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supports expert-driven and technocratic modes of regulation. The refusal to regulate based on uncertain knowledge derives from a *positivist* stance toward science that buttresses their claim that this is the only "sound science" approach to regulation.

Proponents of precaution reject the "sound science"/"anti-science" designation. Neither formulation is inherently more scientific than the other, although they do understand regulatory science differently – as either positivistic or as inherently uncertain. Precautionary approaches also tend to treat science policy as a science/politics hybrid, whereas risk-based approaches appeal to the separation (purification) of science and politics into separate realms.

These paradigms are discursive packages rather than logical constructions, and as such can be reconstructed. Ongoing attempts at harmonizing these approaches seek to recombine various elements in a number of different ways. Some proponents of the precautionary principle fear that it will be coopted as it is thus separated from its historical entailments.

Both risk-based and precautionary approaches have logical extremes, which would make the policy untenable in practice. Positive proof of harm is very rare, and thus an absolutist risk perspective effectively undermines regulation. Critics of risk-based regulation claim that this is currently the case for persistent, bioaccumulative, and synergistic chemicals and those with complex or non-linear modes of action in human and environmental systems. Ecosystem theories and theories of endocrine disruption in particular raise such concerns (the endocrine disruption hypothesis posits that many synthetic chemicals have powerful hormonal effects at extremely low doses). Likewise, positive proof of safety is very difficult, suggesting that no new chemical or genetically modified organism could be approved under precaution. Proponents of strong precaution suggest that most synthetic chemicals have historically proven harmful to human health and the environment, so such an approach is warranted. Critics suggest that this approach is completely untenable and would ultimately stall all innovation, costing many more lives than it would save. Most proponents of precaution reject such extremes, suggesting instead that precaution shifts the calculus of regulation rather than providing a specific legal rule against innovation. Some suggest that precaution should be invoked only when there exists a prima facie case for the danger of a new substance and that priorities for precautionary regulation should be based on the degree of scientific uncertainty in combination with degrees of possible harm.

SEE ALSO: Genetic Engineering as a Social Problem; Global Politics; Knowledge; Positivism; Risk, Risk Society, Risk Behavior, and Social Problems; Science and Culture; Science and the Measurement of Risk; Science, Proof, and Law

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science, proof, and law

Stephen K. Sanderson

Science seeks to describe, explain, and predict features of the natural and social worlds. Scientists try to develop theories or explanations of phenomena by means of producing bodies of empirical evidence that play a major role in determining whether theories are accepted, modified, or rejected. In general, scientists seek theories that are logically consistent, empirically testable, well supported by available empirical evidence (and not too severely contradicted by other available evidence), parsimonious or simple, and that continue to be a source of new ideas and lines of research. Scientists also generally seek to produce theories that yield a unified understanding of the phenomena they study. For example, Wilson (1998) talks of *consilience*, and some physical scientists claim they are moving very close to a "theory of everything" (Barrow 2001).

In the early decades of the twentieth century the Vienna Circle of logical positivists insisted that science consisted only of those propositions which could be verified by facts drawn from experience. However, Popper (1959) responded by arguing that theories could never be verified because a scientist can never possess all of the possible facts bearing on a theory. Popper's solution to this problem was that the scientist had to proceed in a sort of reverse manner, by trying to *falsify* rather than verify a theory. In fact, for Popper, whether a theory was falsifiable or not was the line of demarcation between science and nonscience: science consists of falsifiable statements, and theories are retained so long as they survive these falsifying tests.

Popper recognized that a theory could rarely be falsified by a single disconfirming instance. There are degrees of falsification. In this regard, he spoke of the *corroboration* of theories. Theories are corroborated by being submitted to the most, and the most severe, falsifying efforts possible, and by withstanding them. But corroboration is not "truth." It simply means that a theory is provisionally accepted pending further testing. Better theories are those that are logically stronger, that contain greater empirical content, that have greater explanatory and predictive capabilities, and that have been more severely tested. Any newly proposed theory should also be independently testable, have new and testable consequences, and must predict the existence of phenomena thus far unobserved. And, in the end, Popper admits *verification* back in, because he contends that, just as science would stagnate if it fails to produce refutations, it would also stagnate if new theories failed to produce verifications (i.e., supportive evidence).

Popper's philosophical model is not without its problems, yet his notion that no theory can ever really be "proved true" stands, as does his notion that statements that are unfalsifiable are not to be regarded as scientific. For Popper, science was perhaps the only epistemic activity in which errors can be identified and corrected over time (Harris 1979: 27). This is what allows science to progress toward greater *verisimilitude*, or increasingly accurate approximations to the truth.

Lakatos (1970) argued that Popper's falsificationism was highly inconsistent with actual scientific practice and that it was so strict that it would make scientific advance impossible. Literally applied, Popper's falsificationism would bring science to a halt because virtually every scientific theory that has ever been proposed has *anomalies*, or facts that are inconsistent with it. Indeed, Lakatos contended that every theory is born in an "ocean of anomalies," and that scientists often retain theories for decades or even longer even though they know there are many inconsistencies.

However, Lakatos's critique applies largely only to the very early Popper, who was a *naïve falsificationist*. Later, Popper became more nuanced in adopting a far less restrictive, or *sophisticated*, falsificationism in admitting degrees of falsification (or corroboration). Lakatos regarded sophisticated falsificationism as an improvement on naïve falsificationism, but thought it was still limited in the sense of conceiving of scientific testing as simply a comparison between a single theory and a body of evidence. What is needed is a three-way comparison in which one not only compares a theory to evidence, but at the same time judges it *with respect to its main rivals*.

Moreover, Lakatos argued, it is not really theories that scientists test, but series of theories or research programs. Even if individual theories end up being decisively refuted, the research programs of which they are a part can still stand. Lakatos then went on to identify what he called theoretically progressive problem shifts. These are research programs that can explain everything their rivals explain, and at least some additional content. They can make novel predictions not made by their rivals. Lakatos's own philosophical model of science he called the methodology of scientific research programs. Every research program contains a *negative heuristic* or "hard core" of fundamental assumptions or principles, around which scientists build a "protective belt" of auxiliary hypotheses. And it is the auxiliary hypotheses, rather than the hard core, that is subjected to empirical test.

There is also a *positive heuristic*, which consists of suggestions, hints, and insights that help the scientist to modify the protective belt in order to save the irrefutable hard core, and it is this positive heuristic that "saves the scientist from being confused by the ocean of anomalies" (Lakatos 1970: 135). The anomalies are acknowledged but temporarily shoved aside in hopes that they will eventually be shown to be explainable in the basic terms of the research program. Progress in science, for Lakatos, is therefore a matter of theoretically progressive research programs. However, progressive programs seldom last forever. They often become theoretically degenerating research programs, or programs in which too many (or too severe) anomalies accumulate that can no longer be explained away. Such a research program will then give way to one or more rivals that are theoretically progressive.

Following somewhat in the Lakatosian tradition is Laudan (1977), who agrees that science is a matter of evaluating research programs, and also that one can only evaluate them comparatively. However, Laudan points out that scientists do not consider only empirical evidence when evaluating theories. They also use conceptual problems, which may play at least as large a role in scientists' acceptance or rejection of theories as empirical evidence. Moreover, scientists are rational to consider such conceptual problems if they have been a reliable guide to past knowledge. Conceptual problems are problems that arise from either the internal inconsistencies or ambiguities of a theory, or from conflicts between a theory and another theory (or nonscientific doctrine) that is thought to be well founded.

One type of conceptual problem is methodological disputes. For example, Laudan avers that much of the opposition to psychoanalysis and psychological behaviorism turned on methodological concerns, and many of the arguments over quantum mechanics also involved methodological questions. Another type of conceptual problem is worldviews, which are moral, theological, or ideological stances. Examples abound. After Darwin published *Origin of Species* in 1859, biologists fairly rapidly came to accept the reality of evolution, but there was great resistance to the mechanism he proposed to explain how evolution occurred – natural selection. This was because natural selection eliminated the concept of purpose, to which scientists were deeply attached as a worldview. It was only after about 1930 that an empirical foundation was developed that was capable of convincing scientists to abandon their entrenched concept of purpose and accept natural selection. Worldviews play a particularly crucial role in the acceptance or rejection of theories in the social sciences. For example, there has been great resistance to sociobiology, especially among sociologists, because it clashes with the entrenched Durkheimian worldview – "explain social facts only by relating them to other social facts" – and is seen as a threat to the discipline's identity. Sociobiology has also been resisted because it is widely viewed as promoting a conservative view of society, which clashes with sociologists' strong left-leaning political views.

An important difference between Laudan on the one hand and Popper and Lakatos on the other concerns the debate over realism and antirealism. For Popper and Lakatos, who were scientific realists, science is truth-seeking and is progressive in the sense of producing cumulative knowledge. Laudan, however, advocates antirealism, which means that, as Kuhn (1962) famously argued, science only solves puzzles or problems. Laudan emphasizes that in scientific change there is genuine progress (something Kuhn denied), but this change is not cumulative because new theories (or research traditions) cannot explain all of the phenomena explained by their predecessors. There are losses as well as gains when new research programs replace old ones.

In the 1970s there emerged a whole subfield of sociology, the sociology of scientific knowledge (SSK), which has grown and expanded by leaps and bounds. (For citations to the very large literature, see Laudan 1996: 183–209;

Kincaid 1996: 37–43; and several essays in Segerstrale 2000). Although its proponents vary in the degree to which they hold it, the essential premise is that the content of scientific knowledge is influenced much more by social and cultural factors than by canons of scientific rationality. This is one of the legacies of Kuhn. In addition to his argument that science is a problem-solving rather than a truth-seeking activity, Kuhn also contended that scientists operate within paradigms that are regularly overthrown by the advocates of rival paradigms, and that scientific progress only occurs *within* paradigms, not *between* them. Kuhn often spoke as if commitment to a paradigm is more a matter of group psychology or sociology rather than the rational weighing of evidence, and that paradigmatic change is much like a type of Gestalt switch. Many philosophers of science regard Kuhn's views as highly problematic because of what they see as their subjectivism and relativism.

More recently, science has come in for enormous criticism at the hands of postmodernists and other "antiscientists," who regard science as undeserving of its epistemically privileged position and as just one way of knowing among others. This is one of the legacies of the "epistemological anarchism" of Feyerabend (1975), whose views were considerably more radical than Kuhn's. For Feyerabend, all modes of knowledge are essentially on the same plane, whether science or witchcraft, and thus his basic methodological rule was that there should be no methodological rules – "anything goes." The postmodern attack on science has emphasized its alleged "Eurocentrism" and claimed that commitment to science as a superior epistemology is rooted in western cultural values rather than objective criteria (since, for postmodernism, there can be no such criteria). (For excellent summaries and commentaries, see Segerstrale 2000.) Those philosophers and sociologists who see science as a mere social construction seem to be engaged in a completely self-refuting argument, since they do not "think *their own work* is only a social construction with no claim on evidence and truth as traditionally understood" (Kincaid 1996: 41).

Sociology is a very immature science, and most sociologists have an impoverished understanding of real science. For example, the majority of sociologists study only one society (usually their own) and no general theories can be built on the basis of one case. (It would be like trying to build biological science by studying only penguins.) Many sociologists resolve the acrimonious debates among rival theoretical camps by settling for an eclectic position, but eclecticism as it is understood by sociologists is a strategy rarely if ever favored by natural scientists. Eclecticism violates the principle of parsimonious and highly unified explanation – one of the most fundamental of all scientific goals – and it makes the comparative evaluation of theories impossible (Sanderson 1987). Many sociologists who do highly quantitative survey research build unwieldy models that contain a large number of variables, but real science does not work that way. What results is a kind of "multivariate chaos" that is the antithesis of parsimonious explanation.

Sociology today lacks a highly cumulative body of knowledge, and there is very little agreement on key epistemological, methodological, and theoretical questions. Conceptual problems are particularly acute in sociology, especially in the form of political ideology and its role in settling theoretical debates. From the standpoint of the enormous successes of the natural sciences, sociology is an extremely immature discipline in terrible disarray. At the most general theoretical level, the vast majority of sociologists continue to adhere to the standard social science model, which assumes that human behavior is overwhelmingly determined by the social environment. However, this is a massively degenerating research program, for the accumulated anomalies are extreme. Sociologists cling to it for conceptual, not empirical, reasons.

Although the overall picture in sociology and social science more generally is not an impressive one, the social sciences do have some genuine research programs that may be regarded as at least mildly to moderately progressive. In anthropology, there is the cultural materialism of Harris (1979), which is coherent and unified and has made some impressive accomplishments. In psychology, anthropology, and to some extent sociology, there is a very coherent research program that now goes under the name of evolutionary psychology (Barkow et al. 1992; Crawford & Krebs 1998). Thus far it has proven to be a highly progressive research program.

A closely related research program in anthropology is evolutionary ecology (Smith & Winterhalder 1992). And in sociology a good example of a coherent research program is rational choice theory. This program has been attached to the study of early modern and modern states (Kiser et al. 1995), to the study of human sexuality (Posner 1992), and to numerous other substantive areas. There are also dependency and world-system approaches to economic development, which have the merit of being research programs that have been subjected to extensive empirical testing, even though, unfortunately, the anomalies have become severe and in many ways these approaches are now degenerating programs (Sanderson 2005a). There is also the state-centered approach to revolutions (Wickham-Crowley 1992; Goldstone 1991; cf. Sanderson 2005b), which is something like a research program and seems to be a highly progressive one.

So the situation is by no means totally bleak. Natural scientists do not really need to study the history and philosophy of science, and few do. Indeed, scientists are often highly antagonistic toward philosophy of science. The reason natural scientists do not need philosophy of science is that they have a keen sense of what they are doing, and they generally do it extremely well. Social scientists, by contrast, very badly need to study the history and philosophy of science because they need to gain a much better understanding of how real science actually works and try to emulate it.

One major barrier to success in social science is the complexity and relative unpredictability of the phenomena being studied. The other major barrier is conceptual, and mainly ideological. Ideology is an enormous barrier to scientific objectivity, and indeed to the very practice of science at all. Sociologists and other social scientists can do nothing to alter the nature of the phenomena they study, but they are entirely free to embark along the path of objective social science if they choose to recommit themselves to doing so.

It should be clear that proof is not really possible in science, if by proof we mean "establishment with certainty." It has long been noted by philosophers of science of many stripes that theories will always be "underdetermined" by empirical evidence. (This is the famous Duhem-Quine underdetermination thesis, which has often been used by postmodernists and other relativists to attack science. However, such conclusions are complete non sequiturs.) There is only disproof or, lacking that, provisional acceptance. Proof must be restricted to the domains of logic and mathematics. As for laws, these certainly exist in the physical sciences and to some extent in the biological sciences, but they rarely exist in the social sciences. Social scientists still have enough work to do to bring themselves up to minimal scientific standards. The development of widely agreed upon laws of social behavior, organization, and change are far off into the future.

SEE ALSO: Fact, Theory, and Hypothesis: Including the History of the Scientific Fact; Falsification; Induction and Observation in Science; Kuhn, Thomas and Scientific Paradigms; Paradigms; Science and the Precautionary Principle; Science, Social Construction of; Scientific Knowledge, Sociology of

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science and public participation: the democratization of science

John Forrester

Thomas Jefferson, quoted in Fischer's *Citizens, Experts, and the Environment* (2001), said that wherever the people are well informed, they can be trusted with their own government. But, nowadays, who can claim to be well informed enough about science to govern it except the scientists themselves? In 1959, Sir Charles Snow put forward the thesis in the Rede Lecture that there was what amounted to an opposition between literary intellectualism at one end, and proficiency in the physical sciences at the other. Snow dated his realization of this distinction to the 1930s. What we can say for certain is that there was a coming into common understanding that a reasonably well-educated or cultured person could not, now, be expected to be normally able to comprehend both the sciences and the arts. This state of affairs is not by any means all the scientific community's fault, although science is guilty of creating, along with other forms of knowledge and understanding, elites. Elitism fosters disciplinization and subdisciplinization, and has given rise to mistrust and lack of understanding between the members of different disciplines and of science and scientists in general. The term "lay" was commonly used until the 1990s to describe those untutored in science, thus emphasizing the idea of a scientific priesthood or elite. For various reasons not dealt with here, this state of affairs is seen as being iniquitous, and so public participation in science, also known as public engagement in science, is seen as a means whereby that balance can be redressed.

This broad generalization of why science "needs" to be democratized hides several distinct rationales as to why the public should engage with science or vice versa. Before dealing with these rationales, there is one distinction that needs to be introduced: who or what "the public" or "publics" are engaging in or with. In many "western-style" democracies, members of the public are engaging and being engaged in the governance of science, but not in knowledge creation itself. In contrast, in some continental European countries and in a few developing countries, citizen participation in science is seeing citizens more as co-creators of new knowledge alongside traditional experts, new knowledge that is both "reliable" (after Gibbons 1999 – i.e., knowledge that is scientifically correct) and also "socially robust" (i.e., that overcomes the elitism of traditionally generated scientific knowledge). These two major dimensions to public engagement may be distinguished as public engagement *with* science on the one hand, and public engagement *in* science on the other. Stirling (2005) characterizes the first more exactly as "participation in the social appraisal of science and technology," while the other is also about knowledge production, as is illustrated by the title of the book *The New Production of Knowledge* published in 1994 by an international team of scholars including