a "science" (Greek *epistemē*, Latin *scientia*) meant a series of deductions made from a priori truths. Also known as "natural philosophy," this activity could derive its starting points from fundamental intuitions, from revelation, or from the "natural light" of reason. Indeed, we find remnants of such approaches well into the modern age of science, as in Galileo's deduction that there is only one tide per day, Hegel's treatment of electricity in terms of spirits, and the use of "entelechies" or inner strivings in biological explanations as late as the 1880s.

Modern science as we know it was born with the advent of the *inductive approach*. The power of inductive reasoning lies in its stress on observation as well as in its acceptance of the methodological rule that scientists must generalize from empirical observations to overarching natural laws. Over the last one hundred fifty years it has proved possible to develop rather exact measures of inductive strength, so that researchers can determine the status of particular inductive generalizations (Mill 1865; Jevons 1890; Carnap 1951). The popular view of science—and the view of many practicing scientists—still maintains that the defining mark of scientific practice is careful observation, followed by inductive inferences to general laws that describe the phenomena observed.

Inductivism faces serious problems, however. In the eighteenth century, David Hume showed that no finite number of observations ever really justifies the leap to claims of the form "*All x* are *y*" (see Hume [1777] 1975). As much as we may feel inclined psychologically to believe that the future will be like the past or that some sort of "causal glue" must bind together events that are constantly conjoined in our experience, such leaps of inference tell us more about our psychology than about any real connections in the world. Most philosophers of science today hold that Hume's problem of induction is insoluble (e.g., Lipton 1991).

The agenda for the last sixty-five years in the philosophy of science was set by Karl Popper's famous move from induction to *falsification* as the defining mark of scientific practice (Popper 1965; Popper 1968). What "demarcates" science from nonscience is the practice of formulating risky hypotheses and then doing one's best to falsify them. Each scientific hypothesis must make some testable prediction about what will be found in the world; if this prediction is later falsified by experimentation, the hypothesis should be rejected. Clearly Popper's proposal has some teeth in it for students of religion: it tells scientists to formulate risky hypotheses; it demarcates science and theology; and it describes what religious believers *would have to do* if they want their beliefs to be taken seriously by scientists. At the same time, note that it does not allow us to say that today's scientific theories are actually true or even probably true; it allows us to conclude only that we have *not yet been able* to falsify them, despite our best efforts.

The falsificationist model was associated with another important view of science in the middle of this century, a view so dominant that Frederick Suppe labeled it "the Received View" (Suppe 1974, Introduction). The Received View held that scientific theories and scientific rationality could be clearly delineated from all other human rational endeavors. It assumed a sharp distinction between theoretical and observation sentences: some sentences correctly express our immediate experience of the world, and they are the ones we use for testing theoretical proposals made by scientists. The ideal for science is to formulate "lawlike" or "deductive-nomological" explanations (Hempel 1965). Such explanations specify the antecedent conditions and the "covering laws" that pertain to a given situation. With these in place, the thing to be explained (the *explanandum*) must follow deductively from the conditions and the laws; a phenomenon is ideally explained only when this deduction from universal laws obtains. This model, which still is widely accepted, clearly presents a steep—and presumably unattainable—set of standards for religious accounts of divine action!

Beginning around 1950, however, a growing number of questions and doubts were raised about the Received View. It became clear that science has the goal of understanding; it is not enough merely to formulate laws unless one also has insight into how to apply them (Toulmin 1961). Theorists also realized that "all data are theory-laden" (Hanson 1958), such that scientists are engaged not merely in "seeing" the world but in "seeing it as" something in particular—with the whole range of interpretive issues and presuppositions that this involves. At the same time, Hilary Putnam challenged the theory/observation distinction: theoretical expectations condition what we see (Putnam 1962). Likewise, W. V. O. Quine challenged the "two dogmas of empiricism" (Quine 1953). Experience, he correctly saw, impacts the "web" of our beliefs only at the edges; the "inner" convictions are highly resistant to change, since they are not directly based on or derived from observation. Quine wrote,

> No particular experiences are lined up with any particular statements in the interior of the field, except indirectly through considerations of equilibrium affecting the field as a whole. . . . Any statement can be held true, come what may, if we make

drastic enough adjustments elsewhere in the system. (Quine 1953, 43)

Ian Barbour summarized this new view of science:

> There are *no bare uninterpreted data*. Expectations and conceptual commitments influence perceptions, both in everyday life and in science. Man supplies the categories of interpretation, right from the start. The very language in which observations are reported is influenced by prior theories. The predicates we use in describing the world and the categories with which we classify events depend on the kinds of regularities we anticipate. (Barbour 1974, 95; emphasis in original)

These insights are now almost universally accepted in the philosophy of science. It follows that the Received View, in its strict form, is no longer acceptable as a description of (or prescription for) science. The hotly debated question then becomes: How radically should the new view of science diverge from the Received View? If the "objective" picture of scientific activity and theories is untenable, how far toward the "subjective" end of the continuum should we place the scientific project? Is science merely the product of the will of the scientist, the scientific community, and the cultural and historical "location" in which they exist?[1] Are scientific theories arbitrary constructions, or do they give a (more or less) accurate picture of the world as it exists apart from the human knower? If the latter, can we be realists about today's scientific theories? Is science progressing toward truth, or do its theories merely prove useful to humans at a given time and place, given a particular set of interests?

Undoubtedly, the "subjective" side of this continuum has received far more press and support over the last decades. The "sociology of science," feminist critiques of science, and so-called postmodern views of science[2] all represent moves in this direction. Nonetheless, it must be emphasized that many theorists of science—and, even more significant, many practicing scientists—still understand science in a manner very much like the Received View. *Apparently* there has been a tendency for scholars trained and working in the humanities, excited by the acknowledged difficulties with the Received View, to rethink natural science in their own image, giving a humanities-based account of science that few practicing scientists themselves would accept.[3] What then are the major options in the philosophy of science today?

1. One direction to go is to draw radical conclusions from the doubts about the Received View. Thomas Kuhn's famous book, *The Structure of Scientific Revolutions* (1962), is generally taken as a prime example of this tendency. Kuhn argued that science consists of multiple "paradigms," each of which is composed of a network of commitments and a set of approaches and problems. The paradigm determines for us what there is in the world; it tells the scientist what he or she is seeing when recording observations. Different paradigms are "incommensurable";

they represent different worlds with no overarching perspective for comparing them. The history of science, Kuhn holds, is characterized by long periods of "normal science," in which scientists work within an established paradigm. As an increasing number of anomalies arise, more and more scientists come to have misgivings about the paradigm. When an attractive alternative paradigm is supplied, a "scientific revolution" —a radical shift from the old paradigm to a new one—may occur, such as in the move from the earth-centered cosmology of Ptolemy to the heliocentric model of Copernicus. This movement is not rationally determined but looks much more like a religious conversion in which one scientist seeks to convert others to his or her way of seeing:

> The competition between paradigms is not the sort of battle that can be resolved by proofs. . . . Before they can hope to communicate fully, one group or the other must experience the conversion that we have been calling a paradigm shift. Just because it is a transition between incommensurables, the transition between competing paradigms cannot be made a step at a time, forced by logic and neutral experience. Like a gestalt switch, it must occur all at once or not at all. (Kuhn 1962, 147, 149)

In later works (e.g., 1977) Kuhn has made clear that he does not reject overarching values, which serve as general criteria for theory choice in science, nor does he assert that every paradigm shift is equally rational: we may judge that some scientific developments are more or less rational than others. Nonetheless, the die was cast by Kuhn's early book, and many other philosophers of science were happy to extend his views where he feared to tread.

2. Paradigmatic for the more radical views is the book *Against Method* by Paul Feyerabend (1975). Feyerabend advocates the view that, in scientific practice, the only rule we need is "anything goes." I have summarized his views in *Explanation from Physics to Theology*:

> In Feyerabend's hands, the situational variance of *contextualism* reduces to the complete relativism of *conventionalism*. Given Kuhn's incommensurability, "what remains are aesthetic judgments, judgments of taste, and our own subjective wishes." In Kuhn's science, according to Feyerabend, there is no longer any distinction between science and art ([Feyerabend 1975] p. 228n2); we might as well abolish the honorific connotations attached to the word *science*. Science becomes "an attractive and yielding courtesan" (p. 229) and should be acknowledged and treated as such. More specifically, Feyerabend calls for "epistemological anarchism" in science, the position that "anything goes" (pp. 21, 187n, 189, 296). Proclaiming himself a Dadaist, Feyerabend points out the consequences of abolishing the science/nonscience distinction: we should view the Bible as an alternate cosmology (p. 47n1); voodoo can enrich our physiology (p. 50); acupuncture may be preferable to modern medicine (p. 51); we should break our methodological rules whenever possible, as Galileo did (appendix 2); and finding a satisfactory theory depends, for instance, on having a satisfactory sex life (p. 174). (Clayton 1989a, 45)

3. The emphasis on the human element in science has encouraged a variety of thinkers to evaluate science from the perspective of its impact on humans, other living creatures, and the environment as a whole. This new willingness to question science as an institution, to insist on asking about "science *and* human values,"[4] remains one of the most important developments of the last decades, whether or not one shares their skepticism about knowledge and progress toward truth in science. The philosophy of science and the critique of science have met, for example, in the work of feminist theorists of science such as Sandra Harding and the late Merrill Hintikka (1983), Ruth Bleier (1986), and Carol Gilligan (1988). These authors have been able to show the role that male categories of thought and styles of interacting have played in determining scientific practice—the subjects that get studied, the ideals of scientific progress, and even the actual results of research. Stressing the need for community and for working together instead of in competition, they have begun to develop important new models of science that diverge in many respects from older views.

A Synthesis between the Extremes: The Research-Programs Model

Where does this all leave us? Popper's falsification idea and the Received View seem inadequate: experience does not falsify conclusively, and the separation of theory and observation often proves impossible. On the other hand, there are reasons to think that Dadaism is not the best or most accurate description of scientific practice and that an adequate sex life may not be the best predictor of scientific success. Can the two views be mediated in a way that appropriates what is right about both while avoiding their errors? A number of thinkers have suggested that something like the theory of science developed by Imre Lakatos (1978) is able to accomplish this task (Murphy 1987, 1990; Clayton 1989a, 1989b; Hefner 1993; Russell 1996). Lakatos defended the view of "sophisticated falsificationism": falsification is seldom conclusive and occurs only over time. He wrote:

> It is not that we propose a theory and Nature may shout NO; rather, we propose a maze of theories, and Nature may shout INCONSISTENT. [He later put it:] Nature may shout *no*, but human ingenuity . . . may always be able to shout louder. (Lakatos 1978, 1 : 45, 111)

"Crucial experiments" are only recognizable by hindsight. The unit by which we examine science should not be the individual theory or an entire "paradigm" but individual *research programs* (RP). An RP consists primarily of a hard core—a series of claims so fundamental to it that without them it would be a different RP. The hard core includes a negative heuristic, or what is excluded by the RP, and a positive heuristic, or long-term research policy. In the face of countervailing evidence it is acceptable for scientists to introduce auxiliary hypotheses in order to incorporate new data and information. The hard core of an RP is surrounded by a "protective belt" of auxiliary hypotheses, which protects it against the "ocean of anomalies" that it invariably faces.

Terminology aside, the research-program approach admits the ambiguities in scientific decision making: decisions between theories are made by fallible (and sometimes prejudiced) persons and only over an extended period of time. At the same time, it still insists that many "problemshifts" in scientific research are "degenerating," and only some are genuinely progressive. An RP is progressive when it is able to predict unexpected facts that are later verified; it is degenerating when it must strain the new evidence in order to make the evidence fit with its hard core.

The key insight of recent philosophy of science, then, is *the replacement of a justificatory (or inductive) with a fallibilist epistemology*. Science does allow for rational evaluation between theories, and we can speak of progress in science as we reject unsuccessful RPs and add to the sophistication of current ones. The significance of this shift for theologians and students of religion can hardly be overstated, for theology could never compete with the sciences as an inductive endeavor derived solely from empirical facts and observations. Yet it *can* be carried out in a *fallibilist* manner. If fallibilism and openness to criticism represent the heart of scientific rationality, then there may be much opportunity for genuine collaboration between science and theology (e.g., Pannenberg 1993).

Conclusions

I conclude with six theses that summarize the results of the above discussion and begin to spell out some of its implications for the field of religion:

1. Science is not foundationalist or purely objective. Such notions as "value-free science," pure observations, and conclusive falsifications of theories should be rejected as myths of the past. Instead, there is an important element of *holism* in scientific practice: scientific theories are in many respects like webs on which the world impinges only from the outside.

2. Yet science does not thereby become relative, such that "anything goes," theories create their own data, and testing of scientific theories becomes impossible. Such allegations, often associated with "postmodern" accounts of science, do not correctly describe what scientists do and how science operates.[5]

3. The better account of scientific rationality is not inductivist but falsificationist, even though strict falsification has turned out to be a chimera. Hence, I have suggested, the core of scientific rationality is *fallibilism*: the attempt to formulate hypotheses that are subject to testing (i.e., to being shown wrong) and the concommitant effort to do one's best to show them wrong. Those that stand up most strongly to our best efforts at testing are the ones we have the best reason to accept.

4. It is problematic to speak directly of the truth of our current scientific theories. The switch from inductive strength (and inductivist theories of truth) to falsification means that the highest epistemic status we can attribute to a theory or a set of theories is "not yet falsified, despite our best efforts."

5. Science is fundamentally about explaining the world. Hence, we find some important parallels with the explanatory components of religion and theology. Nonetheless, the differences must be kept in mind: religious beliefs may explain the world for believers, but they also constitute *programs for living* that function very differently from scientific theories (see Clayton 1989b).

6. From a theory of science perspective, the two fields can best be mediated when we understand the explanatory task within the framework of the broader human need for *understanding*. This move suggests a greater stress on the social sciences than is often acknowledged in religion/science discussions, for these are the disciplines in which the human endeavor to *make sense of one's total experience* plays the dominant role. The quest for scientific explanations, on one hand, and the formation and use of religious beliefs, on the other, might then be seen as two of the diverse ways in which humans attempt to make sense of their total experience. (Note that this is not a dichotomy: scientists *also* aim to understand the world, and one function of religious belief is to explain the world and what we find in it.)

Each of the models covered in this article would suggest a very different way of approaching questions about the nature of religion, the rationality of religious beliefs, and the similarities and differences between scientific and religious practice. I leave it to the reader to spell out the sort of implications each different model might have. This, after all, is why it is so crucial to know and to think about the various theories of science: each one leads to a radically different understanding of the relationship between science and religion—and thus to a different view of what it is to hold religious beliefs and to engage in religious practices in a scientific age.

Notes

1. This is the view taken by those identified with the "strong program" in the sociology of science. Barnes (1977), Bloor (1976), and Holton (1978) are founders of this program, which seeks to rethink science as a social endeavor. Such approaches move us yet further from the objectivists' model of science, since they stress even more the role of social conditioning, power dynamics, and personalities in determining the outcome of scientific theorizing. The multifaceted work of Michel Foucault (e.g., 1980), with its emphasis on "power/knowledge" as the replacement for the old-style rational competition between competing scientific theories, represents just one of several important French cousins to this approach.

2. For the source of the term, see Lyotard (1984). Nancey Murphy wishes to call her view of science "postmodern" (Murphy 1990), a move that I have questioned (Clayton 1991). There is danger in using a label that expresses a *cultural* shift to designate a technical position in epistemology, for unintended connotations invariably sneak in from popular usage and sabotage one's attempts at precision. See also the essay by William Grassie in this issue of *Zygon*.

3. This result is somewhat ironic, insofar as these same theorists have insisted that each culture is to be understood not "externally" but in terms that the natives themselves would accept. Apparently this methodological principle does not apply when the "natives" are scientists and the culture is the culture of science!

4. See the classic work by this title (Bronowski 1965). Most recently, see the encyclopedic Barbour (1993).

5. They may satisfy literary theorists or those with a particular philosophical preconception about what science is, but only at the cost of providing an account that scientists themselves do not generally accept.

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