Philosophy and Rhetoric of Science
Contents

Articles

  Rhetoric of science ........................................ 1
  Philosophy of science ....................................... 9
  Logical positivism .......................................... 22
  Karl Popper .................................................. 32
  *The Logic of Scientific Discovery* ..................... 51
  Falsifiability ................................................ 52
  Thomas Kuhn ................................................ 61
  *The Structure of Scientific Revolutions* ............. 65
  Paradigm .................................................... 74
  Imre Lakatos ................................................ 80
  Paul Feyerabend .......................................... 89

References

  Article Sources and Contributors ....................... 99
  Image Sources, Licenses and Contributors ............ 101

Article Licenses

  License ..................................................... 102
Rhetoric of science

Rhetoric of science is a body of scholarly literature exploring the notion that the practice of science is a rhetorical activity. It emerged from a number of disciplines during the late twentieth century, including the disciplines of sociology, history, and philosophy of science, but it is practiced most fully by rhetoricians in departments of English, speech, and communication.

Rhetoric is best known as a discipline that studies the means and ends of persuasion. Science, meanwhile, is typically seen as the discovery and recording of knowledge about the natural world. A key contention of rhetoric of science is that the practice of science is, to varying degrees, persuasive. The study of science from this viewpoint variously examines modes of inquiry, logic, argumentation, the ethos of scientific practitioners, the structures of scientific publications, and the character of scientific discourse and debates. For instance, scientists must convince their community of scientists that their research is based on sound scientific method. From a rhetorical point of view, scientific method involves problem-solution topoi (the materials of discourse) that demonstrate observational and experimental competence (arrangement or order of discourse or method), and as a means of persuasion, offer explanatory and predictive power (Prelli 185-193). Experimental competence is itself a persuasive topos (Prelli 186). Rhetoric of science is a practice of suasion that is an outgrowth of some of the canons of rhetoric.

History

Since 1970, rhetoric of science, as a field involving rhetoricians, flourished. This flourishing of scholarly activity contributed to a shift in the image of science that was taking place (Harris "Intro," Landmark xv). A conservative approach to rhetoric of science involves treating texts as communications designed to persuade members of scientific communities. This approach concerns scientific claims that are already considered true as a result of the scientific process rather than the rhetorical process. A more radical approach, on the other hand, would treat these same texts as if the science held within them is also an object of rhetorical scrutiny (Gross "Rhetoric of Science," Encyclopedia 622-623). Among those in the conservative camp, who view science texts as vehicles of communication, are Charles Bazerman, John Angus Campbell, Greg Myers, Jean Dietz Moss, Lawrence Prelli, Carolyn Miller and Jeanne Fahnestock. Bazerman's close readings of works by Newton and Compton as well as his analysis of the reading habits of physicists and others led to a greater understanding of the successes and failures of communication (Gross "Rhetoric of Science," Encyclopedia 623-624). For a depiction of the views of the more radical camp, see the section titled "Critique of Rhetoric of Science."

The history of the rhetoric of science effectively begins with Thomas Kuhn's seminal work, The Structure of Scientific Revolutions (1962). He examines first normal science, that is, a practice which he saw as routine, patterned and accessible with a specific method of problem-solving. Building on past knowledge, normal science advances by accretions in a knowledge base (Harris "Intro," Landmark xiii). Kuhn then contrasts normal science with revolutionary science (ground-breaking science marked by a paradigm-shift in thought). When Kuhn began to teach Harvard undergraduates historical texts such as Aristotle's writings on motion, he looked to case studies, and sought first to understand Aristotle in his own time, and then to locate his problems and solutions within a wider context of contemporary thought and actions (Nickles 144). That is to say, Kuhn sought first to understand the traditions and established practices of science (Nickles 162). In this instance, Michael Polanyi's influence on Kuhn becomes apparent; that is, his acknowledgement of the importance of inherited practices and rejection of absolute objectivity. Observing the changes in scientific thought and practices, Kuhn concluded that revolutionary changes happen through the defining notion of rhetoric: persuasion (Harris "Intro," Landmark xiv). The critical work of Herbert W. Simons - "Are Scientists Rhetors in Disguise?" in Rhetoric in Transition (1980) - and subsequent works show that Kuhn's Structure is fully rhetorical.
The work of Thomas Kuhn was extended by Richard Rorty (1979, 1989), and this work was to prove fruitful in defining the means and ends of rhetoric in scientific discourse (Jasinski "Intro" xvi). Rorty, who coined the phrase "rhetorical turn," was also interested in assessing periods of scientific stability and instability.

Another component of the shift in science that took place in the past centres on the claim that there is no single scientific method, but rather a plurality of methods, approaches or styles (Harris "Intro," Landmark xvi). Paul Feyerabend in Against Method (1975) contends that science has found no "method that turns ideologically contaminated ideas into true and useful theories," in other words; no special method exists that can guarantee the success of science (302).

As evidenced in the early theory papers after Kuhn's seminal work, the idea that rhetoric is crucial to science came to the fore. Quarterly journals in speech and rhetoric saw a flourishing of discussion on topics such as inquiry, logic, argument fields, ethos of scientific practitioners, argumentation, scientific text, and the character of scientific discourse and debates. Philip Wander (1976) observed, for instance, the phenomenal penetration of science (public science) in modern life. He labelled the obligation of rhetoricians to investigate science's discourse as "The Rhetoric of Science" (Harris "Knowing" 164).

As rhetoric of science began to flourish, discussion arose in a number of areas, including:

- **Epistemic rhetoric and the discourses on the nature of semantics, knowledge, and truth:** One example is the Robert L. Scott's work on viewing rhetoric as epistemic (1967). By the 1990s, epistemic rhetoric was a point of contention in the writing of Dilip Gaonkar (see "Critique" below).

- **The early 1970s Speech Communication Conference ("Wingspread conference")** gave recognition to the fact that rhetoric, in its globalization (multidisciplinary nature), has become a universal hermeneutic (Gross Rhetorical 2-5). Much scholastic output evolved around the theory of interpretation (hermeneutics), the knowledge-making and truth-seeking (epistemic) potential of rhetoric of science.

- **Argument Fields** (part of the Speech Communication Association and American forensic Association program): In this domain the work of Toulmin on argument appeals is exemplary. In addition, Michael Mulkay, Barry Barnes and David Bloor, as pioneers of the "Sociology of Scientific Knowledge" (SSK) movement, fostered a growing sociobiology debate. Others as Greg Myers expressed the benefits of a collaboration between rhetoricians and sociologists. Contributors to discussion pertaining to audience - the way arguments change as they move from the scientific community to the public - include John Lyne and Henry Howe (Harris "Intro," Landmark xxi-xxxii).

- **Scientific Giants:** The important works that investigate the suasive powers of exemplars in science include those of Alan Gross (Newton, Descartes, argument fields in optics), John Angus Campbell (Darwin), and Michael Halloran (Watson and Crick). John Angus Campbell's work on Charles Darwin's *The Origin of Species*, for instance his work titled, "Charles Darwin: Rhetorician of Science" (1987), shows that Darwin was a rhetorician through and through. Campbell provided an early 'case' in support of rhetoric of science (Harris "Intro," Landmark xxx).

Other major themes in rhetoric of science include the investigation of the accomplishments and suasive abilities of individuals (ethos) who have left a mark in their respective sciences as well as an age old concern of rhetoric of science - public science policy. Science policy involves deliberative issues, and the first rhetorical study of science policy was made in 1953 by Richard M. Weaver. Among others, Helen Longino's work on public policy implications of low-level radiation continues this tradition (Gross "Rhetoric of Science," Encyclopedia 622).

The reconstitution of rhetorical theory around the lines of invention (inventio), argumentation and stylistic adaptation is going on today (Simons 6). The key question today is whether training in rhetoric can in fact help scholars and investigators make intelligent choices between rival theories, methods or data collection, and incommensurate values (Simons 14).
Developments and trends

Epistemic rhetoric

Seeing science from the point of texts exhibiting epistemology based on prediction and control offers new comprehensive ways to see the function of rhetoric of science (Gross "The Origin" 91-92). Epistemic rhetoric of science, in a broader context, confronts issues pertaining to truth, relativism, and knowledge.

Rhetoric of science, as a branch of inquiry, does not look at scientific (natural science) texts as a transparent means of conveying knowledge, but rather it looks at these texts as exhibiting persuasive structures. Although the natural sciences and humanities differ in a fundamental way, the enterprise of science can be viewed hermeneutically as a stream of texts which exhibit an epistemology based on understanding (Gross "On the Shoulders 21). Its task then is the rhetorical reconstruction of the means by which scientists convince themselves and others that their knowledge claims and assertions are an integral part of privileged activity of the community of thinkers with which they are allied (Gross "The Origin" 91).

In an article titled "On Viewing Rhetoric as Epistemic" (1967), Robert Scott offers "that truth can arise only from cooperative critical inquiry" (Harris "Knowing" 164). Scott's probe of the issues of belief, knowledge and argumentation substantiates that rhetoric is epistemic. This train of thought goes back to Gorgias who noted that truth is a product of discourse, not a substance added to it (Harris "Knowing" 164).

Scientific discourse is built on accountability of empirical fact which is presented to a scientific community. Each form of communication is a type of genre that fosters human interaction and relations. An example is the emerging form of the experimental report (Bazerman "Reporting" 171-176). The suite of genres to which the rhetoric of science comes to bear on health care and scientific communities is legion.

Aristotle could never accept the unavailability of certain knowledge, although most now believe the contrary (Gross "On Shoulders" 20). That is to say, Aristotle would have rejected the central concern of rhetoric of science: knowledge (Gross "Rhetoric of Science," Encyclopedia 622). Knowing itself generates the explanation of knowing, and this is the domain of the theory of knowledge. The knowledge of knowledge compels an attitude of vigilance against the temptation of certainty (Maturana 239-245).

The claim of the epistemic problematic of rhetoric of science concerns:

- truth - property of statements with respect to other statements
- knowledge - configuration of mutually supporting true statements
- arguments - are situational (first principle of rhetoric)

(Harris "Knowing" 180-181).

Argument fields

By the 1980s, Stephen Toulmin's work on argument fields published in his book titled The Uses of Argument (1958) came to prominence through rhetorical societies such as the Speech Communication Association which adopted a sociological view of science. Toulmin's main contribution is his notion of argument fields that saw a reinvention of the rhetorical concept topoi (topics) (Harris "Intro" Landmark xxi).

Toulmin discusses at length the pattern of an argument – data and warrants to support a claim – and how they tend to vary across argument fields (Toulmin 1417-1422). He delineated two concepts of argumentation, one which relied on universal (field-invariant) appeals and strategies, and one which was field dependent, particular to disciplines, movements, and the like. For Toulmin, audience is important because one speaks to a particular audience at a particular point in time, and thus an argument must be relevant to that audience. In this instance, Toulmin echoes Feyerabend, who in his preoccupation with suasive processes, makes clear the adaptive nature of persuasion (Harris "Intro" Landmark xxv).
Toulmin's ideas pertaining to argument were a radical import to argumentation theory because, in part, he contributes a model, and because he contributes greatly to rhetoric and its subfield, rhetoric of science, by providing a model of analysis (data, warrants) to show that what is argued on a subject is in effect a structured arrangement of values that are purposive and lead to a certain line of thought.

Toulmin showed in *Human Understanding* that the arguments that would support claims as different as the Copernican revolution and the Ptolemaic revolution would not require mediation. On the strength of argument, men of the sixteenth and seventeenth centuries converted to Copernican astronomy (Gross "The Rhetoric" 214).

**Incommensurability**

The rhetorical challenge today is to find discourse that crosses disciplines without sacrificing the specifics of each discipline. The aim is to render description of these disciplines intact — that is to say, the goal of finding language that would make various scientific fields "commensurable" (Baake 29). In contrast, incommensurability is the term used to describe a situation where two scientific programs are fundamentally at odds. Two important voices who applied incommensurability to historical and philosophical notions of science in the 1960s are Thomas Kuhn and Paul Feyerabend. Various strands grew out of this idea that bear on issues of communication and invention. These strands are explicated in Randy Allen Harris's four-part taxonomy that in turn foregrounds his viewpoint that "incommensurability is best understood not as a relation between systems, but as a matter of rhetorical invention and hermeneutics" (Harris "Incommensurability" 1).

Incommensurability of theory at times of radical theory change is at the heart of Thomas Samuel Kuhn's theory of paradigms (Bazerman 1). Kuhn's *Structure of Scientific Revolutions* offers a vision of scientific change that involves persuasion, and thus he brought rhetoric to the heart of scientific studies (Harris "Intro," *Landmark* xiii).

Kuhn's *Structure* provides important accounts related to the concept representation, and the key conceptual changes that occur during a scientific revolution. Kuhn sought to determine ways of representing concepts and taxonomies by frames (Barker 224-230). Kuhn's work attempts to show that incommensurable paradigms can be rationally compared by revealing the compatibility of attribute lists of say a species outlined in a pre-Darwinian and a post-Darwinian milieu accounted for in two incommensurable taxonomies, and that this compatibility is the platform for rational comparison between rival taxonomies (Barker 230-231). With a view to comparing normal science to revolutionary science, Kuhn illustrates his theory of paradigms and theory of concepts within the history of electricity, chemistry and other disciplines. He gives attention to the revolutionary changes that came about as a result of the work of Copernicus, Newton, Einstein, Roentgen, and Lavoisier.

Kuhn's work was influential for rhetoricians, sociologists, and historians (and, in a more muted way, philosophers) for the development of a rhetorical perspective. His view on perception, concept acquisition and language suggest, according to Paul Hoyningen-Huene's analysis of Kuhn's philosophy, a cognitive perspective (Nickles 183).

**Ethos**

Scientists are not just persuaded by logos or argument. Innovative initiatives in science test scientific authority by invoking the authority of past results (initial section of a scientific paper) and the authority of procedure, which establish the scientist's credibility as an investigator (Gross *Starring* 26-27).

Examinations of the ethos of scientists (individually and collectively) spawned significant contributions in the field of rhetoric of science. Michael Halloran notes in "The Birth of Molecular Biology" (*Rhetoric Review* 3, 1984) — an essay that is a rhetorical analysis of James D. Watson and Francis H. Crick's "A Structure for Deoxyribose Nucleic Acid" — that a large part of what constitutes a scientific paradigm is the ethos of its practitioners. This ethos is about an attitude and a way of attacking problems and propagating claims (Harris "Intro," *Landmark* xxxi).

In "The Rhetorical Construction of Scientific Ethos," Lawrence Prelli provides a systematic analysis of ethos as a tool of scientific legitimization. Prelli's work examines the exchange of information in the court of public opinion. His work provides insight into the ways in which scientific argumentation is legitimized, and thus insight into public
Rhetoric of science

science policy. One of the domains of rhetoric is civic life. Rhetorical criticism of science offers much in the investigation of scientific matters that impinge directly upon public opinion and policy-making decisions (Harris "Intro," Landmark xxxiii).

**Rhetoric and language-games**

Rhetoric can also be defined as the strategic use of language: each scientist tries to make those statements that - given the statements made by their colleagues, and the ones the former expects they will do in the future (e.g., accepting or rejecting the claims made by the former) - maximise the chances of the former's attaining the goals he or she has. So, game theory can be applied to study the choice of the claims one scientist makes. Zamora Bonilla (2006) argues that, when rhetoric is understood this way, it can be discussed whether the way scientists interact - e.g., through certain scientific institutions like peer review - leads them to make their claims in an efficient or an inefficient way, i.e., whether the 'rhetorical games' are more analogous to 'invisible hand' processes, or to 'prisoner's dilemma' games. If the former is the case, then we can assert that scientific 'conversation' is organised in such a way that the strategic use of language by scientists leads them to reach cognitive progress, and if the opposite is the case, then this would be an argument to reform scientific institutions.

**Rhetorical figures in science**

Corresponding to distinct lines of reasoning, figures of speech are evident in scientific arguments. The same cognitive and verbal skills that are of service to one line of inquiry – political, economic or popular – are of service to science (Fahnestock 43). This implies that there is less of a division between science and the humanities than initially anticipated. Argumentatively useful figures of speech are found everywhere in scientific writing.

Theodosius Dobzhansky in *Genetics and the Origin of Species* offers a means of reconciliation between Mendelian mutation and Darwinian natural selection. By remaining sensitive to the interests of naturalists and geneticists, Dobzhansky – through a subtle strategy of polysemy – allowed a peaceful solution to a battle between two scientific territories. His expressed aim was to review the genetic information bearing on the problem of organic diversity (Ceccarelli 41, 53). The building blocks of Dobzhansky's interdisciplinary influence that saw much development in two scientific camps were the result of the compositional choices he made. He uses, for instance, prolepsis to make arguments that introduced his research findings, and he provided a metaphoric map as a means to guide his audience (Ceccarelli 57-58). One illustration of metaphor is his use of the term "adaptive landscapes." Seen metaphorically, this term is a way of representing how theorists in two different fields can unite (Ceccarelli 57).

Another figure that is important as an aid to understanding and knowledge is antimetabole (refutation by reversal). Antithesis also works toward a similar end.

An example of antimetabole:

- Antimetabole often appears in writing or visuals where the line of inquiry and experiment has been characterized by mirror-image objects, or of complementarity, reversible or equilibrium processes. Louis Pasteur's revelation that many organic compounds come in left-and right-handed versions or isomers as articulated at a 1883 lecture illustrates the use of this figure. He argues in lecture that "life is the germ and the germ is life" because all life contains unsymmetrical/asymmetrical processes (Fahnestock 137-140).
Critique of rhetoric of science

Globalization of rhetoric

Renewed interest today in rhetoric of science is its positioning as a hermeneutic meta-discourse rather than a substantive discourse practice (Gaonkar 25). Exegesis and hermeneutics are the tools around which the idea of scientific production has been forged.

Criticism of rhetoric of science is mainly limited to discussions around the concept of hermeneutics, which can be seen as follows:

- Rhetorical hermeneutics is about a way of reading texts as rhetoric. Rhetoric is both a discipline and a perspective from which disciplines can be viewed. As a discipline, it has a hermeneutic task and generates knowledge; as a perspective, it has the task of generating new points of view (Gross Rhetorical 111). Whether rhetorical theory can function as a general hermeneutic, a key to all texts, including scientific texts, is still today a point of interest to rhetoricians. Although natural sciences and humanities differ in fundamental ways, science as enterprise can be viewed hermeneutically as a suite of texts exhibiting a study of knowledge (epistemology) based on understanding (Gross "On Shoulders" 21).

A recent critique about the rhetoric of science literature asks not if science is understood properly, but rather if rhetoric is understood properly. This dissension centres around the reading of scientific texts rhetorically; it is a quarrel about how rhetorical theory is seen as a global hermeneutic (Gross "Intro" Rhetorical 1-13).

Dilip Gaonkar in "The Idea of Rhetoric in the Rhetoric of Science" looks at how critics argue about rhetoric, and he unfolds the global ambitions of rhetorical theory as a general hermeneutic (a master key to all texts), with the rhetoric of science as a perfect site of analysis - a hard and fast case.

In his analysis of this 'case', Gaonkar looks at rhetoric's essential character first in traditional sense (Aristotilean and Ciceronian). Then he looked at the practice of rhetoric and the model of persuasive speech from the point of agency (productive orientation) or who controls the speech (means of communication). The rhetorical tradition is one of practice, while the theory evinces practice and teaching (Gross "Intro" Rhetorical 6-11). Gaonkar asserts that rhetoric seen as a tradition (Aristotilean and Ciceronia), and from the point of view of interpretation (not production or agency), rhetorical theory is "thin." He argues that rhetoric appears as a thinly veiled language of criticism in such a way that it is applicable to almost any discourse (Gaonkar 33, 69).

Gaonkar believes that this type of globalization of rhetoric undermines rhetoric's self-representation as a situated practical art, and in so doing, it runs counter to a humanist tradition. It runs counter to the interpretative function of a critical metadiscourse. If there is no more substance, no anchor, no reference to which rhetoric is attached, rhetoric itself is the substance, or the supplement, and thus becomes substantial, giving rise to the question how well rhetoric functions as interpretative discourse (Gaonkar 77).

Dilip Gaonkar's provocations have successfully opened the way to a broad reaching discussion that led to the defense of rhetoric analyses of scientific discourse. Responses to Gaonkar's provocations are many, of which two examples follow.

- When Gaonkar asks if a theory grounded in practice can be translated into a theory of interpretation, Michael Leff in "The Idea of Rhetoric as Interpretative Practice: A Humanist's Response to Gaonkar" see his views as too extreme, treating as opposites two positions that are in dialectic tension (rhetoric as production and rhetoric as interpretation), and separating interpretation from practice in order to establish a causal, rather than accidental, relationship between rhetoric and the globalalization of rhetoric (Gross "Intro" Rhetorical 11).

- John Angus Campbell in "Strategic Readings: Rhetoric, Intention, and Interpretation" also found in Rhetorical Hermeneutics is a verification of Leff's analysis (113). He argues, however, against Gaonkar's notion of invention and the mediation between producer or writer and the audience of a text(114). The differences between Campbell and Gaonkar is one of theory, and not whether agency figures in criticism (115).
Radical Rhetoric of Science

The radical approach to rhetoric of science looks at the rhetorical process itself, and as such, it involves treating scientific texts as an object of rhetorical scrutiny. This approach considers how the methods of natural sciences came into being, and the particular role interaction among scientists has to play. Radical rhetoric of science of a feminist variety include those proponents see the progress of the natural sciences as having been purchased at a high cost, a cost that limits the scope and vision of science. The other branch of radical rhetoric of science is of the epistemological variety (Gross "Rhetoric of Science," Encyclopedia 623-625). Radical rhetoric of science does not require a radical departure of rhetoric of science in relation to persuasion. In the past fifteen years, books written by Bruno Latour, Steve Woolgar and Alan G. Gross ("The Rhetoric of Science"), contain as their subject the generalization of the radical critique.

The question as to the adequacy of rhetoric in its encounter with scientific texts (natural sciences) is problematic on two fronts. The first concerns traditional rhetoric and its capacity as a tool to analyze scientific texts. Secondly, the answer to the question relies on an attack of the epistomological presuppositions of a classical rhetoric of science. For this reason, the radical critique is a call for the renewal of rhetorical theory (Gross "Rhetoric of Science," Encyclopedia 626-627).

Prospects

For rhetoric of science to grow, "it must seek consistent and revealing accounts of why science is rhetorical" (Harris "Knowing" 181). A path is thus open for future research.

Works cited

• Bazerman, Charles and René Agustin De los Santos. "Measuring Incommensurability: Are toxicology and ecotoxicology blind to what the other sees?" 9 January 2006. [1].


Other readings


## References

[3] [http://www.umed.es/dpto_log/jpzb/abstracts.html#Rhetoric_induction_and_the_free_speech_dilemma](http://www.umed.es/dpto_log/jpzb/abstracts.html#Rhetoric_induction_and_the_free_speech_dilemma)
[4] [http://www.pitt.edu/~gordonm/Pubdeb/WanderJaehne.pdf](http://www.pitt.edu/~gordonm/Pubdeb/WanderJaehne.pdf)

### Philosophy of science

The **philosophy of science** is concerned with the assumptions, foundations, methods, and implications of science. It is also concerned with the use and merit of science and sometimes overlaps metaphysics and epistemology by exploring whether scientific results are actually a study of truth. In addition to these central problems of science as a whole, many philosophers of science also consider problems that apply to particular sciences (e.g. philosophy of biology or philosophy of physics). Some philosophers of science also use contemporary results in science to reach conclusions about philosophy.

Philosophy of science has historically been met with mixed response from the scientific community. Though scientists often contribute to the field, many prominent scientists have felt that the practical effect on their work is limited; a popular quote attributed to physicist Richard Feynman goes, "Philosophy of science is about as useful to scientists as ornithology is to birds." In response, some philosophers (e.g. Craig Callender[1]) have suggested that ornithological knowledge would be of great benefit to birds, were it possible for them to possess it.

### Demarcation

The demarcation problem refers to the distinction between science and nonscience (including pseudoscience); Karl Popper called this the central question in the philosophy of science.[2] However, no unified account of the problem has won acceptance among philosophers, and some regard the problem as unsolvable or uninteresting.[3] Early attempts by the logical positivists grounded science in observation while non-science was non-observational and hence meaningless.[4] Popper argued that the central property of science is falsifiability (i.e. all scientific claims can be proven false, at least in principle, and if no such proof can be found despite sufficient effort then the claim is likely true).[5]
Scientific realism and instrumentalism

Two central questions about science are (1) what are the aims of science and (2) how should one interpret the results of science? Scientific realists claim that science aims at truth and that one ought to regard scientific theories as true, approximately true, or likely true. Conversely, a scientific antirealist or instrumentalist argues that science does not aim (or at least does not succeed) at truth and that we should not regard scientific theories as true. Some antirealists claim that scientific theories aim at being instrumentally useful and should only be regarded as useful, but not true, descriptions of the world. Realists often point to the success of recent scientific theories as evidence for the truth (or near truth) of our current theories. Antirealists point to either the history of science, epistemic morals, or widely termed postmodern criticisms of objectivity as evidence against scientific realisms. Some antirealists attempt to explain the success of scientific theories without reference to truth.

Scientific explanation

In addition to providing predictions about future events, we often take scientific theories to offer explanations for those that occur regularly or have already occurred. Philosophers have investigated the criteria by which a scientific theory can be said to have successfully explained a phenomenon, as well as what gives a scientific theory explanatory power. One early and influential theory of scientific explanation was put forward by Carl G. Hempel and Paul Oppenheim in 1948. Their Deductive-Nomological (D-N) model of explanation says that a scientific explanation succeeds by subsuming a phenomenon under a general law. An explanation, then, is a valid deductive argument. For empiricists like Hempel and other logical positivists, this provided a way of understanding explanation without appeal to causation. Although ignored for a decade, this view was subjected to substantial criticism, resulting in several widely believed counter examples to the theory. In addition to their D-N model, Hempel and Oppenheim offered other statistical models of explanation which would account for statistical sciences. These theories have received criticism as well. Salmon attempted to provide an alternative account for some of the problems with Hempel and Oppenheim's model by developing his statistical relevance model. In addition to Salmon's model, others have suggested that explanation is primarily motivated by unifying disparate phenomena or primarily motivated by providing the causal or mechanical histories leading up to the phenomenon (or phenomena of that type).

Analysis and reductionism

Analysis is the activity of breaking an observation or theory down into simpler concepts in order to understand it. Analysis is as essential to science as it is to all rational activities. For example, the task of describing mathematically the motion of a projectile is made easier by separating out the force of gravity, angle of projection and initial velocity. After such analysis it is possible to formulate a suitable theory of motion.

Reductionism can refer to one of several philosophical positions related to this approach. One type of reductionism is the belief that all fields of study are ultimately amenable to scientific explanation. Perhaps a historical event might be explained in sociological and psychological terms, which in turn might be described in terms of human physiology, which in turn might be described in terms of chemistry and physics.

Daniel Dennett invented the term greedy reductionism to describe the assumption that such reductionism was possible. He claims that it is just 'bad science', seeking to find explanations which are appealing or eloquent, rather than those that are of use in predicting natural phenomena. He also says that:

There is no such thing as philosophy-free science; there is only science whose philosophical baggage is taken on board without examination.—Daniel Dennett, Darwin's Dangerous Idea, 1995.
Grounds of validity of scientific reasoning

Empirical verification

Science relies on evidence to validate its theories and models, and the predictions implied by those theories and models should be in agreement with observation. Ultimately, observations reduce to those made by the unaided human senses: sight, hearing, etc. To be accepted by most scientists, several impartial, competent observers should agree on what is observed. Observations should be repeatable, e.g., experiments that generate relevant observations can be (and, if important, usually will be) done again. Furthermore, predictions should be specific; one should be able to describe a possible observation that would falsify the theory or a model that implies the prediction.

Nevertheless, while the basic concept of empirical verification is simple, in practice, there are difficulties as described in the following sections.

Induction

How is it that scientists can state, for example, that Newton's Third Law is universally true? After all, it is not possible to have tested every incidence of an action, and found a reaction. There have, of course, been many, many tests, and in each one a corresponding reaction has been found. But can we be sure that future tests will continue to support this conclusion?

One solution to this problem is to rely on the notion of induction. Inductive reasoning maintains that if a situation holds in all observed cases, then the situation holds in all cases. So, after completing a series of experiments that support the Third Law, and in the absence of any evidence to the contrary, one is justified in maintaining that the Law holds in all cases.

Although induction commonly works (e.g. almost no technology would be possible if induction were not regularly correct), explaining why this is so has been somewhat problematic. One cannot use deduction, the usual process of moving logically from premise to conclusion, because there is no syllogism that allows this. Indeed, induction is sometimes mistaken; 17th century biologists observed many white swans and none of other colours, but not all swans are white. Similarly, it is at least conceivable that an observation will be made tomorrow that shows an occasion in which an action is not accompanied by a reaction; the same is true of any scientific statement.

One answer has been to conceive of a different form of rational argument, one that does not rely on deduction. Deduction allows one to formulate a specific truth from a general truth: all crows are black; this is a crow; therefore this is black. Induction somehow allows one to formulate a general truth from some series of specific observations: this is a crow and it is black; that is a crow and it is black; no crow has been seen that is not black; therefore all crows are black.

The problem of induction is one of considerable debate and importance in the philosophy of science: is induction indeed justified, and if so, how?

Duhem-Quine thesis

According to the Duhem-Quine thesis, after Pierre Duhem and W.V. Quine, it is impossible to test a theory in isolation. One must always add auxiliary hypotheses in order to make testable predictions. For example, to test Newton's Law of Gravitation in our solar system, one needs information about the masses and positions of the Sun and all the planets. Famously, the failure to predict the orbit of Uranus in the 19th century led, not to the rejection of Newton's Law, but rather to the rejection of the hypothesis that there are only seven planets in our solar system. The investigations that followed led to the discovery of an eighth planet, Neptune. If a test fails, something is wrong. But there is a problem in figuring out what that something is: a missing planet, badly calibrated test equipment, an unsuspected curvature of space, etc.
One consequence of the Duhem-Quine thesis is that any theory can be made compatible with any empirical observation by the addition of a sufficient number of suitable ad hoc hypotheses. This is why science uses Occam's Razor; hypotheses without sufficient justification are eliminated.

This thesis was accepted by Karl Popper, leading him to reject naïve falsification in favor of ‘survival of the fittest’, or most falsifiable, of scientific theories. In Popper's view, any hypothesis that does not make testable predictions is simply not science. Such a hypothesis may be useful or valuable, but it cannot be said to be science. Confirmation holism, developed by W.V. Quine, states that empirical data are not sufficient to make a judgment between theories. In this view, a theory can always be made to fit with the available empirical data. However, the fact that empirical evidence does not serve to determine between alternative theories does not necessarily imply that all theories are of equal value, as scientists often use guiding principles such as Occam's Razor.

One result of this view is that specialists in the philosophy of science stress the requirement that observations made for the purposes of science be restricted to *intersubjective* objects. That is, science is restricted to those areas where there is general agreement on the nature of the observations involved. It is comparatively easy to agree on observations of physical phenomena, harder to agree on observations of social or mental phenomena, and difficult in the extreme to reach agreement on matters of theology or ethics (and thus the latter remain outside the normal purview of science).

**Theory-dependence of observations**

When making observations, scientists look through telescopes, study images on electronic screens, record meter readings, and so on. Generally, on a basic level, they can agree on what they see, e.g., the thermometer shows 37.9 C. But, if these scientists have different ideas about the theories that have been developed to explain these basic observations, they can interpret them in different ways. Ancient scientists interpreted the rising of the Sun in the morning as evidence that the Sun moved. Later scientists deduce that the Earth is rotating. For example, if some scientists may conclude that certain observations confirm a specific hypothesis, skeptical colleagues may suspect that something is wrong with the test equipment. Observations when interpreted by a scientist's theories are said to be theory-laden.

Whitehead wrote, "All science must start with some assumptions as to the ultimate analysis of the facts with which it deals. These assumptions are justified partly by their adherence to the types of occurrence of which we are directly conscious, and partly by their success in representing the observed facts with a certain generality, devoid of *ad hoc* suppositions."[22]

Observation involves both perception as well as cognition. That is, one does not make an observation passively, but is also actively engaged in distinguishing the phenomenon being observed from surrounding sensory data. Therefore, observations are affected by our underlying understanding of the way in which the world functions, and that understanding may influence what is perceived, noticed, or deemed worthy of consideration. More importantly, most scientific observation must be done within a theoretical context in order to be useful. For example, when one observes a measured increase in temperature with a thermometer, that observation is based on assumptions about the nature of temperature and its measurement, as well as assumptions about how the thermometer functions. Such assumptions are necessary in order to obtain scientifically useful observations (such as, "the temperature increased by two degrees").

Empirical observation is used to determine the acceptability of hypotheses within a theory. Justification of a hypothesis often includes reference to a theory — operational definitions and hypotheses — in which the observation is embedded. That is, the observation is framed in terms of the theory that also contains the hypothesis it is meant to verify or falsify (though of course the observation should not be based on an assumption of the truth or falsity of the hypothesis being tested). This means that the observation cannot serve as an entirely neutral arbiter between competing hypotheses, but can only arbitrate between hypotheses within the context of the underlying theory that explains the observation.
Thomas Kuhn denied that it is ever possible to isolate the hypothesis being tested from the influence of the theory in which the observations are grounded. He argued that observations always rely on a specific paradigm, and that it is not possible to evaluate competing paradigms independently. By "paradigm" he meant, essentially, a logically consistent "portrait" of the world, one that involves no logical contradictions and that is consistent with observations that are made from the point of view of this paradigm. More than one such logically consistent construct can paint a usable likeness of the world, but there is no common ground from which to pit two against each other, theory against theory. Neither is a standard by which the other can be judged. Instead, the question is which "portrait" is judged by some set of people to promise the most useful in terms of scientific "puzzle solving".

For Kuhn, the choice of paradigm was sustained by, but not ultimately determined by, logical processes. The individual's choice between paradigms involves setting two or more "portraits" against the world and deciding which likeness is most promising. In the case of a general acceptance of one paradigm or another, Kuhn believed that it represented the consensus of the community of scientists. Acceptance or rejection of some paradigm is, he argued, a social process as much as a logical process. Kuhn's position, however, is not one of relativism. According to Kuhn, a paradigm shift will occur when a significant number of observational anomalies in the old paradigm have made the new paradigm more useful. That is, the choice of a new paradigm is based on observations, even though those observations are made against the background of the old paradigm. A new paradigm is chosen because it does a better job of solving scientific problems than the old one.

The fact that observation is embedded in theory does not mean observations are irrelevant to science. Scientific understanding derives from observation, but the acceptance of scientific statements is dependent on the related theoretical background or paradigm as well as on observation. Coherentism, skepticism, and foundationalism are alternatives for dealing with the difficulty of grounding scientific theories in something more than observations. And, of course, further, redesigned testing may resolve differences of opinion.

**Coherentism**

Induction must avoid the problem of the criterion, in which any justification must in turn be justified, resulting in an infinite regress. The regress argument has been used to justify one way out of the infinite regress, foundationalism. Foundationalism claims that there are some basic statements that do not require justification. Both induction and falsification are forms of foundationalism in that they rely on basic statements that derive directly from immediate sensory experience.

The way in which basic statements are derived from observation complicates the problem. Observation is a cognitive act; that is, it relies on our existing understanding, our set of beliefs. An observation of a transit of Venus requires a huge range of auxiliary beliefs, such as those that describe the optics of telescopes, the mechanics of the telescope mount, and an understanding of celestial mechanics, all of which must be justified separately. At first sight, the observation does not appear to be 'basic'.

Coherentism offers an alternative by claiming that statements can be justified by their being a part of a coherent system. In the case of science, the system is usually taken to be the complete set of beliefs of an individual scientist or, more broadly, of the community of scientists. W. V. Quine argued for a Coherentist approach to science, as do E O Wilson and Kenneth Craik, though neither use the term "Coherentism" to describe their views. An observation of a transit of Venus is justified by its being coherent with our beliefs about optics, telescope mounts and celestial mechanics. Where this observation is at odds with one of these auxiliary beliefs, an adjustment in the system will be required to remove the contradiction.
**Ockham's razor**

The practice of scientific inquiry typically involves a number of heuristic principles, such as the principles of conceptual economy or theoretical parsimony. These are customarily placed under the rubric of Ockham's razor, named after the 14th century Franciscan friar William of Ockham, who is credited with many different expressions of the maxim, not all of which have yet been found among his extant works.\(^{[24]}\)

William of Ockham (c. 1295–1349) ... is remembered as an influential nominalist, but his popular fame as a great logician rests chiefly on the maxim known as Ockham's razor: *Entia non sunt multiplicanda praeter necessitatem* ['entities must not be multiplied beyond necessity]. No doubt this represents correctly the general tendency of his philosophy, but it has not so far been found in any of his writings. His nearest pronouncement seems to be *Numquam ponenda est pluralitas sine necessitate* [Plurality must never be posited without necessity], which occurs in his theological work on the Sentences of Peter Lombard (Super Quattuor Libros Sententiarum (ed. Lugd., 1495), i, dist. 27, qua. 2, K).

In his *Summa Totius Logicae*, i, 12, Ockham cites the principle of economy, *Frustra fit per plura quod potest fieri per pauciora* [It is futile to do with more things that which can be done with fewer]. (Kneale and Kneale, 1962, p. 243)

As interpreted in contemporary scientific practice, "entities should not be multiplied beyond necessity" advises opting for the simplest theory among a set of competing theories that have a comparable explanatory power, discarding assumptions that do not improve the explanation. Among the many difficulties that arise in trying to apply Ockham's razor is the problem of formalizing and quantifying the "measure of simplicity" that is implied by the task of deciding which of several theories is the simplest. Although various measures of simplicity have been brought forward as potential candidates, it is generally recognized that there is no such thing as a theory-independent measure of simplicity. In other words, there appear to be as many different measures of simplicity as there are theories themselves, and the task of choosing between measures of simplicity appears to be every bit as problematic as the job of choosing between theories. Moreover, it is extremely difficult to identify the hypotheses or theories that have "comparable explanatory power", though it may be readily possible to rule out some of the extremes. Ockham's razor also does not say that the simplest account is to be preferred regardless of its capacity to explain outliers, exceptions, or other phenomena in question. The principle of falsifiability requires that any exception that can be reliably reproduced should invalidate the simplest theory, and that the next-simplest account which can actually incorporate the exception as part of the theory should then be preferred to the first. As Albert Einstein puts it, "The supreme goal of all theory is to make the irreducible basic elements as simple and as few as possible without having to surrender the adequate representation of a single datum of experience".

**Objectivity of observations in science**

It is vitally important for science that the information about the surrounding world and the objects of study be as accurate and as reliable as possible. For the sake of this, measurements which are the source of this information must be as objective as possible. Before the invention of measuring tools (like weights, meter sticks, clocks, etc.) the only source of information available to humans were their senses (vision, hearing, taste, tactile, sense of heat, sense of gravity, etc.). Because human senses differ from person to person (due to wide variations in personal chemistry, deficiencies, inherited flaws, etc.) there were no objective measurements before the invention of these tools. The consequence of this was the lack of a rigorous science.

With the advent of exchange of goods, trades, and agricultures there arose a need for such measurements, and science (arithmetic, geometry, mechanics, etc.) based on standardized units of measurements (stadia, pounds, seconds, etc.) was born. To further abstract from unreliable human senses and make measurements more objective, science uses measuring devices (like spectrometers, voltmeters, interferometers, thermocouples, counters, etc.) and lately - computers. In most cases, the less human involvement in the measuring process, the more accurate and reliable scientific data are. Currently most measurements are done by a variety of mechanical and electronic sensors directly linked to computers—which further reduces the chance of human error/contamination of information. The current accuracy of measurement of mass is about $10^{-10}$, of angles—about $10^{-9}$, and of time and length intervals in many cases reaches the order of $10^{-13}$ - $10^{-15}$. This has made it possible to measure, say, the distance to the Moon.
with sub-centimeter accuracy (see Lunar laser ranging experiment), to measure slight movement of tectonic plates using GPS with sub-millimeter accuracy, or even to measure as slight variations in the distance between two mirrors separated by several kilometers as $10^{-18}$ m—three orders of magnitude less than the size of a single atomic nucleus—see LIGO.

Another question about the objectivity of observations relates to the so-called "experimenter's regress", as well as to other problems identified from the sociology of scientific knowledge: as with all forms of human reasoning, the people who interpret the observations or experiments always have cognitive and social biases that lead them, often in an unconscious way, to introduce their own interpretations into their description of what they are 'seeing'. Some of these arguments can be shown to be of a limited scope, when analyzed from a game-theoretic point of view. (See also "Rhetoric of Science").

**Philosophy of particular sciences**

In addition to addressing the general questions regarding science and induction, many philosophers of science are occupied by investigating philosophical or foundational problems in particular sciences. The late 20th and early 21st century has seen a rise in the number of practitioners of philosophy of a particular science.

**Philosophy of biology**

Philosophy of biology deals with epistemological, metaphysical, and ethical issues in the biological and biomedical sciences. Although philosophers of science and philosophers generally have long been interested in biology (e.g., Aristotle, Descartes, Leibniz and even Kant), philosophy of biology only emerged as an independent field of philosophy in the 1960s and 1970s. Philosophers of science began to pay increasing attention to developments in biology, from the rise of the Modern synthesis in the 1930s and 1940s to the discovery of the structure of Deoxyribonucleic acid (DNA) in 1953 to more recent advances in genetic engineering. Other key ideas such as the reduction of all life processes to biochemical reactions as well as the incorporation of psychology into a broader neuroscience are also addressed. Research in current philosophy of biology is dominated by investigations about the foundations of evolutionary theory.

**Philosophy of chemistry**

Philosophy of chemistry considers the methodology and underlying assumptions of the science of chemistry. It is explored by philosophers, chemists, and philosopher-chemist teams.

Topics of interest include:

1. The relationship between chemical concepts and reality. Resonance structures are often used in chemical explanations despite their decided non-reality. In a similar sense, the reality of concepts such as nucleophiles and electrophiles has been questioned.
2. Questions regarding whether chemistry studies atoms (substances) or reactions (processes).
3. Symmetry in chemistry, specifically the origin of homochirality in biological molecules
4. Reductionism with respect to physics and questions regarding whether quantum mechanics can fully explain all chemical phenomena.

**Philosophy of physics**

Philosophy of physics is the study of the fundamental, philosophical questions underlying modern physics, the study of matter and energy and how they interact. The main questions concern the nature of space and time, atoms and atomism. Also included are the predictions of cosmology, the results of the interpretation of quantum mechanics, the foundations of statistical mechanics, causality, determinism, and the nature of physical laws. Classically, several of these questions were studied as part of metaphysics (for example, those about causality, determinism, and space and
Philosophy of psychology

Philosophy of psychology refers to issues at the theoretical foundations of modern psychology. Some of these issues are epistemological concerns about the methodology of psychological investigation. For example:

- What is the most appropriate methodology for psychology: mentalism, behaviorism, or a compromise?
- Are self-reports a reliable data gathering method?
- What conclusions can be drawn from null hypothesis tests?
- Can first-person experiences (emotions, desires, beliefs, etc.) be measured objectively?

Other issues in philosophy of psychology are philosophical questions about the nature of mind, brain, and cognition, and are perhaps more commonly thought of as part of cognitive science, or philosophy of mind, such as:

- What is a cognitive module?
- Are humans rational creatures?
- What psychological phenomena comes up to the standard required for calling it knowledge?
- What is innateness?

Philosophy of psychology also closely monitors contemporary work conducted in cognitive neuroscience, evolutionary psychology, and artificial intelligence, questioning what they can and cannot explain in psychology.

Philosophy of psychology is a relatively young field, because psychology only became a discipline of its own in the late 1800s. Philosophy of mind, by contrast, has been a well-established discipline since before psychology was a field of study at all. It is concerned with questions about the very nature of mind, the qualities of experience, and particular issues like the debate between dualism and monism.

Also, neurophilosophy has become its own field with the works of Paul and Patricia Churchland.

Philosophy of economics

Philosophy of economics is the branch of philosophy which studies philosophical issues relating to economics. It can also be defined as the branch of economics which studies its own foundations and morality.

Philosophy of mathematics

Philosophy of mathematics is the branch of philosophy that studies the philosophical assumptions, foundations, and implications of mathematics.

Recurrent themes include:

- What are the sources of mathematical subject matter?
- What is the ontological status of mathematical entities?
- What does it mean to refer to a mathematical object?
- What is the character of a mathematical proposition?
- What is the relation between logic and mathematics?
- What is the role of hermeneutics in mathematics?
- What kinds of inquiry play a role in mathematics?
- What are the objectives of mathematical inquiry?
- What gives mathematics its hold on experience?
- What are the human traits behind mathematics?
- What is mathematical beauty?
- What is the source and nature of mathematical truth?
- What is the relationship between the abstract world of mathematics and the material universe?
- What is a number?
• Are mathematical proofs exercises in tautology?
• Why does it make sense to ask whether "1+1=2" is true?
• How do we know whether a mathematical proof is correct?

**Positivism and social science**

The French philosopher, Auguste Comte (1798–1857), established the epistemological perspective of positivism in *The Course in Positivist Philosophy*, a series of texts published between 1830 and 1842. These texts were followed by the 1844 work, *A General View of Positivism* (published in English in 1865). The first three volumes of the *Course* dealt chiefly with the physical sciences already in existence (mathematics, astronomy, physics, chemistry, biology), whereas the latter two emphasised the inevitable coming of social science: "sociologie". Observing the circular dependence of theory and observation in science, and classifying the sciences in this way, Comte may be regarded as the first philosopher of science in the modern sense of the term.[27] For him, the physical sciences had necessarily to arrive first, before humanity could adequately channel its efforts into the most challenging and complex "Queen science" of human society itself. Comte offers an evolutionary system proposing that society undergoes three phases in its quest for the truth according to a general 'law of three stages'. These are (1) the *theological*, (2) the *metaphysical*, and (3) the *positive*. [28]

Comte's positivism laid the initial philosophical foundations for formal sociology and social research. Durkheim, Marx, and Weber are more typically cited as the fathers of contemporary social science. In psychology, a positivistic approach has historically been favoured in behaviourism. In the early 20th century, logical positivism—a stricter version of Comte's basic thesis but a broadly independent movement—sprang up in Vienna and grew to become one of the dominant movements in Anglo-American philosophy and the analytic tradition. Logical positivists (or 'neopositivists') reject metaphysical assertions and attempt to reduce statements and propositions to pure logic.

The positivist perspective, however, has been associated with 'scientism'; the view that the methods of the natural sciences may be applied to all areas of investigation, be it philosophical, social scientific, or otherwise. Among most social scientists and historians, orthodox positivism has long since fallen out of favor. Today, practitioners of both social and physical sciences recognize the distorting effect of observer bias and structural limitations. This scepticism has been facilitated by a general weakening of deductivist accounts of science by philosophers such as Thomas Kuhn, and new philosophical movements such as critical realism and neopragmatism. Positivism has also been espoused by 'technocrats' who believe in the inevitability of social progress through science and technology.[29]

The philosopher-sociologist Jürgen Habermas has critiqued pure instrumental rationality as meaning that scientific-thinking becomes something akin to ideology itself.[30]

**Social accountability**

**Scientific openness**

A very broad issue affecting the neutrality of science concerns the areas over which science chooses to explore, so what part of the world and man is studied by science. Since the areas for science to investigate are theoretically infinite, the issue then arises as to what science should attempt to question or find out.

Philip Kitcher in his "Science, Truth, and Democracy"[31] argues that scientific studies that attempt to show one segment of the population as being less intelligent, successful or emotionally backward compared to others have a political feedback effect which further excludes such groups from access to science. Thus such studies undermine the broad consensus required for good science by excluding certain people, and so proving themselves in the end to be unscientific.
Critiques of scientific method

Paul Feyerabend argued that no description of scientific method could possibly be broad enough to encompass all the approaches and methods used by scientists. Feyerabend objected to prescriptive scientific method on the grounds that any such method would stifle and cramp scientific progress. Feyerabend claimed, "the only principle that does not inhibit progress is: anything goes."[32]

Sociology, anthropology and economics of science

In his book *The Structure of Scientific Revolutions* Kuhn argues that the process of observation and evaluation take place within a paradigm. 'A paradigm is what the members of a community of scientists share, and, conversely, a scientific community consists of men who share a paradigm'.[33] On this account, science can be done only as a part of a community, and is inherently a communal activity.

For Kuhn, the fundamental difference between science and other disciplines is in the way in which the communities function. Others, especially Feyerabend and some post-modernist thinkers, have argued that there is insufficient difference between social practices in science and other disciplines to maintain this distinction. It is apparent that social factors play an important and direct role in scientific method, but that they do not serve to differentiate science from other disciplines. Furthermore, although on this account science is socially constructed, it does not follow that reality is a social construct. (See Science studies and the links there.) Kuhn's ideas are equally applicable to both realist and anti-realist ontologies.

There are, however, those who maintain that scientific reality is indeed a social construct, to quote Quine:

> Physical objects are conceptually imported into the situation as convenient intermediaries not by definition in terms of experience, but simply as irreducible posits comparable, epistemologically, to the gods of Homer . . . For my part I do, qua lay physicist, believe in physical objects and not in Homer's gods; and I consider it a scientific error to believe otherwise. But in point of epistemological footing, the physical objects and the gods differ only in degree and not in kind. Both sorts of entities enter our conceptions only as cultural posits[34]

A major development in recent decades has been the study of the formation, structure, and evolution of scientific communities by sociologists and anthropologists including Michel Callon, Bruno Latour, John Law, Anselm Strauss, Lucy Suchman, and others. Some of their work has been previously loosely gathered in actor network theory. Here the approach to the philosophy of science is to study how scientific communities actually operate.

Concepts and methods from economic science (as rational choice, social choice or game theory) have also been applied for understanding the efficiency of scientific communities in the production of epistemic outputs. (Main entry 'Economics of scientific knowledge').

Continental philosophy of science

In the Continental philosophical tradition, science is viewed from a world-historical perspective. One of the first philosophers who supported this view was Georg Wilhelm Friedrich Hegel. Philosophers such as Pierre Duhem and Gaston Bachelard also wrote their works with this world-historical approach to science. Nietzsche advanced the thesis in his "The Genealogy of Morals" that the motive for search of truth in sciences is a kind of ascetic ideal.

All of these approaches involve a historical and sociological turn to science, with a special emphasis on lived experience (a kind of Husserlian "life-world"), rather than a progress-based or anti-historical approach as done in the analytic tradition. This emphasis can be traced through Edmund Husserl's phenomenology, the late works of Merleau-Ponty (*Nature: Course Notes from the Collège de France*, 1956–1960), and Martin Heidegger's hermeneutics.
The largest effect on the continental tradition with respect to science was Martin Heidegger's critique of the theoretical attitude in general which of course includes the scientific attitude. For this reason the Continental tradition has remained much more skeptical of the importance of science in human life and philosophical inquiry. Notwithstanding, there have been a number of important works: especially a Kuhnian precursor, Alexandre Koyré. Another important development was that of Foucault's analysis of the historical and scientific thought in The Order of Things and his study of power and corruption under the "science" of madness. Several post-Heideggerian authors contributing to the Continental philosophy of science in the second half of the 20th century include Jürgen Habermas (e.g., "Truth and Justification", 1998), Carl Friedrich von Weizsäcker ("The Unity of Nature", 1980), and Wolfgang Stegmüller ("Probleme und Resultate der Wissenschaftstheorie und Analytischen Philosophie", 1973–1986).

Notes

[24] Ockham's razor, however, was not originally a principle of science but of theology and the issue of parsimony comes, not from science, but from the vow of poverty that was modeled on the life of Christ. However, the origins of the idea do not necessarily take away from its overall usefulness.
Further reading

• Ladyman, J. (2002), Understanding Philosophy of Science, Routledge, London, UK.
• Salmon, M.H., et al. (1999), Introduction to the Philosophy of Science: A Text By Members of the Department of the History and Philosophy of Science of the University of Pittsburgh, Hacket Publishing Company, Indianapolis, IN.
• Snyder, Paul (1977), Toward One Science: The Convergence of Traditions, St Martin's Press.
• van Fraassen, Bas C. (2008), Scientific Representation: Paradoxes of Perspective, Oxford University Press, Oxford, UK.

External links
• Philosophy of science (http://philpapers.org/browse/general-philosophy-of-science) at PhilPapers
• Philosophy of science (https://inpho.cogs.indiana.edu/taxonomy/2218) at the Indiana Philosophy Ontology Project
• Philosophy of science (http://www.iep.utm.edu/category/s-l-m/science/) entry in the Internet Encyclopedia of Philosophy
• An introduction to the Philosophy of Science, aimed at beginners - Paul Newall. (http://www.galilean-library.org/manuscript.php?postid=43784)
• Centre for Philosophy of Natural and Social Science at the London School of Economics (http://www2.lse.ac.uk/CPNSS/Home.aspx)
• Interdisciplinary Centre for Science and Technology Studies (http://www.izwt.uni-wuppertal.de)
• Essays on concepts in the Philosophy of Science (http://www.galilean-library.org/hps.php) at The Galilean Library.
Logical positivism

Logical positivism (also known as logical empiricism, scientific philosophy, and neo-positivism) is a philosophy that combines empiricism—the idea that observational evidence is indispensable for knowledge—with a version of rationalism incorporating mathematical and logico-linguistic constructs and deductions of epistemology. It may be considered as a type of analytic philosophy.[1]

Logical positivism, in the formal sense, began from discussions of a group known as the First Vienna Circle which gathered during the earliest years of the 20th century in Vienna at the Café Central. After World War I, Hans Hahn, a member of that early group, helped bring Moritz Schlick to Vienna. Schlick's Vienna Circle, along with Hans Reichenbach's Berlin Circle, propagated the new doctrines more widely during the 1920s and early 1930s. It was Otto Neurath's advocacy that made the movement self-conscious and more widely known. A 1929 pamphlet written by Neurath, Hahn, and Rudolf Carnap summarized the doctrines of the Vienna Circle at that time. The doctrines included the opposition to all metaphysics, especially ontology and synthetic a priori propositions; the rejection of metaphysics not as wrong but as having no meaning; a criterion of meaning based on Ludwig Wittgenstein's early work; the idea that all knowledge should be codifiable by a single standard language of science; and above all the project of rational reconstruction, in which ordinary-language concepts were gradually to be replaced by more precise equivalents in that standard language.

During the early 1930s, the Vienna Circle dispersed, mainly because of political upheaval and the deaths of Hahn and Schlick. The most prominent proponents of logical positivism emigrated to the United Kingdom and the United States, where they influenced American philosophy considerably. Until the 1950s, logical positivism was the leading school in the philosophy of science. Ultimately, it failed to solve many of the problems with which it was centrally concerned,[2][3][4] and after the Second World War, its doctrines increasingly came under attack by thinkers such as Nelson Goodman, Willard Van Orman Quine, J. L. Austin, Peter Strawson, Hilary Putnam, and Richard Rorty.

Summary

During the late 1920s, '30s, and '40s, Bertrand Russell and Ludwig Wittgenstein's formalism was developed by a group of philosophers in Vienna and Berlin, who formed the Vienna Circle and Berlin Circle into a doctrine known as logical positivism (or logical empiricism). Logical positivism used formal logic to underpin an empiricist account of our knowledge of the world.[5] Philosophers such as Rudolf Carnap and Hans Reichenbach, along with other members of the Vienna Circle, claimed that the truths of logic and mathematics were tautologies, and those of science were verifiable empirical claims. These two constituted the entire universe of meaningful judgements; anything else was nonsense. The claims of ethics and aesthetics were subjective preferences. Theology and other metaphysics were pseudo-statements, neither true nor false, simply meaningless nonsense.

Karl Popper's insistence upon the role of falsification in the philosophy of science was a reaction to the logical positivists.[6] With the rise of Adolf Hitler and National Socialism in Germany and Austria, some members of the
Logical positivism and analytic philosophy in the Anglophone world.

Logical positivists typically considered philosophy as having a very limited function. For them, philosophy is concerned with the organization of thoughts, rather than having distinct topics of its own. The positivists adopted the principle of verificationism, according to which every meaningful statement is either analytic or is capable of being verified by experience. This caused the logical positivists to reject many traditional problems of philosophy, especially those of metaphysics or ontology, as meaningless.

**Themes**

Logical positivism was a movement without a fixed set of doctrines. The logical positivists held a wide range of views on many matters. Nonetheless, they were all interested in science and skeptical of theology and metaphysics.

Early, most logical positivists proposed that all knowledge is based on logical inference from simple "protocol sentences" grounded in observable facts. Many logical positivists endorsed forms of materialism, metaphysical naturalism, and empiricism. (See James Ladyman, *Understanding Philosophy of Science*, p. 147)

**Verifiability criterion of meaning**

Perhaps the view for which the logical positivists are best known is the verifiability criterion of meaning, or verificationism. In one of its earlier and stronger formulations, this is the doctrine that a proposition is "cognitively meaningful" only if there is a finite procedure for conclusively determining its truth. An intended consequence of this opinion, for most logical positivists, is that metaphysical, theological, and ethical statements fail this criterion, and so are not cognitively meaningful. They distinguished cognitive from other varieties of meaningfulness (e.g. emotive, expressive, figurative), and most authors concede that the non-cognitive statements of the history of philosophy possess some other kind of meaningfulness. The positive characterization of cognitive meaningfulness varies from author to author. It has been described as the property of having a truth value, corresponding to a possible state of affairs, naming a proposition, or being intelligible or understandable in the sense in which scientific statements are intelligible or understandable.

**Strong and weak verificationism**

In response to criticism of verificationism, A. J. Ayer proposed a weak version. In *Language, Truth and Logic* he defines the distinction between "strong" and "weak" verification: "A proposition is said to be verifiable, in the strong sense of the term, if, and only if, its truth could be conclusively established by experience." (Ayer 1946:50) It is this sense of verifiable that causes the problem of verification with negative existential claims and positive universal claims. However, weak sense of verification states that a proposition is "verifiable... if it is possible for experience to render it probable" (ibid.). After establishing this distinction, Ayer claims that "no proposition, other than a tautology, can possibly be anything more than a probable hypothesis" (Ayer 1946:51), and therefore can only be subject to weak verification. This defense was controversial among logical positivists, some of whom touted strong verification, and claimed that general propositions were indeed nonsense.

**Analytic and synthetic knowledge**

Logical positivists divided knowledge into analytic and synthetic categories. Analytic knowledge, such as mathematical theorems, is tautological (it is entirely deducable from its presuppositions) and thus can be validated *a priori*. Synthetic knowledge, such as assertions about the real world, must be verified *a posteriori* by observation. Logical positivists rejected the existence of any synthetic *a priori* knowledge. (For example, the scientific progress of general relativity demonstrates that philosophers are wrong to pronounce *a priori* that space should have a Euclidean nature.) The analytic-synthetic distinction was attacked by Quine's 1951 paper "Two Dogmas of Empiricism". Logical positivists also distinguished observational and theoretical terms. This distinction was
criticised by Popper, who emphasised even basic observations as being "theory-laden".[8]

Unified science
Another characteristic feature of logical positivism is the commitment to "Unified Science"; that is, the development of a common language or, in Neurath's phrase, a "universal slang" in which all scientific propositions can be expressed.[12] The adequacy of proposals or fragments of proposals for such a language was often asserted on the basis of various "reductions" or "explications" of the terms of one special science to the terms of another, putatively more fundamental one. Sometimes these reductions consisted of set-theoretic manipulations of a few logically primitive concepts (as in Carnap's (1928) Logical Structure of the World); sometimes these reductions consisted of allegedly analytic or a priori deductive relationships (as in Carnap's Testability and Meaning). A number of publications over a period of thirty years would attempt to elucidate this concept.

History
Origins
The main influences on the early logical positivists were the positivist Ernst Mach, Gottlob Frege, Bertrand Russell and the young Ludwig Wittgenstein.

Mach's influence is most apparent in the logical positivists' persistent concern with metaphysics, the unity of science, and the interpretation of the theoretical terms of science, as well as the doctrines of reductionism and phenomenalism, later abandoned by many positivists.

Wittgenstein's Tractatus Logico-Philosophicus was a text of great importance for the positivists. The Tractatus introduced many doctrines which later influenced logical positivism, including the concept of philosophy as a "critique of language," and the possibility of making a theoretically principled distinction between intelligible and nonsensical discourse. The Tractatus also adhered to a correspondence theory of truth which the positivists adopted, although some, like Otto Neurath, preferred a form of coherentism. Wittgenstein's influence is also evident in certain formulations of the verification principle. Compare, for example, Proposition 4.024 of the Tractatus, where Wittgenstein asserts that we understand a proposition when we know what happens if it is true, with Schlick's assertion that "To state the circumstances under which a proposition is true is the same as stating its meaning."[13]

The tractarian doctrine that the truths of logic are tautologies was widely believed among the logical positivists. Wittgenstein also influenced the logical positivists' interpretation of probability. According to Neurath, some logical positivists disliked the Tractatus, since they thought it included a great deal of metaphysics.[14]

Contemporary developments in logic and the foundations of mathematics, especially Bertrand Russell and Alfred North Whitehead's monumental Principia Mathematica, impressed the more mathematically minded logical positivists such as Hans Hahn and Rudolf Carnap. "Language-planning" and syntactical techniques derived from these developments were used to defend logicism in the philosophy of mathematics and various reductionist theses. Russell's theory of types was employed to great effect in Carnap's early anti-metaphysical polemics.[15]

Immanuel Kant also had an important influence on the positivists, both positive and negative. Negatively, Kant was often scorned by the positivists in their early debates, and Kant's doctrine of synthetic a priori truths was the doctrine they most wished to discredit. However, Kant's opinions about the nature of physical objects pervaded the protocol sentence debate,[16] and Kantian opinions of the relationship between philosophy and science were shared by the positivists to some degree.[17]
Logical positivism in Germany

Positivism in Germany is thought to have developed in response to Hegelian and neo-Hegelian metaphysics, which was a famous philosophy in Germany.\[^{18}\] Hegelian successors such as F.H. Bradley attempted to explain reality by postulating metaphysical entities that did not have any empirical basis.\[^{18}\] Logical positivists in response wanted to stop such metaphysical entities from being used as an explanation.

Another, less well-known factor that encouraged logical positivism was the urgency of solving new philosophical problems raised by new scientific developments. The Vienna Circle under the influence of Moritz Schlick and the Berlin Circle under the influence of Hans Reichenbach consisted of scientists, mathematicians, and scientists turned philosophers, who shared a common goal of solving new problems in the philosophy of science.

Influence

Logical positivism spread throughout almost the entire western world. It was disseminated throughout the European continent. It was spread to Britain by the influence of A. J. Ayer. And later, it was brought to American universities by members of the Vienna Circle after they fled Europe and settled in the United States during and after WWII. Logical positivism was essential to the development of early analytic philosophy. The term subsequently came to be almost interchangeable with "analytic philosophy" during the first half of the twentieth century. Logical positivism was immensely influential in the philosophy of language. It represented the dominant philosophy of science between World War I and the Cold War.

Criticisms

Early critics of logical positivism said that its fundamental tenets could not themselves be formulated consistently. The verifiability criterion of meaning did not seem verifiable; but neither was it simply a logical tautology, since it had implications for the practice of science and the empirical truth of other statements. This presented severe problems for the logical consistency of the theory.\[^{19}\]

Another problem was that universal claims (e.g. "(all) philosophers are mortal") are problematic in terms of verification.\[^{20}\][\(^{*}\)^{21}] The verifiability criterion was seen as being too strong. In its initial formulation, it made universal statements meaningless, and this was seen as a problem for science. This led to the weakening of the criterion.\[^{20}\][\(^{*}\)^{21}]

Witgenstein's principle of verifiability posed fairly obvious problems in any scientific context. No universal generalization can ever be verified. Perhaps independently, Karl Popper perceived the same problem... This led him to replace the requirement of verifiability with that of falsifiability, though only as a criterion to demarcate science from metaphysics, and not as one to be also used to demarcate meaningful from meaningless claims. It is also unclear what the status of the principle itself is, that is, whether it is meaningful by its own criterion of meaningfulness. Carnap, as well as other members of the Vienna Circle including Hahn and Neurath, realized that a weaker criterion of meaningfulness was necessary. Thus began the program of the "liberalization of empiricism." There was no unanimity within the Vienna Circle on this point. The differences between the members are sometimes described as those between a conservative "right" wing, led by Schlick and Waismann, which rejected both the liberalization of empiricism and the epistemological antifoundationalism of the move [from phenomenalism] to physicalism, and a radical "left" wing, led by Neurath and Carnap, which endorsed the opposite views. The "left" wing also emphasized fallibilism and pragmatics; Carnap went far enough along this line to suggest that empiricism itself was a proposal to be accepted on pragmatic grounds. This difference also reflected political attitudes insofar as Neurath and, to a lesser extent, Carnap viewed science as a tool for social reform.

The precise formulation of what came to be called the criterion of cognitive significance took three decades (see Hempel 1950; Carnap 1956 and 1961)... In an important pair of papers, "Testability and Meaning," Carnap (1936-1937) replaced the requirement of verification with that of confirmation; at this stage, he made
no attempt to quantify the latter. Individual terms replace sentences as the unit of meaning. Universal generalizations are no longer problematic; though they cannot be conclusively verified, they can yet be confirmed. Moreover, in "Testability and Meaning," theoretical terms no longer require explicit definition from observational ones in order to acquire meaning; the connection between the two may be indirect through a system of implicit definitions. Carnap also provides an important pioneering discussion of disposition predicates. [23]

—Sahotra Sarkar, *The Philosophy of Science: An Encyclopedia*

**Karl Popper's objection**

A well-known critic of logical positivism was Karl Popper, who published the book *Logik der Forschung* in 1934 (translated by himself as *The Logic of Scientific Discovery*, published 1959). In it he argued that the positivists' criterion of verifiability was too strong a criterion for science, and should be replaced by a criterion of falsifiability. Popper thought that falsifiability was a better criterion because it did not invite the philosophical problems inherent in verifying an inductive inference, and it allowed statements from the physical sciences which seemed scientific but which did not satisfy the verification criterion.

Popper's concern was not with distinguishing meaningful from meaningless statements, but distinguishing scientific from metaphysical statements. Unlike the positivists, he did not claim that metaphysical statements must be meaningless; he also claimed that a statement which was "metaphysical" and unfalsifiable in one century (like the ancient Greek philosophy about atoms) could, in another century, be developed into falsifiable theories that have the metaphysical views as a consequence, and thus become scientific.

Popper denied that science need rely on inductive reasoning, or that inductive reasoning actually exists, although most philosophers think it obvious that science does rely on it. [24]

**Hilary Putnam's objection**

According to Hilary Putnam, a former student of Hans Reichenbach and Rudolf Carnap, making an observational/theoretical distinction is meaningless. The "received view" operates on the correspondence rule that states "The observational terms are taken as referring to specified phenomena or phenomenal properties, and the only interpretation given to the theoretical terms is their explicit definition provided by the correspondence rules." [18] Putnam argues that introducing this dichotomy of observational terms and theoretical terms is the problem from which to start. [25] Putnam demonstrates this with four objections:

1. Something is referred to as "observational" if it is observable directly with our senses. Then an observation term cannot be applied to something unobservable. If this is the case, there are no observation terms.
2. With Carnap's classification, some unobservable terms are not even theoretical and belong to neither observation terms nor theoretical terms. Some theoretical terms refer primarily to observation terms.
3. Reports of observation terms frequently contain theoretical terms.
4. A scientific theory may not contain any theoretical terms (an example of this is Darwin's original theory of evolution).

**Subsequent objections from Quine and Kuhn**

Subsequent philosophy of science tends to use certain aspects of both of these approaches. Willard Van Orman Quine criticized the distinction between analytic and synthetic statements and the reduction of meaningful statements to immediate experience. Work by Thomas Kuhn has claimed that it is not possible to provide truth conditions for science independent of its historical paradigm. But even this criticism was not unknown to the logical positivists: Otto Neurath compared science to a boat which we must rebuild on the open sea.
Contemporary status within philosophy

Key tenets of logical positivism, including its atomistic philosophy of science, the verifiability principle, and the fact–value distinction, came under attack after the Second World War by philosophers such as Nelson Goodman, Quine, J. L. Austin, and Peter Strawson. Nicholas G. Fotion comments that "By the late 1960s it became obvious that the movement had pretty much run its course." Most philosophers consider logical positivism to be, as John Passmore expressed it, "dead, or as dead as a philosophical movement ever becomes." By the late 1970s, its ideas were so generally recognized to be seriously defective that one of its own main proponents, A. J. Ayer, could say in an interview: "I suppose the most important [defect]...was that nearly all of it was false." It retains an important place in the history of analytic philosophy as the antecedent of contemporary philosophies, such as constructive empiricism, positivism, and postpositivism.

Notes

[2] Smith, L.D. (1986). Behaviorism and Logical Positivism: A Reassessment of the Alliance (http://books.google.com/books?id=EF6aAAAMAAJ&pg=PA314). Stanford University Press. p. 314. ISBN 9780804713016. LCCN 85030366. “The secondary and historical literature on positivism affords substantial grounds for concluding that logical positivism failed to solve many of the central problems it generated for itself. Prominent among the unsolved problems was the failure to find an acceptable statement of the verifiability (later confirmability) criterion of meaningfulness. Until a competing tradition emerged (about the late 1950s), the problems of logical positivism continued to be attacked from within that tradition. But as the new tradition in the philosophy of science began to demonstrate its effectiveness—by dissolving and rephrasing old problems as well as by generating new ones—philosophers began to shift allegiances to the new tradition, even though that tradition has yet to receive a canonical formulation.”
[3] Bunge, M.A. (1996). Finding Philosophy in Social Science (http://books.google.com/books?id=8YAV43gVMsIC&pg=PA317). Yale University Press. p. 317. ISBN 9780300066067. LCCN lc96004399. “To conclude, logical positivism was progressive compared with the classical positivism of Ptolemy, Hume, d'Alembert, Compte, Mill, and Mach. It was even more so by comparison with its contemporary rivals—neo-Thomism, neo-Kantianism, intuitionism, dialectical materialism, phenomenology, and existentialism. However, neo-positivism failed dismally to give a faithful account of science, whether natural or social. It failed because it remained anchored to sense-data and to a phenomenalist metaphysics, overrated the power of induction and underrated that of hypothesis, and denounced realism and materialism as metaphysical nonsense. Although it has never been practiced consistently in the advanced natural sciences and has been criticized by many philosophers, notably Popper (1959 [1935], 1963), logical positivism remains the tacit philosophy of many scientists. Regrettably, the anti-positivism fashionable in the metatheory of social science is often nothing but an excuse for sloppiness and wild speculation.”
[4] Popper, Falsifiability, and the Failure of Positivism (http://www.drury.edu/ess/philscl/popper.html). 7 August 2000. Retrieved 30 June 2012. “The upshot is that the positivists seem caught between insisting on the V.C. [Verifiability Criterion]—but for no defensible reason—or admitting that the V.C. requires a background language, etc., which opens the door to relativism, etc. In light of this dilemma, many folk—especially following Popper's "last-ditch" effort to "save" empiricism/positivism/realism with the falsifiability criterion—have agreed that positivism is a dead-end.”
[7] Prominent amongst these were Ludwig Wittgenstein and Rudolf Carnap. Karl Popper might also be included, despite his rejection of the term his method has much in common with the analytic tradition.
[14] For a very informative summary of the effect the Tractatus had on the main logical positivists, see the Entwicklung der Thesen des "Wiener Kreises" (http://www.pitt.edu/~philosop/images/doc4-full.jpg)

[16] See the essays by Schlick, Carnap, and Neurath in Ayer's Logical Positivism.


[20] Fetzer, James (2012). "Carl Hempel" (http://plato.stanford.edu/entries/hempel/#SciRea). In Edward N. Zalta. The Stanford Encyclopedia of Philosophy. . "It would fall to Hempel to become perhaps the most astute critic of that movement and to contribute to its refinement as logical empiricism... Hempel himself attained a certain degree of prominence as a critic of this movement... The analytic/synthetic distinction and the observational/theoretical distinction were tied together by the verifiability criterion of meaningfulness... By this standard, sentences that are non-analytic but also non-verifiable, including various theological or metaphysical assertions concerning God or The Absolute, qualify as cognitively meaningless. This was viewed as a desirable result. But, as Hempel would demonstrate, its scope was far too sweeping, since it also rendered meaningless the distinctively scientific assertions made by laws and theories... The analytic/synthetic distinction took a decided hit when the noted logician, Willard van Orman Quine, published "Two Dogmas of Empiricism" (1953), challenging its adequacy... While the analytic/synthetic distinction appears to be justifiable in modeling important properties of languages, the observational/theoretical distinction does not fare equally well. Within logical positivism, observation language was assumed to consist of names and predicates whose applicability or not can be ascertained, under suitable conditions, by means of direct observation... Karl Popper (1965, 1968), however, would carry the argument in a different direction by looking at the ontic nature of properties... Hempel (1950, 1951), meanwhile, demonstrated that the verifiability criterion could not be sustained. Since it restricts empirical knowledge to observation sentences and their deductive consequences, scientific theories are reduced to logical constructions from observables. In a series of studies about cognitive significance and empirical testability, he demonstrated that the verifiability criterion implies that existential generalizations are meaningful, but that universal generalizations are not, even though they include general laws, the principal objects of scientific discovery. Hypotheses about relative frequencies in finite sequences are meaningful, but hypotheses concerning limits in infinite sequences are not. The verifiability criterion thus imposed a standard that was too strong to accommodate the characteristic claims of science and was not justifiable... Both theoretical and dispositional predicates, which refer to non-observables, posed serious problems for the positivist position, since the verifiability criterion implies they must be reducible to observables or are empirically meaningless... The need to dismantle the verifiability criterion of meaningfulness together with the demise of the observational/theoretical distinction meant that logical positivism no longer represented a rationally defensible position. At least two of its defining tenets had been shown to be without merit. Since most philosophers believed that Quine had shown the analytic/synthetic distinction was also untenable, moreover, many concluded that the enterprise had been a total failure. Among the important benefits of Hempel's critique, however, was the production of more general and flexible criteria of cognitive significance... Hempel suggested multiple criteria for assessing the cognitive significance of different theoretical systems, where significance is not categorical but rather a matter of degree... The elegance of Hempel's study laid to rest any lingering aspirations for simple criteria of cognitive significance and signaled the demise of logical positivism as a philosophical movement. Precisely what remained, however, was in doubt. Presumably, anyone who rejected one or more of the three principles defining positivism—the analytic/synthetic distinction, the observational/theoretical distinction, and the verifiability criterion of significance—was not a logical positivist. The precise outlines of its philosophical successor, which would be known as "logical empiricism", were not entirely evident. Perhaps this study came the closest to defining its intellectual core. Those who accepted Hempel's four criteria and viewed cognitive significance as a matter of degree were members, at least in spirit. But some new problems were beginning to surface with respect to Hempel's covering-law explication of explanation and old problems remained from his studies of induction, the most remarkable of which was known as the paradox of confirmation.

[21] Uebel, Thomas (2008). "Vienna Circle" (http://plato.stanford.edu/archives/fall2008/entries/vienna-circle/). In Edward N. Zalta. The Stanford Encyclopedia of Philosophy. . "What Carnap later called the "liberalization of empiricism" was underway and different camps became discernible within the Circle... In the first place, this liberalization meant the accomodation of universally quantified statements and the return, as were, to salient aspects of Carnap's 1928 conception. Everybody had noted that the Wittgensteinian verificationist criterion rendered universally quantified statements meaningless. Schlick (1931) thus followed Wittgenstein's own suggestion to treat them instead as representing rules for the formation of verifiable singular statements. (His abandonment of conclusive verifiability is indicated only in Schlick 1936a.) A second element that began to do so soon was the recognition of the problem of the irreducibility of disposition terms to observation terms... A third element was that disagreement arose as to whether the in-principle verifiability or support turned on what was merely logically possible or on what was nomologically possible, as a matter of physical law etc. A fourth element, finally, was that differences emerged as to whether the criterion of significance was to apply to all languages or whether it was to apply primarily to constructed, formal languages. Schlick retained the focus on logical possibility and natural languages throughout, but Carnap had firmly settled his focus on nomological possibility and constructed languages by the mid-thirties. Concerned with natural language, Schlick (1932, 1936a) deemed all statements meaningful for which it was logically possible to conceive of a procedure of verification; concerned with constructed languages only, Carnap (1936-37) deemed meaningful only statements for which it was nomologically possible to conceive of a procedure of confirmation of disconfirmation.

Many of these essays were openly discussed at the Paris congress in 1935. Already in 1932 Carnap had sought to sharpen his previous criterion by stipulating that those statements were meaningful that were syntactically well-formed and whose non-logical terms were reducible to terms.
occurring in the basic observational evidence statements of science. While Carnap's focus on the reduction of descriptive terms allows for the conclusive verification of some statements, his criterion also allowed universally quantified statements to be meaningful, provided they were syntactically and terminologically correct (1932a, §2). It was not until one of his Paris addresses, however, that Carnap officially declared the meaning criterion to be merely confirmability. Carnap's new criterion required neither verification nor falsification but only partial testability so as now to include not only universal statements but also the disposition statements of science... Though plausible initially, the device of introducing non-observational terms in this way gave rise to a number of difficulties which impugned the supposedly clear distinctions between logical and empirical matters and analytic and synthetic statements (Hempel 1951). Independently, Carnap himself (1939) soon gave up the hope that all theoretical terms of science could be related to an observational base by such reduction chains. This admission raised a serious problem for the formulation of a meaning criterion: how was one to rule out unwanted metaphysical claims while admitting as significant highly abstract scientific claims?''

[22] Vicker, John (2011). "The Problem of Induction" (http://plato.stanford.edu/archives/fall2011/entries/induction-problem/#VerCon). In Edward N. Zalta, The Stanford Encyclopedia of Philosophy. . "This initial formulation of the criterion was soon seen to be too strong; it counted as meaningless not only metaphysical statements but also statements that are clearly empirically meaningful, such as that all copper conducts electricity and, indeed, any universally quantified statement of infinite scope, as well as statements that were at the time beyond the reach of experience for technical, and not conceptual, reasons, such as that there are mountains on the back side of the moon. These difficulties led to modification of the criterion: The latter to allow empirical verification if not in fact then at least in principle, the former to soften verification to empirical confirmation."


Further reading


**External links**

Articles by logical positivists

• Carnap, Rudolf. 'Empiricism, Semantics, and Ontology.' (http://www.ditext.com/carnap/carnap.html)
• Hempel, Carl. 'Problems and Changes in the Empiricist Criterion of Meaning.' (http://www.lawrence.edu/fast/boardmaw/Hempel_Emp_Crit.html)

Articles on logical positivism

• Logical Empiricism (http://plato.stanford.edu/entries/logical-empiricism/) entry by Richard Creath in the *Stanford Encyclopedia of Philosophy*
• Kemerling, Garth. 'Logical Positivism', *Philosophy Pages* (http://www.philosophypages.com/hy/6q.htm)
• Murzi, Mauro. 'The Philosophy of Logical Positivism.' (http://murzim.net/LP/LP00.html)

Articles on related philosophical topics


# Karl Popper

**Karl Popper**  
CH FRS FBA

![Sir Karl Popper c. 1980s](image)

| **Born**     | Karl Raimund Popper  
28 July 1902  
Vienna, Austria-Hungary          |
|-------------|---------------------|
| **Died**    | 17 September 1994 (aged 92)  
London, England                  |
| **Nationality** | Austro-British      |
| **Era**     | 20th century philosophy |
| **Region**  | Western philosophy   |
| **Religion** | Agnosticism          |
| **School**  | Critical rationalism  
Liberalism                      |
| **Main interests** | Epistemology  
Rationality  
Philosophy of science  
Logic  
Social and political philosophy  
Metaphysics  
Philosophy of mind  
Origin of life  
Interpretation of Quantum mechanics |
Sir Karl Raimund Popper, CH FRS (28 July 1902 – 17 September 1994) was an Austro-British philosopher and professor at the London School of Economics. He is generally regarded as one of the greatest philosophers of science of the 20th century. He also wrote extensively on social and political philosophy. In 1992 he was awarded the Kyoto Prize in Arts and Philosophy for "symbolizing the open spirit of the 20th century" and for his "enormous influence on the formation of the modern intellectual climate".

Popper is known for his attempt to repudiate the classical observationalist/inductivist form of scientific method in favour of empirical falsification. He is also known for his opposition to the classical justificationist account of knowledge which he replaced with critical rationalism, "the first non justificational philosophy of criticism in the history of philosophy." In political discourse, he is known for his vigorous defence of liberal democracy and the principles of social criticism that he came to believe made a flourishing "open society" possible.

Personal life

Karl Popper was born in Vienna (then in Austria-Hungary) in 1902, to upper middle-class parents. All of Karl Popper's grandparents were Jewish, but the Popper family converted to Lutheranism before Karl was born, and so he received Lutheran baptism. They understood this as part of their cultural assimilation, not as an expression of devout belief. Karl's father Simon Siegmund Carl Popper was a lawyer from Bohemia and a doctor of law at the Vienna University, and mother Jenny Schiff was of Silesian and Hungarian descent. After establishing themselves in Vienna, the Poppers made a rapid social climb in Viennese society: Simon Siegmund Carl became a legal partner of Vienna's liberal mayor Raimond Grübl and, after his death in 1898, took over the firm (Karl received his middle name from the mayor). Popper inherited both the library and the disposition from him.

Popper left school at the age of 16 and attended lectures in math, physics, philosophy, psychology and the history of music as a guest student at the University of Vienna. In 1919, Popper became attracted by Marxism and subsequently joined the Association of Socialist School Students. He also became a member of the Social Democratic Workers' Party of Austria, which was at that time a party that fully adopted the Marxist ideology. After the June 15, 1919 street battle in the Hörlgasse, when police shot eight of his unarmed party comrades, he became disillusioned by what he saw to be the pseudo-scientific historical materialism of Marx, abandoned the ideology, and remained a supporter of social liberalism throughout his life.

He worked in street construction for a short amount of time, but was unable to cope with the heavy labour. Continuing to attend university as a guest student, he started an apprenticeship as cabinetmaker, which he completed as a journeyman. He was dreaming at that time of starting a daycare facility for children, for which he assumed the ability to make furniture might be useful. After that he did voluntary service in one of psychoanalyst Alfred Adler's clinics for children. In 1922, he did his matura by way of a second chance education and finally joined the University as an ordinary student. He completed his examination as an elementary teacher in 1924 and started working at an after-school care club for socially endangered children.

### Notable ideas

- Critical rationalism
- Falsificationism
- Universal evolutionary trial and error model of life and knowledge
- Propensity interpretation
- Open society
- Cosmological pluralism
- Modified essentialism
- Axiomatization of probability
- Active Darwinism
- Spearhead model
- Truthlikeness
- Objective Hermeneutics

---

**Karl Popper**

Sir Karl Raimund Popper, CH FRS (28 July 1902 – 17 September 1994) was an Austro-British philosopher and professor at the London School of Economics. He is generally regarded as one of the greatest philosophers of science of the 20th century. He also wrote extensively on social and political philosophy. In 1992 he was awarded the Kyoto Prize in Arts and Philosophy for "symbolizing the open spirit of the 20th century" and for his "enormous influence on the formation of the modern intellectual climate".

Popper is known for his attempt to repudiate the classical observationalist/inductivist form of scientific method in favour of empirical falsification. He is also known for his opposition to the classical justificationist account of knowledge which he replaced with critical rationalism, "the first non justificational philosophy of criticism in the history of philosophy." In political discourse, he is known for his vigorous defence of liberal democracy and the principles of social criticism that he came to believe made a flourishing "open society" possible.

### Personal life

Karl Popper was born in Vienna (then in Austria-Hungary) in 1902, to upper middle-class parents. All of Karl Popper's grandparents were Jewish, but the Popper family converted to Lutheranism before Karl was born, and so he received Lutheran baptism. They understood this as part of their cultural assimilation, not as an expression of devout belief. Karl's father Simon Siegmund Carl Popper was a lawyer from Bohemia and a doctor of law at the Vienna University, and mother Jenny Schiff was of Silesian and Hungarian descent. After establishing themselves in Vienna, the Poppers made a rapid social climb in Viennese society: Simon Siegmund Carl became a legal partner of Vienna's liberal mayor Raimond Grübl and, after his death in 1898, took over the firm (Karl received his middle name from the mayor). Popper inherited both the library and the disposition from him.

Popper left school at the age of 16 and attended lectures in math, physics, philosophy, psychology and the history of music as a guest student at the University of Vienna. In 1919, Popper became attracted by Marxism and subsequently joined the Association of Socialist School Students. He also became a member of the Social Democratic Workers' Party of Austria, which was at that time a party that fully adopted the Marxist ideology. After the June 15, 1919 street battle in the Hörlgasse, when police shot eight of his unarmed party comrades, he became disillusioned by what he saw to be the pseudo-scientific historical materialism of Marx, abandoned the ideology, and remained a supporter of social liberalism throughout his life.

He worked in street construction for a short amount of time, but was unable to cope with the heavy labour. Continuing to attend university as a guest student, he started an apprenticeship as cabinetmaker, which he completed as a journeyman. He was dreaming at that time of starting a daycare facility for children, for which he assumed the ability to make furniture might be useful. After that he did voluntary service in one of psychoanalyst Alfred Adler's clinics for children. In 1922, he did his matura by way of a second chance education and finally joined the University as an ordinary student. He completed his examination as an elementary teacher in 1924 and started working at an after-school care club for socially endangered children. In 1925 he went to the newly founded Pädagogisches Institut.
and continued studying philosophy and psychology. Around that time he started dating Josephine Anna Henninger, later to become his wife.

In 1928, he earned a doctorate in psychology, under the supervision of Karl Bühler. His dissertation was entitled "Die Methodenfrage der Denkpsychologie" (The question of method in cognitive psychology).[15] In 1929 he obtained the authorization to teach math and physics in secondary school, which he started doing. He married in 1930. Fearing the rise of Nazism and the threat of the Anschluss, he started to use the evenings and the nights to write his first book Die beiden Grundprobleme der Erkenntnistheorie. He needed to publish one in order to get some academic position in a country that was safe for people of Jewish descent. However, he ended up not publishing the two-volume work, but a condensed version of it with some new material, Logik der Forschung (The Logic of Scientific Discovery), in 1934. Here, he criticised psychologism, naturalism, inductionism, and logical positivism, and put forth his theory of potential falsifiability as the criterion demarcating science from non-science. In 1935 and 1936, he took unpaid leave to go to England for a study visit.

In 1937, Popper finally managed to get a position that allowed him to emigrate to New Zealand, where he became lecturer in philosophy at Canterbury University College of the New Zealand (at Christchurch). It was here that he wrote his influential work The Open Society and its Enemies. In 1946, after the Second World War, he moved to England to become reader in logic and scientific method at the London School of Economics. Three years later, he was appointed as professor of logic and scientific method at the University of London in 1949. Popper was president of the Aristotelian Society from 1958 to 1959. He retired from academic life in 1969, though he remained intellectually active for the rest of his life. In 1985, he returned to Austria to let his wife have her relatives around her during the last months of her life. She died in November that year. After the Ludwig Boltzmann Gesellschaft had failed to establish him as the director of a newly founded branch researching the philosophy of science, he went back again to the United Kingdom in 1986, settling in Kenley, Surrey.[11]

Popper died in Croydon at the age of 92 on 17 September 1994. He had been working continuously on his philosophy until two weeks before, when he suddenly fell terminally ill.[16] After cremation, his ashes were taken to Vienna and buried at Lainzer cemetery adjacent to the ORF Centre, where his wife Josefine Anna Henninger had already been buried.[17] Popper's estate is managed by his secretary and personal assistant Melitta Mew and her husband Raymond. Popper's manuscripts went to the Hoover institution, partly during his lifetime and partly as supplementary material after his death. Klagenfurt University possesses Popper's library including his precious bibliophilia, as well as hard copies of the original Hoover material and microfilms of the supplementary material. The remaining parts of the estate were mostly transferred to The Karl Popper Charitable Trust.[18] In October 2008, Klagenfurt University acquired the copyrights from the estate.

Together with his wife, Popper chose not to have children, because of the circumstances of war in the early years of their marriage. Popper commented that this "was perhaps a cowardly but in a way a right decision".[19]

Popper won many awards and honours in his field, including the Lippincott Award of the American Political Science Association, the Sonning Prize, the Otto Hahn Peace Medal of the United Nations Association of Germany in Berlin and fellowships in the Royal Society,[11] British Academy, London School of Economics, King's College London, Darwin College, Cambridge, and Charles University, Prague. Austria awarded him the Grand Decoration for Services to the Republic of Austria in Gold. He received the Humanist Laureate Award from the International
Academy of Humanism. He was knighted by Queen Elizabeth II in 1965, and was elected a Fellow of the Royal Society in 1976. He was invested with the Insignia of a Companion of Honour in 1982.

Philosophy

Philosophy of science

Falsifiability/problem of demarcation

Popper coined the term "critical rationalism" to describe his philosophy. Concerning the method of science, the term indicates his rejection of classical empiricism, and the classical observationalist-inductivist account of science that had grown out of it. Popper argued strongly against the latter, holding that scientific theories are abstract in nature, and can be tested only indirectly, by reference to their implications. He also held that scientific theory, and human knowledge generally, is irreducibly conjectural or hypothetical, and is generated by the creative imagination in order to solve problems that have arisen in specific historio-cultural settings.

Logically, no number of positive outcomes at the level of experimental testing can confirm a scientific theory, but a single counterexample is logically decisive: it shows the theory, from which the implication is derived, to be false. The term "falsifiable" does not mean something is made false, but rather that, if it is false, it can be shown by observation or experiment. Popper's account of the logical asymmetry between verification and falsifiability lies at the heart of his philosophy of science. It also inspired him to take falsifiability as his criterion of demarcation between what is, and is not, genuinely scientific: a theory should be considered scientific if, and only if, it is falsifiable. This led him to attack the claims of both psychoanalysis and contemporary Marxism to scientific status, on the basis that their theories are not falsifiable.

Popper also wrote extensively against the famous Copenhagen interpretation of quantum mechanics. He strongly disagreed with Niels Bohr's instrumentalism and supported Albert Einstein's realist approach to scientific theories about the universe. Popper's falsifiability resembles Charles Peirce's nineteenth century fallibilism. In Of Clocks and Clouds (1966), Popper remarked that he wished he had known of Peirce's work earlier.

In All Life is Problem Solving, Popper sought to explain the apparent progress of scientific knowledge — that is, how it is that our understanding of the universe seems to improve over time. This problem arises from his position that the truth content of our theories, even the best of them, cannot be verified by scientific testing, but can only be falsified. Again, in this context the word 'falsified' does not refer to something being 'fake'; rather, that something can be (i.e., is capable of being) shown to be false by observation or experiment. Some things simply do not lend themselves to being shown to be false, and therefore, are not falsifiable. If so, then how is it that the growth of science appears to result in a growth in knowledge? In Popper's view, the advance of scientific knowledge is an evolutionary process characterized by his formula:

\[ PS_1 \rightarrow TT_1 \rightarrow EE_1 \rightarrow PS_2. \]

In response to a given problem situation (PS₁), a number of competing conjectures, or tentative theories (TT), are systematically subjected to the most rigorous attempts at falsification possible. This process, error elimination (EE), performs a similar function for science that natural selection performs for biological evolution. Theories that better survive the process of refutation are not more true, but rather, more "fit"—in other words, more applicable to the problem situation at hand (PS₁). Consequently, just as a species' biological fitness does not ensure continued survival, neither does rigorous testing protect a scientific theory from refutation in the future. Yet, as it appears that the engine of biological evolution has produced, over time, adaptive traits equipped to deal with more and more complex problems of survival, likewise, the evolution of theories through the scientific method may, in Popper's view, reflect a certain type of progress: toward more and more interesting problems (PS₂). For Popper, it is in the interplay between the tentative theories (conjectures) and error elimination (refutation) that scientific knowledge advances toward greater and greater problems; in a process very much akin to the interplay between genetic variation
and natural selection.

**Falsification/problem of induction**

Among his contributions to philosophy is his claim to solve the philosophical problem of induction. He states that while there is no way to prove that the sun will rise, it is possible to formulate the theory that every day the sun will rise; if it does not rise on some particular day, the theory will be falsified and will have to be replaced by a different one. Until that day, there is no need to reject the assumption that the theory is true. Neither is it rational according to Popper to instead make the more complex assumption that the sun will rise until a given day, but will stop doing so the day after, or similar statements with additional conditions.

Such a theory would be true with higher probability, because it cannot be attacked so easily: to falsify the first one, it is sufficient to find that sun has stopped rising; to falsify the second one, one additionally needs the assumption that the given day has not yet been reached. Popper held that it is the least likely, or most easily falsifiable, or simplest theory (attributes which he identified as all the same thing) that explains known facts that one should rationally prefer. His opposition to positivism, which held that it is the theory most likely to be true that one should prefer, here becomes very apparent. It is impossible, Popper argues, to ensure a theory to be true; it is more important that their falsity can be detected as easily as possible.

Popper and David Hume agreed that there is often a psychological belief that the sun will rise tomorrow, but both denied that there is logical justification for the supposition that it will, simply because it always has in the past. Popper writes, "I approached the problem of induction through Hume. Hume, I felt, was perfectly right in pointing out that induction cannot be logically justified." (Conjectures and Refutations, p. 55)

**Rationality**

Popper held that rationality is not restricted to the realm of empirical or scientific theories, but that it is merely a special case of the general method of criticism, the method of finding and eliminating contradictions in knowledge without ad-hoc-measures. According to this view, rational discussion about metaphysical ideas, about moral and even about purposes are possible. Popper's student Bartley tried to radicalize this idea and made the controversial claim that not only can criticism go beyond empirical knowledge, but that everything can be rationally criticized.

To Popper, who was an anti-justificationist, traditional philosophy is misled by the false principle of sufficient reason. He thinks that no assumption can ever be or needs ever to be justified, so a lack of justification is not a justification for doubt. Instead, theories should be tested and scrutinized. It is not the goal to bless theories with claims of certainty or justification, but to eliminate errors in them. He writes, "there are no such things as good positive reasons; nor do we need such things [...] But [philosophers] obviously cannot quite bring [themselves] to believe that this is my opinion, let alone that it is right" (The Philosophy of Karl Popper, p. 1043)

**Philosophy of arithmetic**

Popper's principle of falsifiability runs into *prima facie* difficulties when the epistemological status of mathematics is considered. It is difficult to conceive how simple statements of arithmetic, such as "2 + 2 = 4", could ever be shown to be false. If they are not open to falsification they can not be scientific. If they are not scientific, it needs to be explained how they can be informative about real world objects and events.

Popper's solution[^21] was an original contribution in the philosophy of mathematics. His idea was that a number statement such as "2 apples + 2 apples = 4 apples" can be taken in two senses. In one sense it is irrefutable and logically true, in the second sense it is factually true and falsifiable. Concisely, the pure mathematics "2 + 2 = 4" is always true, but, when the formula is applied to real world apples, it is open to falsification.[^22]
Political philosophy

In *The Open Society and Its Enemies* and *The Poverty of Historicism*, Popper developed a critique of historicism and a defence of the 'Open Society'. Popper considered historicism to be the theory that history develops inexorably and necessarily according to knowable general laws towards a determinate end. He argued that this view is the principal theoretical presupposition underpinning most forms of authoritarianism and totalitarianism. He argued that historicism is founded upon mistaken assumptions regarding the nature of scientific law and prediction. Since the growth of human knowledge is a causal factor in the evolution of human history, and since "no society can predict, scientifically, its own future states of knowledge", it follows, he argued, that there can be no predictive science of human history. For Popper, metaphysical and historical indeterminism go hand in hand. In *After The Open Society*, which was published posthumously, a large collection of his previously unpublished and uncollected essays on social and political topics was assembled. In this, one can trace his ideas from material that pre-dated *The Open Society* to something that was completed just as he died.

In a 1992 lecture, Popper explained the connection between his political philosophy and his philosophy of science. As he stated, he was in his early years impressed by communism and also active in the Austrian Communist party. What had a profound effect on him was an event that happened in 1919: during a riot, caused by the Communists, the police shot several people, including some of Popper's friends. When Popper later told the leaders of the Communist party about this, they responded by stating that this loss of life was necessary in working towards the inevitable workers' revolution. This statement did not convince Popper and he started to think about what kind of reasoning would justify such a statement. He later concluded that there could not be any justification for it, and this was the start of his later criticism of historicism.

In 1947, Popper founded with Friedrich Hayek, Milton Friedman, Ludwig von Mises and others the Mont Pelerin Society to defend classical liberalism, in the spirit of the Open Society.

The paradox of tolerance

Although Popper was an advocate of toleration, he said that intolerance should not be tolerated, for if tolerance allowed intolerance to succeed completely, tolerance itself would be threatened. In *The Open Society and Its Enemies*, he argued:

> Unlimited tolerance must lead to the disappearance of tolerance. If we extend unlimited tolerance even to those who are intolerant, if we are not prepared to defend a tolerant society against the onslaught of the intolerant, then the tolerant will be destroyed, and tolerance with them.

The utterance of intolerant philosophies should not always be suppressed, "as long as we can counter them by rational argument and keep them in check by public opinion." However:

> we should claim the right to suppress them if necessary even by force; for it may easily turn out that they are not prepared to meet us on the level of rational argument, but begin by denouncing all argument; they may forbid their followers to listen to rational argument, because it is deceptive, and teach them to answer arguments by the use of their fists or pistols.

Furthermore, in support of human rights legislation in the second half of the 20th century, he stated:

> We should therefore claim, in the name of tolerance, the right not to tolerate the intolerant. We should claim that any movement preaching intolerance places itself outside the law, and we should consider incitement to intolerance and persecution as criminal, in the same way as we should consider incitement to murder, or to kidnapping, or to the revival of the slave trade, as criminal."}^{23}
Metaphysics

Truth
As early as 1934, Popper wrote of the search for truth as "one of the strongest motives for scientific discovery." Still, he describes in *Objective Knowledge* (1972) early concerns about the much-criticized notion of truth as correspondence. Then came the semantic theory of truth formulated by the logician Alfred Tarski and published in 1933. Popper writes of learning in 1935 of the consequences of Tarski's theory, to his intense joy. The theory met critical objections to truth as correspondence and thereby rehabilitated it. The theory also seemed, in Popper's eyes, to support metaphysical realism and the regulative idea of a search for truth.

According to this theory, the conditions for the truth of a sentence as well as the sentences themselves are part of a metalanguage. So, for example, the sentence "Snow is white" is true if and only if snow is white. Although many philosophers have interpreted, and continue to interpret, Tarski's theory as a deflationary theory, Popper refers to it as a theory in which "is true" is replaced with "corresponds to the facts". He bases this interpretation on the fact that examples such as the one described above refer to two things: assertions and the facts to which they refer. He identifies Tarski's formulation of the truth conditions of sentences as the introduction of a "metalinguistic predicate" and distinguishes the following cases:
1. "John called" is true.
2. "It is true that John called."

The first case belongs to the metalanguage whereas the second is more likely to belong to the object language. Hence, "it is true that" possesses the logical status of a redundancy. "Is true", on the other hand, is a predicate necessary for making general observations such as "John was telling the truth about Phillip."

Upon this basis, along with that of the logical content of assertions (where logical content is inversely proportional to probability), Popper went on to develop his important notion of verisimilitude or "truthlikeness". The intuitive idea behind verisimilitude is that the assertions or hypotheses of scientific theories can be objectively measured with respect to the amount of truth and falsity that they imply. And, in this way, one theory can be evaluated as more or less true than another on a quantitative basis which, Popper emphasizes forcefully, has nothing to do with "subjective probabilities" or other merely "epistemic" considerations.

The simplest mathematical formulation that Popper gives of this concept can be found in the tenth chapter of *Conjectures and Refutations*. Here he defines it as:

\[ V_s(a) = CT_w(a) - CT_f(a) \]

where \( V_s(a) \) is the verisimilitude of \( a \), \( CT_w(a) \) is a measure of the content of truth of \( a \), and \( CT_f(a) \) is a measure of the content of the falsity of \( a \).

Popper's original attempt to define not just verisimilitude, but an actual measure on it, turned out to be inadequate. However, it inspired a wealth of new attempts.\[24\]

Cosmological pluralism
Knowledge, for Popper, was objective, both in the sense that it is objectively true (or truthlike), and also in the sense that knowledge has an ontological status (i.e., knowledge as object) independent of the knowing subject (*Objective Knowledge: An Evolutionary Approach*, 1972). He proposed three worlds \[25\]. World One, being the physical world, or physical states; World Two, being the world of mind, or mental states, ideas, and perceptions; and World Three, being the body of human knowledge expressed in its manifold forms, or the products of the second world made manifest in the materials of the first world (i.e., books, papers, paintings, symphonies, and all the products of the human mind). World Three, he argued, was the product of individual human beings in exactly the same sense that an animal path is the product of individual animals, and that, as such, has an existence and evolution independent of any individual knowing subjects. The influence of World Three, in his view, on the individual human mind (World Two) is at least as strong as the influence of World One. In other words, the knowledge held by a given individual mind
owes at least as much to the total accumulated wealth of human knowledge, made manifest, as to the world of direct experience. As such, the growth of human knowledge could be said to be a function of the independent evolution of World Three. Many contemporary philosophers have not embraced Popper's Three World conjecture, due mostly, it seems, to its resemblance to Cartesian dualism.

Origin and evolution of life

The creation–evolution controversy in the United States raises the issue of whether creationistic ideas may be legitimately called science and whether evolution itself may be legitimately called science. In the debate, both sides and even courts in their decisions have frequently invoked Popper's criterion of falsifiability. In this context, passages written by Popper are frequently quoted in which he speaks about such issues himself. For example, he famously stated "Darwinism is not a testable scientific theory, but a metaphysical research program — a possible framework for testable scientific theories." He continued:

And yet, the theory is invaluable. I do not see how, without it, our knowledge could have grown as it has done since Darwin. In trying to explain experiments with bacteria which become adapted to, say, penicillin, it is quite clear that we are greatly helped by the theory of natural selection. Although it is metaphysical, it sheds much light upon very concrete and very practical researches. It allows us to study adaptation to a new environment (such as a penicillin-infested environment) in a rational way: it suggests the existence of a mechanism of adaptation, and it allows us even to study in detail the mechanism at work.\[26]\[26\]

He also noted that theism, presented as explaining adaptation, "was worse than an open admission of failure, for it created the impression that an ultimate explanation had been reached."\[27]\[27\]

Popper later said:

When speaking here of Darwinism, I shall speak always of today's theory — that is Darwin's own theory of natural selection supported by the Mendelian theory of heredity, by the theory of the mutation and recombination of genes in a gene pool, and by the decoded genetic code. This is an immensely impressive and powerful theory. The claim that it completely explains evolution is of course a bold claim, and very far from being established. All scientific theories are conjectures, even those that have successfully passed many severe and varied tests. The Mendelian underpinning of modern Darwinism has been well tested, and so has the theory of evolution which says that all terrestrial life has evolved from a few primitive unicellular organisms, possibly even from one single organism.\[27]\[27\]

He explained that the difficulty of testing had led some people to describe natural selection as a tautology, and that he too had in the past described the theory as 'almost tautological', and had tried to explain how the theory could be untestable (as is a tautology) and yet of great scientific interest:

My solution was that the doctrine of natural selection is a most successful metaphysical research programme. It raises detailed problems in many fields, and it tells us what we would expect of an acceptable solution of these problems. I still believe that natural selection works in this way as a research programme. Nevertheless, I have changed my mind about the testability and logical status of the theory of natural selection; and I am glad to have an opportunity to make a recantation.\[27]\[27\]

Popper summarized his new view as follows:

The theory of natural selection may be so formulated that it is far from tautological. In this case it is not only testable, but it turns out to be not strictly universally true. There seem to be exceptions, as with so many biological theories; and considering the random character of the variations on which natural selection operates, the occurrence of exceptions is not surprising. Thus not all phenomena of evolution are explained by natural selection alone. Yet in every particular case it is a challenging research program to show how far natural selection can possibly be held responsible for the evolution of a particular organ or behavioral program.\[28]\[28\]
These frequently quoted passages are only a very small part of what Popper wrote on the issue of evolution, however, and give the wrong impression that he mainly discussed questions of its falsifiability. Popper never invented this criterion to give justifiable use of words like science. In fact, Popper says at the beginning of Logic of Scientific Discovery that it is not his aim to define science, and that science can in fact be defined quite arbitrarily.

Popper had his own sophisticated views on evolution that go much beyond what the frequently-quoted passages say. In effect, Popper agreed with some of the points of both creationists and naturalists, but also disagreed with both views on crucial aspects. Popper understood the universe as a creative entity that invents new things, including life, but without the necessity of something like a god, especially not one who is pulling strings from behind the curtain. He said that evolution must, as the creationists say, work in a goal-directed way but disagreed with their view that it must necessarily be the hand of god that imposes these goals onto the stage of life.

Instead, he formulated the spearhead model of evolution, a version of genetic pluralism. According to this model, living organisms themselves have goals, and act according to these goals, each guided by a central control. In its most sophisticated form, this is the brain of humans, but controls also exist in much less sophisticated ways for species of lower complexity, such as the amoeba. This control organ plays a special role in evolution — it is the "spearhead of evolution". The goals bring the purpose into the world. Mutations in the genes that determine the structure of the control may then cause drastic changes in behaviour, preferences and goals, without having an impact on the organism's phenotype. Popper postulates that such purely behavioral changes are less likely to be lethal for the organism compared to drastic changes of the phenotype.

Popper contrasts his views with the notion of the "hopeful monster" that has large phenotype mutations and calls it the "hopeful behavioral monster". After behavior has changed radically, small but quick changes of the phenotype follow to make the organism fitter to its changed goals. This way it looks as if the phenotype were changing guided by some invisible hand, while it is merely natural selection working in combination with the new behaviour. For example, according to this hypothesis, the eating habits of the giraffe must have changed before its elongated neck evolved. Popper contrasted this view as evolution from within or active darwisim (the organism actively trying to discover new ways of life and being on a quest for conquering new ecological niches), with the naturalistic evolution from without (which has the picture of a hostile environment only trying to kill the mostly passive organism, or perhaps segregate some of its groups).

Popper was a key figure encouraging patent lawyer Günter Wächtershäuser to publish his Iron–sulfur world theory on abiogenesis and his criticism of "soup" theory.

Free will

Popper and John Eccles speculated on the problem of free will for many years generally agreeing on an interactionist dualist theory of mind. However, although Popper was a body-mind dualist, he did not think that the mind is a substance separate from the body: he thought that mental or psychological properties or aspects of people are distinct from physical ones.

When he gave the second Arthur Holly Compton Memorial Lecture in 1965, Popper revisited the idea of quantum indeterminacy as a source of human freedom. Eccles had suggested that "critically poised neurons" might be influenced by the mind to assist in a decision. Popper criticized Compton's idea of amplified quantum events affecting the decision. He wrote:

The idea that the only alternative to determinism is just sheer chance was taken over by Schlick, together with many of his views on the subject, from Hume, who asserted that 'the removal' of what he called 'physical necessity' must always result in 'the same thing with chance. As objects must either be join'd or not,... 'tis impossible to admit of any medium betwixt chance and an absolute necessity'.

I shall later argue against this important doctrine according to which the alternative to determinism is sheer chance. Yet I must admit that the doctrine seems to hold good for the quantum-theoretical models which have been designed to explain, or at least to illustrate, the possibility of human freedom. This seems to be the reason
why these models are so very unsatisfactory.\[32\]

Hume's and Schlick's ontological thesis that there cannot exist anything intermediate between chance and determinism seems to me not only highly dogmatic (not to say doctrinaire) but clearly absurd; and it is understandable only on the assumption that they believed in a complete determinism in which chance has no status except as a symptom of our ignorance.\[33\]

Popper called not for something between chance and necessity but for a combination of randomness and control to explain freedom, though not yet explicitly in two stages with random chance before the controlled decision, saying, "freedom is not just chance but, rather, the result of a subtle interplay between something almost random or haphazard, and something like a restrictive or selective control."\[34\]

Then in his 1977 book with John Eccles, *The Self and its Brain*, Popper finally formulates the two-stage model in a temporal sequence. And he compares free will to Darwinian evolution and natural selection:

New ideas have a striking similarity to genetic mutations. Now, let us look for a moment at genetic mutations. Mutations are, it seems, brought about by quantum theoretical indeterminacy (including radiation effects). Accordingly, they are also probabilistic and not in themselves originally selected or adequate, but on them there subsequently operates natural selection which eliminates inappropriate mutations. Now we could conceive of a similar process with respect to new ideas and to free-will decisions, and similar things.

That is to say, a range of possibilities is brought about by a probabilistic and quantum mechanically characterized set of proposals, as it were — of possibilities brought forward by the brain. On these there then operates a kind of selective procedure which eliminates those proposals and those possibilities which are not acceptable to the mind.\[35\]

Other thinkers who have formulated a two-stage model for free will include William James, Henri Poincaré, Arthur Compton, Henry Margenau, and Daniel Dennett.

**Religion and God**

In an interview\[19\] that Popper gave in 1969 with the condition that it shall be kept secret until after his death, he summarized his position on God as follows: "I don't know whether God exists or not. ... Some forms of atheism are arrogant and ignorant and should be rejected, but agnosticism—to admit that we don't know and to search—is all right. ... When I look at what I call the gift of life, I feel a gratitude which is in tune with some religious ideas of God. However, the moment I even speak of it, I am embarrassed that I may do something wrong to God in talking about God." He objected to organized religion, saying "it tends to use the name of God in vain", noting the danger of fanaticism because of religious conflicts: "The whole thing goes back to myths which, though they may have a kernel of truth, are untrue. Why then should the Jewish myth be true and the Indian and Egyptian myths not be true?"

In a letter unrelated to the interview, he stressed his tolerant attitude: "Although I am not for religion, I do think that we should show respect for anybody who believes honestly."\[1]\[36]\[37\]
Influence

Popper played a vital role in establishing the philosophy of science as a vigorous, autonomous discipline within analytic philosophy, through his own prolific and influential works, and also through his influence on his own contemporaries and students. Popper founded in 1946 the Department of Philosophy, Logic and Scientific Method at the London School of Economics and there lectured and influenced both Imre Lakatos and Paul Feyerabend, two of the foremost philosophers of science in the next generation of philosophy of science. (Lakatos significantly modified Popper's position, and Feyerabend repudiated it entirely, but the work of both is deeply influenced by Popper and engaged with many of the problems that Popper set.)

While there is some dispute as to the matter of influence, Popper had a long-standing and close friendship with economist Friedrich Hayek, who was also brought to the London School of Economics from Vienna. Each found support and similarities in each other's work, citing each other often, though not without qualification. In a letter to Hayek in 1944, Popper stated, "I think I have learnt more from you than from any other living thinker, except perhaps Alfred Tarski."[38] Popper dedicated his Conjectures and Refutations to Hayek. For his part, Hayek dedicated a collection of papers, Studies in Philosophy, Politics, and Economics, to Popper, and in 1982 said, "...ever since his Logik der Forschung first came out in 1934, I have been a complete adherent to his general theory of methodology."[39]

Popper also had long and mutually influential friendships with art historian Ernst Gombrich, biologist Peter Medawar, and neuro-scientist John Carew Eccles.

Popper's influence, both through his work in philosophy of science and through his political philosophy, has also extended beyond the academy. One of Popper's students at the London School of Economics was the billionaire investor George Soros, among whose philanthropic foundations is the Open Society Institute, a think-tank named in honor of Popper's The Open Society and Its Enemies.

Popperian philosophy also inspired the creation of Taking Children Seriously, a libertarian movement which noticed that Popper's general theory of knowledge creation does not differentiate between adults and children.

Criticism

Philosophy of science

Most criticisms of Popper's philosophy are of the falsification, or error elimination, element in his account of problem solving. It is intended as an ideal, practical method of effective human problem solving; as such, the current conclusions of science are stronger than pseudo-sciences or non-sciences, insofar as they have survived this particularly vigorous selection method. He does not argue that any such conclusions are therefore true, or that this describes the actual methods of any particular scientist.

Rather, it is a recommended ideal method that, if enacted by a system or community, will over time lead to slow but steady progress of a sort (relative to how well the system or community enacts the method). It has been suggested that Popper's ideas are often mistaken for a hard logical account of truth because of the historical co-incidence of
their appearing at the same time as logical positivism, the followers of which mistook his aims for their own.[40]

The Quine-Duhem thesis argues that it's impossible to test a single hypothesis on its own, since each one comes as part of an environment of theories. Thus we can only say that the whole package of relevant theories has been collectively falsified, but cannot conclusively say which element of the package must be replaced. An example of this is given by the discovery of the planet Neptune: when the motion of Uranus was found not to match the predictions of Newton's laws, the theory "There are seven planets in the solar system" was rejected, and not Newton's laws themselves. Popper discussed this critique of naïve falsificationism in Chapters 3 & 4 of *The Logic of Scientific Discovery*. For Popper, theories are accepted or rejected via a sort of selection process. Theories that say more about the way things appear are to be preferred over those that do not; the more generally applicable a theory is, the greater its value. Thus Newton's laws, with their wide general application, are to be preferred over the much more specific "the solar system has seven planets".

Thomas Kuhn's influential book *The Structure of Scientific Revolutions* argued that scientists work in a series of paradigms, and that falsificationist methodologies would make science impossible:

No theory ever solves all the puzzles with which it is confronted at a given time; nor are the solutions already achieved often perfect. On the contrary, it is just the incompleteness and imperfection of the existing data-theory fit that, at any given time, define many of the puzzles that characterize normal science. If any and every failure to fit were ground for theory rejection, all theories ought to be rejected at all times. On the other hand, if only severe failure to fit justifies theory rejection, then the Popperians will require some criterion of 'improbability' or of 'degree of falsification.' In developing one they will almost certainly encounter the same network of difficulties that has haunted the advocates of the various probabilistic verification theories [that the evaluative theory cannot itself be legitimated without appeal to another evaluative theory, leading to regress].[41]

Popper's student Imre Lakatos attempted to reconcile Kuhn's work with falsificationism by arguing that science progresses by the falsification of research programs rather than the more specific universal statements of naïve falsificationism. Another of Popper's students Paul Feyerabend ultimately rejected any prescriptive methodology, and argued that the only universal method characterizing scientific progress was anything goes.

Popper claimed to have recognized already in the 1934 version of his *Logic of Discovery* a fact later stressed by Kuhn, "that scientists necessarily develop their ideas within a definite theoretical framework", and to that extent to have anticipated Kuhn's central point about 'normal science'.[42] (But Popper criticised what he saw as Kuhn's relativism.[43]) Also, in his collection *Conjectures and Refutations: The Growth of Scientific Knowledge* (Harper & Row, 1963), Popper writes, "Science must begin with myths, and with the criticism of myths; neither with the collection of observations, nor with the invention of experiments, but with the critical discussion of myths, and of magical techniques and practices. The scientific tradition is distinguished from the pre-scientific tradition in having two layers. Like the latter, it passes on its theories; but it also passes on a critical attitude towards them. The theories are passed on, not as dogmas, but rather with the challenge to discuss them and improve upon them."

Another objection is that it is not always possible to demonstrate falsehood definitively, especially if one is using statistical criteria to evaluate a null hypothesis. More generally it is not always clear, if evidence contradicts a hypothesis, that this is a sign of flaws in the hypothesis rather than of flaws in the evidence. However, this is a misunderstanding of what Popper's philosophy of science sets out to do. Rather than offering a set of instructions that merely need to be followed diligently to achieve science, Popper makes it clear in *The Logic of Scientific Discovery* that his belief is that the resolution of conflicts between hypotheses and observations can only be a matter of the collective judgment of scientists, in each individual case.[44]

Popper's falsificationism can be questioned logically: it is not clear how Popper would deal with a statement like "for every metal, there is a temperature at which it will melt." The hypothesis cannot be falsified by any possible observation, for there will always be a higher temperature than tested at which the metal may in fact melt, yet it seems to be a valid scientific hypothesis. These examples were pointed out by Carl Gustav Hempel. Hempel came to
Karl Popper

acknowledge that Logical Positivism's verificationism was untenable, but argued that falsificationism was equally untenable on logical grounds alone. The simplest response to this is that, because Popper describes how theories attain, maintain and lose scientific status, individual consequences of currently accepted scientific theories are scientific in the sense of being part of tentative scientific knowledge, and both of Hempel's examples fall under this category. For instance, atomic theory implies that all metals melt at some temperature.

An early adversary of so-called critical rationalism, Karl-Otto Apel attempted a comprehensive refutation of Popper's philosophy. In *Transformation der Philosophie* (1973), Apel charged Popper with being guilty of, amongst other things, a pragmatic contradiction.[45]

Other criticism

It has been argued that Popper's student Imre Lakatos transformed Popper's philosophy using historicism and updated Hegelian historiographic ideas.[46][47]

Ludwig Wittgenstein was accused of brandishing a poker at Popper during a meeting of the Cambridge Moral Sciences Club, when they argued about whether issues in philosophy were real or just linguistic puzzles. Wittgenstein's friends say he was merely handling a poker, but Popper used the situation to make a joke at Wittgenstein's expense.[48][49]

Charles Taylor accuses Popper of exploiting his worldwide fame as an epistemologist to diminish the importance of philosophers of the 20th century continental tradition. According to Taylor, Popper's criticisms are completely baseless, but they are received with an attention and respect that Popper's "intrinsic worth hardly merits".[50] William W. Bartley defended Popper against such allegations: "Sir Karl Popper is not really a participant in the contemporary professional philosophical dialogue; quite the contrary, he has ruined that dialogue. If he is on the right track, then the majority of professional philosophers the world over has wasted or is wasting their intellectual careers. The gulf between Popper's way of doing philosophy and that of the bulk of professional philosophers is as great as that between astronomy and astrology."[51]

In 2004, philosopher and psychologist Michel ter Hark (Groningen, The Netherlands) published a book, called *Popper, Otto Selz and the rise of evolutionary epistemology*, ISBN 0-521-83074-5, in which he claimed that Popper took some of his ideas from his tutor, the German psychologist Otto Selz. Selz himself never published his ideas, partly because of the rise of Nazism which forced him to quit his work in 1933, and the prohibition of referring to Selz' work. Popper, the historian of ideas and his scholarship, is criticized in some academic quarters, for his rejection of Plato, Hegel and Marx.[52]

According to John N. Gray, Popper held that "a theory is scientific only in so far as it is falsifiable, and should be given up as soon as it is falsified."[53] By applying Popper's account of scientific method, Gray's *Straw Dogs* states that this would have "killed the theories of Darwin and Einstein at birth." When they were first advanced, Gray claims, each of them was "at odds with some available evidence; only later did evidence become available that gave them crucial support."[54] Against this, Gray seeks to establish the irrationalist thesis that "the progress of science comes from acting against reason."[55]

Gray does not, however, give any indication of what available evidence these theories were at odds with, and his appeal to "crucial support" illustrates the very inductivist approach to science that Popper sought to show was logically illegitimate. For, according to Popper, Einstein's theory was at least equally as well corroborated as Newton's upon its initial conception; they both equally well accounted for all the hitherto available evidence. Moreover, since Einstein also explained the empirical refutations of Newton's theory, general relativity was immediately deemed suitable for tentative acceptance on the Popperian account.[56] Indeed, Popper wrote, several decades before Gray's criticism, in reply to a critical essay by Imre Lakatos:

> It is true that I have used the terms "elimination", and even "rejection" when discussing "refutation". But it is clear from my main discussion that these terms mean, when applied to a scientific theory, that it is eliminated as a contender for the truth- that is, refuted, but not necessarily abandoned. Moreover, I have often pointed out
that any such refutation is fallible. It is a typical matter of conjecture and of risk-taking whether or not we accept a refutation and, furthermore, of whether we "abandon" a theory or, say, only modify it, or even stick to it, and try to find some alternative, and methodologically acceptable, way round the problem involved. That I do not conflate even admitted falsity with the need to abandon a theory may be seen from the fact that I have frequently pointed out, that Einstein regarded general relativity as false, yet as a better approximation to the truth than Newton's gravitational theory. He certainly did not "abandon" it. But he worked to the end of his life in an attempt to improve upon it by way of a further generalization.\[57\]

References

[17] Sir Karl Popper (http://www.findagrave.com/cgi-bin/fg.cgi?page=gr&GRid=66663) at Find a Grave
[26] Unended Quest ch. 37 – see Bibliography
[28] Karl Popper in Evolutionary epistemology, rationality, and the sociology of knowledge by Radnitzky, Bartley and Popper, pp. 144–145, accessible at (http://books.google.com/books?id=QnF1rCzgSoC&pg=PA145&dq=The+theory+of+natural+selection+may+so+be+formulated+and+that+it+is+f&lpg=L004&hl=en&ei=68ktZuQPcOdKx6LkB&sa=X&oi=book_result&ct=result&resnum=1&ved=0CCMQ6AEwAA#v=onepage&q=The+theory+of+natural+selection+may+so+be+formulated+that+it+is+f) is not only testable, but it turns out to be not strictly universally true. There seems to be exceptions&f=false)
[29] Objective Knowledge, section "The Hopeful Behavioral Monster"

In Defense of Hegel; ethicists have found contradictions in the ethical theory (‘critical dualism’) upon which his polemic is largely based.

Philosophers have deplored his radical denial of historical causation, together with his espousal of Hayek’s systematic distrust of larger—have objected to Popper’s conclusions in those very fields . . ." and "Social scientists and social here Lindsay is again to be included—With a few exceptions in Popper’s favor, however, it is noticeable that reviewers possessed of special competence in particular fields . . ."

Popper’s Hume quote is from Treatise on Human Understanding (1977).

Karl Popper, Replies to my Critics, p1009 Open Court, London, 1974

Karl Popper, Replies to my Critics, Open Court, London, 1974


Karl Popper, Replies to my Critics, Open Court, London, 1974

Karl Popper, Replies to my Critics, p1009 Open Court, London, 1974
Bibliography

• The Two Fundamental Problems of the Theory of Knowledge, 1930–33 (as a typescript circulating as Die beiden Grandprobleme der Erkenntnistheorie; as a German book 1979, as English translation 2008), ISBN 0-415-39431-7
• The Logic of Scientific Discovery, 1934 (as Logik der Forschung, English translation 1959), ISBN 0-415-27844-9
• Realism and the Aim of Science, 1956/57 (as privately circulated galley proofs; published as a book 1983), ISBN 0-09-151450-9
• Die Zukunft ist offen (The Future is Open) (with Konrad Lorenz), 1985 (in German), ISBN 3-492-00640-X
• A World of Propensities, 1990, ISBN 1-85506-000-0
• All life is Problem Solving, 1994, ISBN 0-415-24992-9
• After The Open Society, 2008. (Edited by Jeremy Shearmur and Piers Norris Turner, this volume contains a large number of Popper's previously unpublished or uncollected writings on political and social themes.) ISBN 978-0-415-30908-0
• Frühe Schriften, 2006 (Edited by Troels Eggers Hansen, includes Popper's writings and publications from before the Logic, including his previously unpublished thesis, dissertation and journal articles published that relate to the Wiener Schultreform) ISBN 978-3-16-147632-7
Further reading

- David Miller (Ed.). Popper Selections.
- Kuhn, Thomas S. The Structure of Scientific Revolutions. Chicago: University of Chicago Press, 1962. Central to contemporary philosophy of science is the debate between the followers of Kuhn and Popper on the nature of scientific enquiry. This is the book in which Kuhn's views received their classical statement.


External links

- The Karl Popper Web (http://www.eeng.dcu.ie/~tkpw/)
- Sir Karl Popper Society (http://www.poppersociety.net/) International Association for the Promotion of Science and Research, in German
- University of Canterbury (NZ) (http://www.phil.canterbury.ac.nz/haps/kiwihps.shtml#popper) brief biography of Popper
- Influence on Friesian Philosophy (http://www.friesian.com/popper.htm)
- Open Society Institute (http://www.soros.org/) George Soros foundations network
- Synopsis and background of The poverty of historicism (http://fac.comtech.depaul.edu/profpjm/Murphy JMH (2009).pdf)
- "A Skeptical Look at Karl Popper" (http://www.stephenjaygould.org/ctrl/gardner_popper.html) by Martin Gardner
- Sir Karl Popper: Science: Conjectures and Refutations (http://cla.calpoly.edu/~fotool/e321.1/popper.html)
- Information on Lakatos/Popper (http://www.johnkadvany.com/GettingStarted/Kadvany_Design/Assets/LakatosPage/Lakatos_Frameset_3.htm) Site maintained by John Kadvany, PhD.
- Discovering Karl Popper (http://www.utilitarian.net/singer/by/19740502.htm) by Peter Singer The New York Review of Books, vol. 21, no. 7 (2 May 1974)
- The Liberalism of Karl Popper (http://www.libertarian.co.uk/lapubs/philn/philn009.pdf) by John N. Gray
- Karl Popper on Information Philosopher (http://www.informationphilosopher.com/solutions/philosophers/popper)
- Karl Popper (II Diogene) (it) (http://www.ildiogene.it/EncyPages/Ency=Popper.html)
- Popper (http://www.bbc.co.uk/programmes/b00773y4) on In Our Time at the BBC. ( listen now (http://www.bbc.co.uk/iplayer/console/b00773y4/In_Our_Time_Popper))

, 8 February 2007. Discussion with John Worrall, Professor of Philosophy of Science at the London School of Economics, Anthony O'Hear, Weston Professor of Philosophy at Buckingham University, Nancy Cartwright, Professor of Philosophy at the LSE and the University of California, hosted by Melvyn Bragg.

- History of Twentieth-Century Philosophy of Science, BOOK V: Karl Popper (http://www.philssci.com/) Site offers free downloads by chapter available for public use.
- Karl Popper Archive at LSE British Library (http://www.lse.ac.uk/library/archive/gutoho/philosophy_archives.htm) This is a microfilm copy of the Stanford University Popper Archive of Popper's papers to whose catalogue a weblink is provided.
- Karl Popper Archive at University Library Klagenfurt (http://web.archive.org/web/20080101043641/http://www.uni-klu.ac.at/ub/sondersammlungen/karl-popper-sammlung/index.html), consists of Popper's Library and paper copies of the Popper Papers at The Hoover Institution Archive at Stanford, California
- Austrian Karl R. Popper Research Association (http://www.karlpopper.info), University of Graz, Austria
The Logic of Scientific Discovery

<table>
<thead>
<tr>
<th>The Logic of Scientific Discovery</th>
</tr>
</thead>
<tbody>
<tr>
<td>Author(s)</td>
</tr>
<tr>
<td>Original title</td>
</tr>
<tr>
<td>Cover artist</td>
</tr>
<tr>
<td>Language</td>
</tr>
<tr>
<td>Subject(s)</td>
</tr>
<tr>
<td>Publisher</td>
</tr>
<tr>
<td>Publication date</td>
</tr>
<tr>
<td>Published in English</td>
</tr>
<tr>
<td>OCLC Number</td>
</tr>
</tbody>
</table>

The Logic of Scientific Discovery (German: Logik der Forschung, [2] which, however, literally means "The Logic of Research"[3]) is a 1934 book by Karl Popper. Popper rewrote his book in English and republished it in 1959. It argues that science should adopt a methodology based on falsifiability, because no number of experiments can ever prove a theory, but a single experiment can contradict one. Popper holds that empirical theories are characterized by falsifiability.


Notes

External links
- English version publisher's web site (http://www.routledge.com/popper/works/logic_discovery.html)
- German version publisher's web site (http://www.mohr.de/)
- Limited preview of the book with a table of contents (http://books.google.com/books?id=Yq6xeupNStMC&dq=The+Logic+of+Scientific+Discovery&source=gbs_navlinks_s), Google Books
Falsifiability

In the philosophy of science, falsifiability or refutability is a quality or characteristic of a scientific hypothesis or theory. Falsifiability is considered a positive (and often essential) quality of a hypothesis because it means that the hypothesis is testable by empirical experiment and thus conforms to the standards of scientific method. That something is "falsifiable" does not mean it is false, rather it means that if it is false, then observation or experiment will at some point demonstrate its falsehood.

For example, the assertion that "all swans are white" is falsifiable, because it is logically possible that a swan can be found that is not white. Not all statements that are falsifiable in principle are falsifiable in practice. For example, "It will be raining here in one million years" is theoretically falsifiable, but not practically so.

The concept first popularized by Karl Popper, who, in his philosophical criticism of the popular positivist view of the scientific method, concluded that a hypothesis, proposition, or theory talks about the observable only if it is falsifiable. "Falsifiable" is often taken to loosely mean "testable." An adage states it loosely as "if it's not falsifiable, then it's not scientific". But the state of being falsifiable or scientific says nothing about its truth, soundness or validity, for example the unfalsifiable statement "That sunset is beautiful."

Overview

The classical view of the philosophy of science is that it is the goal of science to prove hypotheses like "All swans are white" or to induce them from observational data. Popper argued that this would require the inference of a general rule from a number of individual cases, which is inadmissible in deductive logic. However, if one finds one single black swan, deductive logic admits the conclusion that the statement that all swans are white is false. Falsificationism thus strives for questioning, for falsification, of hypotheses instead of proving them.

For a statement to be questioned using observation, it needs to be at least theoretically possible that it can come in conflict with observation. A key observation of falsificationism is thus that a criterion of demarcation is needed to distinguish those statements that can come in conflict with observation and those that cannot (Chorlton, 2012). Popper chose falsifiability as the name of this criterion.

For example, the statement "All swans are white" is falsifiable, because it can come in conflict with the observation "this swan is black." In contrast, the statement "White swans do exist" is not falsifiable, since no counter-example is logically possible. Falsifiability exploits this asymmetry of deductive logic with respect to universal and existential statements to attempt to solve the problem of demarcation.

Popper stressed that unfalsifiable statements are important in science. Contrary to intuition, unfalsifiable statements can be embedded in - and deductively entailed by - falsifiable theories. For example, while "all men are mortal" is unfalsifiable, it is a logical consequence of the falsifiable theory that "every man dies before he reaches the age of 150 years". Similarly, the ancient metaphysical and unfalsifiable idea of the existence of atoms has led to corresponding falsifiable modern theories. Popper invented the notion of metaphysical research programs to name such unfalsifiable ideas. In contrast to Positivism, which held that statements are meaningless if they cannot be verified or falsified, Popper claimed that falsifiability is merely a special case of the more general notion of criticizability, even though he admitted that empirical refutation is one of the most effective methods by which theories can be criticized. Criticizability, in contrast to falsifiability, and thus rationality, may be comprehensive (i.e., have no logical limits), though this claim is controversial even among proponents of Popper's philosophy and critical
Falsifiability is an important concept within the creation–evolution controversy, where proponents of both sides claim that Popper developed falsifiability to denote ideas as unscientific or pseudoscientific and use it to make arguments against the views of the respective other side. The question of what can and cannot be legitimately called science is of major importance in this debate because the First Amendment to the United States Constitution prohibits teaching of religion in public schools. Falsifiability has even been used in court decisions in this context as a key deciding factor to distinguish genuine science from the religious.

Naïve falsification

Two types of statements: observational and categorical

In work beginning in the 1930s, Popper gave falsifiability a renewed emphasis as a criterion of empirical statements in science.

Popper noticed that two types of statements are of particular value to scientists. The first are statements of observations, such as "there is a white swan." Logicians call these statements singular existential statements, since they assert the existence of some particular thing. They are equivalent to a predicate calculus statement of the form: There exists an x such that x is a swan, and x is white.

The second are statements that categorize all instances of something, such as "all swans are white". Logicians call these statements universal. They are usually parsed in the form: For all x, if x is a swan, then x is white. Scientific laws are commonly supposed to be of this type. One difficult question in the methodology of science is: How does one move from observations to laws? How can one validly infer a universal statement from any number of existential statements?

Inductivist methodology supposed that one can somehow move from a series of singular existential statements to a universal statement. That is, that one can move from 'this is a white swan', 'that is a white swan', and so on, to a universal statement such as 'all swans are white.' This method is clearly deductively invalid, since it is always possible that there may be a non-white swan that has eluded observation (and, in fact, the discovery of the Australian black swan demonstrated the deductive invalidity of this particular statement).

Inductive categorical inference

Popper held that science could not be grounded on such an invalid inference. He proposed falsification as a solution to the problem of induction. Popper noticed that although a singular existential statement such as 'there is a white swan' cannot be used to affirm a universal statement, it can be used to show that one is false: the singular existential observation of a black swan serves to show that the universal statement 'all swans are white' is false—in logic this is called modus tollens. 'There is a black swan' implies 'there is a non-white swan,' which, in turn, implies 'there is something that is a swan and that is not white', hence 'all swans are white' is false, because that is the same as 'there is nothing that is a swan and that is not white'.

One notices a white swan. From this one can conclude:

   At least one swan is white.

From this, one may wish to conjecture:

   All swans are white.

It is impractical to observe all the swans in the world to verify that they are all white.

Even so, the statement all swans are white is testable by being falsifiable. For, if in testing many swans, the researcher finds a single black swan, then the statement all swans are white would be falsified by the counterexample of the single black swan.
Deductive falsification

Deductive falsification is different from an absence of verification. The falsification of statements occurs through modus tollens, via some observation. Suppose some universal statement $U$ forbids some observation $O$:

$$U \rightarrow \neg O$$

Observation $O$, however, is made:

$$O$$

So by modus tollens,

$$\neg U$$

Although the logic of naïve falsification is valid, it is rather limited. Nearly any statement can be made to fit the data, so long as one makes the requisite 'compensatory adjustments'. Popper drew attention to these limitations in *The Logic of Scientific Discovery* in response to criticism from Pierre Duhem. W. V. Quine expounded this argument in detail, calling it confirmation holism. To logically falsify a universal, one must find a true falsifying singular statement. But Popper pointed out that it is always possible to change the universal statement or the existential statement so that falsification does not occur. On hearing that a black swan has been observed in Australia, one might introduce the ad hoc hypothesis, 'all swans are white except those found in Australia'; or one might adopt another, more cynical view about some observers, 'Australian bird watchers are incompetent'.

Thus, naïve falsification ought to, but does not, supply a way of handling competing hypotheses for many subject controversies (for instance conspiracy theories and urban legends). People arguing that there is no support for such an observation may argue that there is nothing to see, that all is normal, or that the differences or appearances are too small to be statistically significant. On the other side are those who concede that an observation has occurred and that a universal statement has been falsified as a consequence. Therefore, naïve falsification does not enable scientists, who rely on objective criteria, to present a definitive falsification of universal statements.

Falsificationism

Naïve falsificationism is an unsuccessful attempt to prescribe a rationally unavoidable method for science. Sophisticated methodological falsification, on the other hand, is a prescription of a way in which scientists ought to behave as a matter of choice. The object of this is to arrive at an evolutionary process whereby theories become *less bad*.

Naïve falsification considers scientific statements individually. Scientific theories are formed from groups of these sorts of statements, and it is these groups that must be accepted or rejected by scientists. Scientific theories can always be defended by the addition of ad hoc hypotheses. As Popper put it, a *decision* is required on the part of the scientist to accept or reject the statements that go to make up a theory or that might falsify it. At some point, the weight of the ad hoc hypotheses and disregarded falsifying observations will become so great that it becomes unreasonable to support the base theory any longer, and a decision will be made to reject it.

In place of naïve falsification, Popper envisioned science as evolving by the successive rejection of falsified theories, rather than falsified statements. Falsified theories are to be replaced by theories that can account for the phenomena that falsified the prior theory, that is, with greater explanatory power. For example, Aristotelian mechanics explained observations of everyday situations, but were falsified by Galileo's experiments, and were replaced by Newtonian mechanics, which accounted for the phenomena noted by Galileo (and others). Newtonian mechanics' reach included the observed motion of the planets and the mechanics of gases. The Youngian wave theory of light (i.e., waves carried by the luminiferous aether) replaced Newton's (and many of the Classical Greeks') particles of light but in turn was falsified by the Michelson-Morley experiment and was superseded by Maxwell's electrodynamics and Einstein's special relativity, which did account for the newly observed phenomena. Furthermore, Newtonian mechanics applied to the atomic scale was replaced with quantum mechanics, when the old theory could not provide an answer to the ultraviolet catastrophe, the Gibbs paradox, or how electron orbits could exist without the particles...
radiating away their energy and spiraling towards the centre. Thus the new theory had to posit the existence of
unintuitive concepts such as energy levels, quanta and Heisenberg's uncertainty principle. Although it is generally
believed that one theory replacing another means that the previous has been falsified, in reality the reason why one
theory is accepted and another abandoned is because the former is considered to have more utility than the latter. In
other words, the previous theory is not falsified, but rather, the new theory is simply preferred by scientists as it helps
solve certain puzzles that the previous theory was not capable of addressing. The criterion of falsifiability is
therefore limited. This has been illustrated in the work of Thomas Kuhn, The Structure of Scientific Revolution.\[7\]

The criterion of demarcation

Popper uses falsification as a criterion of demarcation to draw a sharp line between those theories that are scientific
and those that are unscientific. It is useful to know if a statement or theory is falsifiable, if for no other reason than
that it provides us with an understanding of the ways in which one might assess the theory. One might at the least be
saved from attempting to falsify a non-falsifiable theory, or come to see an unfalsifiable theory as unsupportable.

Popper claimed that, if a theory is falsifiable, then it is scientific.

The Popperian criterion excludes from the domain of science not unfalsifiable statements but only whole theories
that contain no falsifiable statements; thus it leaves us with the Duhemian problem of what constitutes a 'whole
theory' as well as the problem of what makes a statement 'meaningful'. Popper's own falsificationism, thus, is not
only an alternative to verificationism, it is also an acknowledgement of the conceptual distinction that previous
theories had ignored.

Verificationism

In the philosophy of science, verificationism (also known as the verifiability theory of meaning) holds that a
statement must, in principle, be empirically verifiable for it to be both meaningful and scientific. This was an
essential feature of the logical positivism of the so-called Vienna Circle that included such philosophers as Moritz
Schlick, Rudolf Carnap, Otto Neurath, the Berlin philosopher Hans Reichenbach, and the logical empiricism of A.J.
Ayer.

Popper noticed that the philosophers of the Vienna Circle had mixed two different problems, that of meaning and
that of demarcation, and had proposed in verificationism a single solution to both. In opposition to this view, Popper
emphasized that there are meaningful theories that are not scientific, and that, accordingly, a criterion of
meaningfulness does not coincide with a criterion of demarcation.

Thus, Popper urged that verifiability be replaced with falsifiability as the criterion of demarcation. On the other
hand, he strictly opposed the view that non-falsifiable statements are meaningless or otherwise inherently bad, and
noted that falsificationism does not imply it.\[8\]

Use in courts of law

Falsifiability was one of the criteria used by Judge William Overton in the McLean v. Arkansas ruling to determine
that 'creation science' was not scientific and should not be taught in Arkansas public schools as such (it can be taught
as religion). In his conclusion related to this criterion he stated that "While anybody is free to approach a scientific
inquiry in any fashion they choose, they cannot properly describe the methodology as scientific, if they start with the
conclusion and refuse to change it regardless of the evidence developed during the course of the investigation."\[9\]

It was also enshrined in United States law as part of the Daubert Standard set by the Supreme Court for whether
scientific evidence is admissible in a jury trial.
Criticisms

Contemporary philosophers

Many contemporary philosophers of science and analytic philosophers are strongly critical of Popper's philosophy of science.\[10]\ Popper's mistrust of inductive reasoning has led to claims that he misrepresents scientific practice. Among the professional philosophers of science, the Popperian view has never been seriously preferred to probabilistic induction, which is the mainstream account of scientific reasoning.\[11]\ Adherents of Popper speak with disrespect of "professional philosophy", for example W. W. Bartley:

Sir Karl Popper is not really a participant in the contemporary professional philosophical dialogue; quite the contrary, he has ruined that dialogue. If he is on the right track, then the majority of professional philosophers the world over have wasted or are wasting their intellectual careers. The gulf between Popper's way of doing philosophy and that of the bulk of contemporary professional philosophers is as great as that between astronomy and astrology.\[12]\

Rafe Champion:

Popper's ideas have failed to convince the majority of professional philosophers because his theory of conjectural knowledge does not even pretend to provide positively justified foundations of belief. Nobody else does better, but they keep trying, like chemists still in search of the Philosopher's Stone or physicists trying to build perpetual motion machines.\[13]\

and David Miller:

What distinguishes science from all other human endeavours is that the accounts of the world that our best, mature sciences deliver are strongly supported by evidence and this evidence gives us the strongest reason to believe them.' That anyway is what is said at the beginning of the advertisement for a recent conference on induction at a celebrated seat of learning in the UK. It shows how much critical rationalists still have to do to make known the message of Logik der Forschung concerning what empirical evidence is able to do and what it does.\[14]\

Kuhn and Lakatos

Whereas Popper was concerned in the main with the logic of science, Thomas Kuhn's influential book The Structure of Scientific Revolutions examined in detail the history of science. Kuhn argued that scientists work within a conceptual paradigm that strongly influences the way in which they see data. Scientists will go to great length to defend their paradigm against falsification, by the addition of \textit{ad hoc} hypotheses to existing theories. Changing a 'paradigm' is difficult, as it requires an individual scientist to break with his or her peers and defend a heterodox theory.

Some falsificationists saw Kuhn's work as a vindication, since it provided historical evidence that science progressed by rejecting inadequate theories, and that it is the \textit{decision}, on the part of the scientist, to accept or reject a theory that is the crucial element of falsificationism. Foremost amongst these was Imre Lakatos.

Lakatos attempted to explain Kuhn's work by arguing that science progresses by the falsification of research programs rather than the more specific universal statements of naïve falsification. In Lakatos' approach, a scientist works within a research program that corresponds roughly with Kuhn's 'paradigm'. Whereas Popper rejected the use of \textit{ad hoc} hypotheses as unscientific, Lakatos accepted their place in the development of new theories.

Some philosophers of science, such as Paul Feyerabend, take Kuhn's work as showing that social factors, rather than adherence to a purely rational method, decide which scientific theories gain general acceptance. Many other philosophers of science dispute such a view, such as Alan Sokal and Kuhn himself.\[15]\
**Feyerabend**

Paul Feyerabend examined the history of science with a more critical eye, and ultimately rejected any prescriptive methodology at all. He rejected Lakatos' argument for ad hoc hypothesis, arguing that science would not have progressed without making use of any and all available methods to support new theories. He rejected any reliance on a scientific method, along with any special authority for science that might derive from such a method. Rather, he claimed that if one is keen to have a universally valid methodological rule, epistemological anarchism or *anything goes* would be the only candidate. For Feyerabend, any special status that science might have derives from the social and physical value of the results of science rather than its method.

**Sokal and Bricmont**

In their book *Fashionable Nonsense* (published in the UK as *Intellectual Impostures*) the physicists Alan Sokal and Jean Bricmont criticized falsifiability on the grounds that it does not accurately describe the way science really works. They argue that theories are used because of their successes, not because of the failures of other theories. Their discussion of Popper, falsifiability and the philosophy of science comes in a chapter entitled "Intermezzo," which contains an attempt to make clear their own views of what constitutes truth, in contrast with the extreme epistemological relativism of postmodernism.

Sokal and Bricmont write, "When a theory successfully withstands an attempt at falsification, a scientist will, quite naturally, consider the theory to be partially confirmed and will accord it a greater likelihood or a higher subjective probability. ... But Popper will have none of this: throughout his life he was a stubborn opponent of any idea of 'confirmation' of a theory, or even of its 'probability'. ... [but] the history of science teaches us that scientific theories come to be accepted above all because of their successes." (Sokal and Bricmont 1997, 62f)

They further argue that falsifiability cannot distinguish between astrology and astronomy, as both make technical predictions that are sometimes incorrect.

David Miller, a contemporary philosopher of critical rationalism, has attempted to defend Popper against these claims. Miller argues that astrology does not lay itself open to falsification, while astronomy does, and this is the litmus test for science.

**Examples**

Claims about verifiability and falsifiability have been used to criticize various controversial views. Examining these examples shows the usefulness of falsifiability by showing us where to look when attempting to criticise a theory.

**Economics**

Aspects of economics have been accused of not being falsifiable, mainly by sociologists and other social scientists in general.

The most common argument is made against rational expectations theories, which work under the assumption that people act to maximize their utility. However, under this viewpoint, it is impossible to disprove the fundamental theory that people are utility-maximizers. The political scientist Graham T. Allison, in his book *Essence of Decision*, attempted to both quash this theory and substitute other possible models of behavior.

Karl Popper argued that Marxism shifted from falsifiable to unfalsifiable. Some economists, such as those of the Austrian School, believe that macroeconomics is empirically unfalsifiable and that thus the only appropriate means to understand economic events is by logically studying the intentions of individual economic decision-makers, based on certain fundamental truths. Prominent figures within the Austrian School of economics Ludwig von Mises and Friedrich Hayek were associates of Karl Popper's, whom they co-founded the Mont Pelerin Society with.
Evolution

Numerous examples of potential (indirect) ways to falsify common descent have been proposed by its proponents. J.B.S. Haldane, when asked what hypothetical evidence could disprove evolution, replied “fossil rabbits in the Precambrian era”. Richard Dawkins adds that any other modern animals, such as a hippo, would suffice. Karl Popper at first spoke against the testability of natural selection but later recanted, "I have changed my mind about the testability and logical status of the theory of natural selection, and I am glad to have the opportunity to make a recantation.

Historicism

Theories of history or politics that allegedly predict future events have a logical form that renders them neither falsifiable nor verifiable. They claim that for every historically significant event, there exists an historical or economic law that determines the way in which events proceeded. Failure to identify the law does not mean that it does not exist, yet an event that satisfies the law does not prove the general case. Evaluation of such claims is at best difficult. On this basis, Popper "fundamentally criticized historicism in the sense of any preordained prediction of history", and argued that neither Marxism nor psychoanalysis was science, although both made such claims. Again, this does not mean that any of these types of theories is necessarily incorrect. Popper considered falsifiability a test of whether theories are scientific, not of whether propositions that they contain or support are true.

Mathematics

Many philosophers believe that mathematics is not experimentally falsifiable, and thus not a science according to the definition of Karl Popper. However, in the 1930s Gödel's incompleteness theorems convinced many mathematicians that mathematics cannot be reduced to logic alone, and Karl Popper concluded that "most mathematical theories are, like those of physics and biology, hypothetico-deductive: pure mathematics therefore turns out to be much closer to the natural sciences whose hypotheses are conjectures, than it seemed even recently." Other thinkers, notably Imre Lakatos, have applied a version of falsificationism to mathematics itself. Like all formal sciences, mathematics is not concerned with the validity of theories based on observations in the empirical world, but rather, mathematics is occupied with the theoretical, abstract study of such topics as quantity, structure, space and change. Methods of the mathematical sciences are, however, applied in constructing and testing scientific models dealing with observable reality. Albert Einstein stated that "as far as the laws of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality."

Quotations

• Albert Einstein is reported to have said: No amount of experimentation can ever prove me right; a single experiment can prove me wrong. (paraphrased)

• The criterion of the scientific status of a theory is its falsifiability, or refutability, or testability. — Karl Popper,

Notes


[2] LScD p. 4


[6] Quantum theory and the schism in physics, introductory comments
Falsifiability

[8] Logic of Scientific Discovery, section 6, footnote *3
[17] "For Marxism, Popper believed, had been initially scientific, in that Marx had postulated a theory which was genuinely predictive. However, when these predictions were not in fact borne out, the theory was saved from falsification by the addition of ad hoc hypotheses which made it compatible with the facts. By this means, Popper asserted, a theory which was initially genuinely scientific degenerated into pseudo-scientific dogma." Karl Popper (http://plato.stanford.edu/entries/popper/) at the Stanford Encyclopedia of Philosophy.
[31] Einstein, p. 28. The quote is Einstein's answer to the question: "how can it be that mathematics, being after all a product of human thought which is independent of experience, is so admirably appropriate to the objects of reality?" He, too, is concerned with The Unreasonable Effectiveness of Mathematics in the Natural Sciences.
References


External links

• Problems with Falsificationism (http://www.galilean-library.org/falsificationism.html) at The Galilean Library
Thomas Kuhn

Thomas Samuel Kuhn

| Born       | July 18, 1922  
|            | Cincinnati, Ohio  |
| Died       | June 17, 1996 (aged 73)  
|            | Cambridge, Massachusetts  |
| Era        | 20th-century philosophy  |
| Region     | Western Philosophy  |
| School     | Analytic  |
| Main interests | Philosophy of science  |
| Notable ideas | Paradigm shift  
|            | Incommensurability  
|            | "Normal" science  |

Thomas Samuel Kuhn (ˈkuːn; July 18, 1922 – June 17, 1996) was an American physicist, historian, and philosopher of science whose controversial 1962 book *The Structure of Scientific Revolutions* was deeply influential in both academic and popular circles, introducing the term "paradigm shift", which has since become an English-language staple.

Kuhn made several notable claims concerning the progress of scientific knowledge: that scientific fields undergo periodic "paradigm shifts" rather than solely progressing in a linear and continuous way; that these paradigm shifts open up new approaches to understanding that scientists would never have considered valid before; and that the notion of scientific truth, at any given moment, cannot be established solely by objective criteria but is defined by a consensus of a scientific community. Competing paradigms are frequently incommensurable; that is, they are competing accounts of reality which cannot be coherently reconciled. Thus, our comprehension of science can never rely on full "objectivity"; we must account for subjective perspectives as well.

Life

Kuhn was born in Cincinnati, Ohio, to Samuel L. Kuhn, an industrial engineer, and Minette Stroock Kuhn. He obtained his B.S. degree in physics from Harvard University in 1943, where he also obtained M.S. and Ph.D. degrees in physics in 1946 and 1949, respectively. As he states in the first few pages of the preface to the second edition of *The Structure of Scientific Revolutions*, his three years of total academic freedom as a Harvard Junior Fellow were crucial in allowing him to switch from physics to the history (and philosophy) of science. He later taught a course in the history of science at Harvard from 1948 until 1956, at the suggestion of university president James Conant. After leaving Harvard, Kuhn taught at the University of California, Berkeley, in both the philosophy department and the history department, being named Professor of the History of Science in 1961. Kuhn interviewed and tape recorded Danish physicist Niels Bohr the day before Bohr's death.[1] At Berkeley, he wrote and published (in 1962) his best known and most influential work: [2] *The Structure of Scientific Revolutions*. In 1964, he joined Princeton University as the M. Taylor Pyne Professor of Philosophy and History of Science. In 1979 he joined the Massachusetts Institute of Technology (MIT) as the Laurance S. Rockefeller Professor of Philosophy, remaining there until 1991. In 1994 Kuhn was diagnosed with lung cancer. He passed away in 1996.

Thomas Kuhn was married twice, first to Kathryn Muhs (with whom he had three children,) and later to Jehane Barton Burns (Jehane R. Kuhn).
The Structure of Scientific Revolutions

The Structure of Scientific Revolutions (SSR) was originally printed as an article in the International Encyclopedia of Unified Science, published by the logical positivists of the Vienna Circle. In this book, Kuhn argued that science does not progress via a linear accumulation of new knowledge, but undergoes periodic revolutions, also called "paradigm shifts" (although he did not coin the phrase),[3] in which the nature of scientific inquiry within a particular field is abruptly transformed. In general, science is broken up into three distinct stages. Prescience, which lacks a central paradigm, comes first. This is followed by "normal science", when scientists attempt to enlarge the central paradigm by "puzzle-solving". Guided by the paradigm, normal science is extremely productive: "when the paradigm is successful, the profession will have solved problems that its members could scarcely have imagined and would never have undertaken without commitment to the paradigm".[4]

During the period of normal science, the failure of a result to conform to the paradigm is seen not as refuting the paradigm, but as the mistake of the researcher, contra Popper's falsifiability criterion. As anomalous results build up, science reaches a crisis, at which point a new paradigm, which subsumes the old results along with the anomalous results into one framework, is accepted. This is termed revolutionary science.

In SSR, Kuhn also argues that rival paradigms are incommensurable—that is, it is not possible to understand one paradigm through the conceptual framework and terminology of another rival paradigm. For many critics, for example David Stove (Popper and After, 1982), this thesis seemed to entail that theory choice is fundamentally irrational: if rival theories cannot be directly compared, then one cannot make a rational choice as to which one is better. Whether Kuhn's views had such relativistic consequences is the subject of much debate; Kuhn himself denied the accusation of relativism in the third edition of SSR, and sought to clarify his views to avoid further misinterpretation. Freeman Dyson has quoted Kuhn as saying "I am not a Kuhnian!",[5] referring to the relativism that some philosophers have developed based on his work.

The enormous impact of Kuhn's work can be measured in the changes it brought about in the vocabulary of the philosophy of science: besides "paradigm shift", Kuhn popularized the word "paradigm" itself from a term used in certain forms of linguistics and the work of Georg Lichtenberg to its current broader meaning, coined the term "normal science" to refer to the relatively routine, day-to-day work of scientists working within a paradigm, and was largely responsible for the use of the term "scientific revolutions" in the plural, taking place at widely different periods of time and in different disciplines, as opposed to a single "Scientific Revolution" in the late Renaissance. The frequent use of the phrase "paradigm shift" has made scientists more aware of and in many cases more receptive to paradigm changes, so that Kuhn's analysis of the evolution of scientific views has by itself influenced that evolution.

Kuhn's work has been extensively used in social science; for instance, in the post-positivist/positivist debate within International Relations. Kuhn is credited as a foundational force behind the post-Mertonian Sociology of Scientific Knowledge.

A defense Kuhn gives against the objection that his account of science from The Structure of Scientific Revolutions results in relativism can be found in an essay by Kuhn called "Objectivity, Value Judgment, and Theory Choice."[6] In this essay, he reiterates five criteria from the penultimate chapter of SSR that determine (or help determine, more properly) theory choice:

1. - **Accurate** - empirically adequate with experimentation and observation
2. - **Consistent** - internally consistent, but also externally consistent with other theories
3. - **Broad Scope** - a theory's consequences should extend beyond that which it was initially designed to explain
4. - **Simple** - the simplest explanation, principally similar to Occam's razor
5. - **Fruitful** - a theory should disclose new phenomena or new relationships among phenomena

He then goes on to show how, although these criteria admittedly determine theory choice, they are imprecise in practice and relative to individual scientists. According to Kuhn, "When scientists must choose between competing
theories, two men fully committed to the same list of criteria for choice may nevertheless reach different conclusions."[6] For this reason, the criteria still are not "objective" in the usual sense of the word because individual scientists reach different conclusions with the same criteria due to valuing one criterion over another or even adding additional criteria for selfish or other subjective reasons. Kuhn then goes on to say, "I am suggesting, of course, that the criteria of choice with which I began function not as rules, which determine choice, but as values, which influence it."[6] Because Kuhn utilizes the history of science in his account of science, his criteria or values for theory choice are often understood as descriptive normative rules (or more properly, values) of theory choice for the scientific community rather than prescriptive normative rules in the usual sense of the word "criteria", although there are many varied interpretations of Kuhn's account of science.

Polanyi–Kuhn debate

Although they used different terminologies, both Kuhn and Michael Polanyi believed that scientists' subjective experiences made science a relativized discipline. Polanyi lectured on this topic for decades before Kuhn published *The Structure of Scientific Revolutions*.

Supporters of Polanyi charged Kuhn with plagiarism, as it was known that Kuhn attended several of Polanyi's lectures, and that the two men had debated endlessly over the epistemology of science before either had achieved fame. In response to these critics, Kuhn cited Polanyi in the second edition of "The Structure of Scientific Revolutions",[7] and the two scientists agreed to set aside their differences. Despite this intellectual alliance, Polanyi's work was constantly interpreted by others within the framework of Kuhn's paradigm shifts, much to Polanyi's (and Kuhn's) dismay.[8]

Thomas Kuhn Paradigm Shift Award

In honor of his legacy, the "Thomas Kuhn Paradigm Shift Award" is awarded by the American Chemical Society to speakers who present original views that are at odds with mainstream scientific understanding. The winner is selected based in the novelty of the viewpoint and its potential impact if it were to be widely accepted.[9][10]

Honors

Kuhn was named a Guggenheim Fellow in 1954, and in 1982 was awarded the George Sarton Medal by the History of Science Society. He has also received numerous honorary doctorates.

Bibliography


**References**


**External links**

- Thomas Kuhn (http://plato.stanford.edu/entries/thomas-kuhn/) entry by Alexander Bird in the *Stanford Encyclopedia of Philosophy*
- Thomas Kuhn (http://des.emory.edu/mpf/Kuhnsnap.html) (Biography, Outline of Structure of Scientific Revolutions)
- Color Portrait (http://stanstudio.com/Boston_Photo_Blog/photo-of-thomas-samuel-kuhn/)
- History of Twentieth-Century Philosophy of Science (http://www.philsci.com) BOOK VI: Kuhn on Revolution and Feyerabend on Anarchy - with free downloads for public use.
- article on his *Road Since Structure: Philosophical Essays* (http://www.shvoong.com/books/98090-road-structure-philosophical-essays/)
- John Horgan’s Interview http://www.stevens.edu/csw/cgi-bin/shapers/kuhn/
The Structure of Scientific Revolutions

The Structure of Scientific Revolutions by Thomas S. Kuhn, is an analysis of the history of science, published in 1962. Its publication was a landmark event in the history, philosophy, and sociology of scientific knowledge and it triggered an ongoing worldwide assessment and reaction in—and beyond—those scholarly communities. In this work, Kuhn challenged the then prevailing view of progress in "normal science". Scientific progress had been seen primarily as "development-by-accumulation" of accepted facts and theories. Kuhn argued for an episodic model in which periods of such conceptual continuity in normal science were interrupted by periods of revolutionary science. During revolutions in science the discovery of anomalies leads to a whole new paradigm that changes the rules of the game and the "map" directing new research, asks new questions of old data, and moves beyond the puzzle-solving of normal science.¹

For example, Kuhn's analysis of the Copernican Revolution emphasized that, in its beginning, it did not offer more accurate predictions of celestial events, such as planetary positions, than the Ptolemaic system, but instead appealed to some practitioners based on a promise of better, simpler, solutions that might be developed at some point in the future. Kuhn called the core concepts of an ascendant revolution its "paradigms" and thereby launched this word into widespread analogical use in the second half of the 20th century. Kuhn's insistence that a paradigm shift was a mélange of sociology, enthusiasm and scientific promise, but not a logically determinate procedure, caused an uproar in reaction to his work. Kuhn addressed concerns in the 1969 postscript to the second edition. For some commentators it introduced a realistic humanism into the core of science while for other the nobility of science was tarnished by Kuhn's introduction of an irrational element into the heart of its greatest achievements.
History


Kuhn dated the genesis of his book to 1947, when he was a graduate student at Harvard University and had been asked to teach a science class for humanities undergraduates with a focus on historical case studies. Kuhn later commented that until then, "I'd never read an old document in science." Aristotle's *Physics* was astonishingly unlike Isaac Newton's work in its concepts of matter and motion. Kuhn concluded that Aristotle's concepts were not "bad Newton", just different.

Synopsis

Basic approach

Kuhn's approach to the history and philosophy of science has been described as focusing on conceptual issues: what sorts of ideas were thinkable at a particular time? What sorts of intellectual options and strategies were available to people during a given period? What types of lexicons and terminology were known and employed during certain epochs? Stressing the importance of not attributing modern modes of thought to historical actors, Kuhn's book argues that the evolution of scientific theory does not emerge from the straightforward accumulation of facts, but rather from a set of changing intellectual circumstances and possibilities. Such an approach is largely commensurate with the general historical school of non-linear history.

Historical examples

Kuhn explains his ideas using examples taken from the history of science. For instance, at a particular stage in the history of chemistry, some chemists began to explore the idea of atomism. When many substances are heated they have a tendency to decompose into their constituent elements, and often (though not invariably) these elements can be observed to combine only in set proportions. At one time, a combination of water and alcohol was generally classified as a *compound*. Nowadays it is considered to be a *solution*, but there was no reason then to suspect that it was not a compound. Water and alcohol would not separate spontaneously, but they could be separated when heated. Water and alcohol can be combined in any proportion.

A chemist favoring atomic theory would have viewed all compounds whose elements combine in fixed proportions as exhibiting normal behavior, and all known exceptions to this pattern would be regarded as anomalies whose behavior would probably be explained at some time in the future. On the other hand, if a chemist believed that theories of the atomicity of matter were erroneous, then all compounds whose elements combined in fixed proportions would be regarded as anomalies whose behavior would probably be explained at some time in the future, and all those compounds whose elements are capable of combining in any ratio would be seen as exhibiting the normal behavior of compounds. Nowadays the consensus is that the atomists' view was correct. But if one were to restrict oneself to thinking about chemistry using only the knowledge available at the time, either point of view would be defensible.
The Copernican Revolution

What is arguably the most famous example of a revolution in scientific thought is the Copernican Revolution. In Ptolemy's school of thought, cycles and epicycles (with some additional concepts) were used for modeling the movements of the planets in a cosmos that had a stationary Earth at its center. As accuracy of celestial observations increased, complexity of the Ptolemaic cyclical and epicyclical mechanisms had to increase to maintain the calculated planetary positions close to the observed positions. Copernicus proposed a cosmology in which the Sun was at the center and the Earth was one of the planets revolving around it. For modeling the planetary motions, Copernicus used the tools he was familiar with, namely the cycles and epicycles of the Ptolemaic toolbox. But Copernicus' model needed more cycles and epicycles than existed in the then-current Ptolemaic model, and due to a lack of accuracy in calculations, Copernicus's model did not appear to provide more accurate predictions than the Ptolemy model. Copernicus' contemporaries rejected his cosmology, and Kuhn asserts that they were quite right to do so: Copernicus' cosmology lacked credibility.

Thomas Kuhn illustrates how a paradigm shift later became possible when Galileo Galilei introduced his new ideas concerning motion. Intuitively, when an object is set in motion, it soon comes to a halt. A well-made cart may travel a long distance before it stops, but unless something keeps pushing it, it will eventually stop moving. Aristotle had argued that this was presumably a fundamental property of nature: for the motion of an object to be sustained, it must continue to be pushed. Given the knowledge available at the time, this represented sensible, reasonable thinking. Galileo put forward a bold alternative conjecture: suppose, he said, that we always observe objects coming to a halt simply because some friction is always occurring. Galileo had no equipment with which to objectively confirm his conjecture, but he suggested that without any friction to slow down an object in motion, its inherent tendency is to maintain its speed without the application of any additional force.

The Ptolemaic approach of using cycles and epicycles was becoming strained: there seemed to be no end to the mushrooming growth in complexity required to account for the observable phenomena. Johannes Kepler was the first person to abandon the tools of the Ptolemaic paradigm. He started to explore the possibility that the planet Mars might have an elliptical orbit rather than a circular one. Clearly, the angular velocity could not be constant, but it proved very difficult to find the formula describing the rate of change of the planet's angular velocity. After many years of calculations, Kepler arrived at what we now know as the law of equal areas.

Galileo's conjecture was merely that— a conjecture. So was Kepler's cosmology. But each conjecture increased the credibility of the other, and together, they changed the prevailing perceptions of the scientific community. Later, Newton showed that Kepler's three laws could all be derived from a single theory of motion and planetary motion. Newton solidified and unified the paradigm shift that Galileo and Kepler had initiated.

Coherence

One of the aims of science is to find models that will account for as many observations as possible within a coherent framework. Together, Galileo's rethinking of the nature of motion and Keplerian cosmology represented a coherent framework that was capable of rivaling the Aristotelian/Ptolemaic framework.

Once a paradigm shift has taken place, the textbooks are rewritten. Often the history of science too is rewritten, being presented as an inevitable process leading up to the current, established framework of thought. There is a prevalent belief that all hitherto-unexplained phenomena will in due course be accounted for in terms of this established framework. Kuhn states that scientists spend most (if not all) of their careers in a process of puzzle-solving. Their puzzle-solving is pursued with great tenacity, because the previous successes of the established paradigm tend to generate great confidence that the approach being taken guarantees that a solution to the puzzle exists, even though it may be very hard to find. Kuhn calls this process normal science.

As a paradigm is stretched to its limits, anomalies— failures of the current paradigm to take into account observed phenomena— accumulate. Their significance is judged by the practitioners of the discipline. Some anomalies may
be dismissed as errors in observation, others as merely requiring small adjustments to the current paradigm that will be clarified in due course. Some anomalies resolve themselves spontaneously, having increased the available depth of insight along the way. But no matter how great or numerous the anomalies that persist, Kuhn observes, the practicing scientists will not lose faith in the established paradigm for as long as no credible alternative is available; to lose faith in the solubility of the problems would in effect mean ceasing to be a scientist.

In any community of scientists, Kuhn states, there are some individuals who are bolder than most. These scientists, judging that a crisis exists, embark on what Thomas Kuhn calls revolutionary science, exploring alternatives to long-held, obvious-seeming assumptions. Occasionally this generates a rival to the established framework of thought. The new candidate paradigm will appear to be accompanied by numerous anomalies, partly because it is still so new and incomplete. The majority of the scientific community will oppose any conceptual change, and, Kuhn emphasizes, so they should. To fulfill its potential, a scientific community needs to contain both individuals who are bold and individuals who are conservative. There are many examples in the history of science in which confidence in the established frame of thought was eventually vindicated. Whether the anomalies of a candidate for a new paradigm will be resolvable is almost impossible to predict. Those scientists who possess an exceptional ability to recognize a theory's potential will be the first whose preference is likely to shift in favour of the challenging paradigm. There typically follows a period in which there are adherents of both paradigms. In time, if the challenging paradigm is solidified and unified, it will replace the old paradigm, and a paradigm shift will have occurred.

Three phases

Chronologically, Kuhn distinguishes between three phases. The first phase, which exists only once, is the pre-paradigm phase, in which there is no consensus on any particular theory, though the research being carried out can be considered scientific in nature. This phase is characterized by several incompatible and incomplete theories. If the actors in the pre-paradigm community eventually gravitate to one of these conceptual frameworks and ultimately to a widespread consensus on the appropriate choice of methods, terminology and on the kinds of experiment that are likely to contribute to increased insights, then the second phase, normal science, begins, in which puzzles are solved within the context of the dominant paradigm. As long as there is consensus within the discipline, normal science continues. Over time, progress in normal science may reveal anomalies, facts that are difficult to explain within the context of the existing paradigm. While usually these anomalies are resolved, in some cases they may accumulate to the point where normal science becomes difficult and where weaknesses in the old paradigm are revealed. Kuhn refers to this as a crisis. Crises are often resolved within the context of normal science. However, after significant efforts of normal science within a paradigm fail, science may enter the third phase, that of revolutionary science, in which the underlying assumptions of the field are reexamined and a new paradigm is established. After the new paradigm's dominance is established, scientists return to normal science, solving puzzles within the new paradigm. A science may go through these cycles repeatedly, though Kuhn notes that it is a good thing for science that such shifts do not occur often or easily.

Incommensurability

According to Kuhn, the scientific paradigms preceding and succeeding a paradigm shift are so different that their theories are incommensurable — the new paradigm cannot be proven or disproven by the rules of the old paradigm, and vice versa. The paradigm shift does not merely involve the revision or transformation of an individual theory, it changes the way terminology is defined, how the scientists in that field view their subject, and, perhaps most significantly, what questions are regarded as valid, and what rules are used to determine the truth of a particular theory. The new theories were not, as the scientists had previously thought, just extensions of old theories, but were instead completely new world views. Such incommensurability exists not just before and after a paradigm shift, but in the periods in between conflicting paradigms. It is simply not possible, according to Kuhn, to construct an impartial language that can be used to perform a neutral comparison between conflicting paradigms, because the
very terms used are integral to the respective paradigms, and therefore have different connotations in each paradigm. The advocates of mutually exclusive paradigms are in a difficult position: "Though each may hope to convert the other to his way of seeing science and its problems, neither may hope to prove his case. The competition between paradigms is not the sort of battle that can be resolved by proofs." (SSR, p. 148). Scientists subscribing to different paradigms end up talking past one another.

Kuhn (SSR, section XII) states that the probabilistic tools used by verificationists are inherently inadequate for the task of deciding between conflicting theories, since they belong to the very paradigms they seek to compare. Similarly, observations that are intended to falsify a statement will fall under one of the paradigms they are supposed to help compare, and will therefore also be inadequate for the task. According to Kuhn, the concept of falsifiability is unhelpful for understanding why and how science has developed as it has. In the practice of science, scientists will only consider the possibility that a theory has been falsified if an alternative theory is available that they judge credible. If there is not, scientists will continue to adhere to the established conceptual framework. If a paradigm shift has occurred, the textbooks will be rewritten to state that the previous theory has been falsified.

**Kuhn's opinion on scientific progress**

The first edition of SSR ended with a chapter entitled "Progress through Revolutions", in which Kuhn spelled out his views on the nature of scientific progress. Since he considered problem solving to be a central element of science, Kuhn saw that for a new candidate for paradigm to be accepted by a scientific community, "First, the new candidate must seem to resolve some outstanding and generally recognized problem that can be met in no other way. Second, the new paradigm must promise to preserve a relatively large part of the concrete problem solving activity that has accrued to science through its predecessors. And overall Kuhn maintained that the new paradigm must also solve more problems than its predecessor, which therefore entailed that the number of newly solved problems must be greater than those solved in the old paradigm.

In the second edition of SSR, Kuhn added a postscript in which he elaborated his ideas on the nature of scientific progress. He described a thought experiment involving an observer who has the opportunity to inspect an assortment of theories, each corresponding to a single stage in a succession of theories. What if the observer is presented with these theories without any explicit indication of their chronological order? Kuhn anticipates that it will be possible to reconstruct their chronology on the basis of the theories' scope and content, because the more recent a theory is, the better it will be as an instrument for solving the kinds of puzzle that scientists aim to solve. Kuhn remarked: "That is not a relativist's position, and it displays the sense in which I am a convinced believer in scientific progress."

**Influence**

In 1987, Kuhn's work was reported to be the twentieth-century book most frequently cited in the period 1976-83 in the Arts and the Humanities and the Times Literary Supplement labeled it one of "The Hundred Most Influential Books Since the Second World War." The book's basic concepts have been adopted and co-opted by a variety of fields and disciplines beyond those encompassing the history and philosophy of science.

SSR is viewed by postmodern and post-structuralist thinkers as having called into question the enterprise of science by demonstrating that scientific knowledge is dependent on the culture and historical circumstances of groups of scientists rather than on their adherence to a specific, definable method. In this regard, Kuhn is considered a precursor to the more radical thinking of Paul Feyerabend. Kuhn's work has also been regarded as blurring the demarcation between scientific and non-scientific enterprises, because it describes the mechanism of scientific progress without invoking any idealized scientific method that is capable of distinguishing science from non-science. In the years following the publication of The Structure of Scientific Revolutions, debate raged with adherents of Karl Popper's doctrine of falsificationism, such as Imre Lakatos.
On the one hand, logical positivists and many scientists have criticized Kuhn's "humanizing" of the scientific process for going too far, while the postmodernists, together with Feyerabend, have criticized Kuhn for not going far enough. SSR has also been embraced by creationists who see creationism as an incommensurate worldview in contrast to naturalism while holding science as a valuable tool.\[7]\[8\] It was also in tune with a national change in attitudes towards science\[9\] in the United States at the time of the book's publication, influenced by the Cold War confrontation with the Soviet Union, beginning with the launching of the space satellite Sputnik in 1957. (Rachel Carson's Silent Spring was also published in 1962).

The changes that occur in politics, society and business are often expressed in Kuhnian terms, however poor their parallel with the practice of science may seem to scientists and historians of science. The terms "paradigm" and "paradigm shift" have become such notorious clichés and buzzwords that they are viewed as effectively devoid of content.\[10\] [11] [12] Misused and overused to the point of becoming meaningless, their use in these contexts rarely has any firm foundation in Kuhn's original definitions.

**Criticisms**

Kuhn's SSR was soon criticized by his colleagues in the history and philosophy of science. In 1965, a special symposium on SSR was held at an International Colloquium on the Philosophy of Science that took place at Bedford College, London, and was chaired by Karl Popper. The symposium led to the publication of the symposium's presentations plus other essays, most of them critical, which eventually appeared in an influential volume of essays that by 1999 had gone through 21 printings. Kuhn expressed the opinion that his critics’ readings of his book were so inconsistent with his own understanding of it that he was "...tempted to posit the existence of two Thomas Kuhns," one the author of his book, the other the individual who had been criticized in the symposium by "Professors Popper, Feyerabend, Lakatos, Toulmin and Watkins."[13]

**Concept of paradigm**

In his 1972 work, Human Understanding, Stephen Toulmin argued that a more realistic picture of science than that presented in SSR would admit the fact that revisions in science take place much more frequently, and are much less dramatic than can be explained by the model of revolution/normal science. In Toulmin's view, such revisions occur quite often during periods of what Kuhn would call “normal science.” For Kuhn to explain such revisions in terms of the non-paradigmatic puzzle solutions of normal science, he would need to delineate what is perhaps an implausibly sharp distinction between paradigmatic and non-paradigmatic science.[14]

**Incommensurability of paradigms**

In a series of texts published in the early 1970s, C.R. Kordig asserted a position somewhere between that of Kuhn and the older philosophy of science. His criticism of the Kuhnian position was that the incommensurability thesis was too radical, and that this made it impossible to explain the confrontation of scientific theories that actually occurs. According to Kordig, it is in fact possible to admit the existence of revolutions and paradigm shifts in science while still recognizing that theories belonging to different paradigms can be compared and confronted on the plane of observation. Those who accept the incommensurability thesis do not do so because they admit the discontinuity of paradigms, but because they attribute a radical change in meanings to such shifts.[15]

Kordig maintains that there is a common observational plane. For example, when Kepler and Tycho Brahe are trying to explain the relative variation of the distance of the sun from the horizon at sunrise, both see the same thing (the same configuration is focused on the retina of each individual). This is just one example of the fact that "rival scientific theories share some observations, and therefore some meanings.” Kordig suggests that with this approach, he is not reintroducing the distinction between observations and theory in which the former is assigned a privileged and neutral status, but that it is possible to affirm more simply the fact that, even if no sharp distinction exists between theory and observations, this does not imply that there are no comprehensible differences at the two
extremes of this polarity.

At a secondary level, for Kordig there is a common plane of inter-paradigmatic standards or shared norms that permit the effective confrontation of rival theories.\[15\]

In 1973, Hartry Field published an article that also sharply criticized Kuhn's idea of incommensurability. In particular, he took issue with this passage from Kuhn:

"Newtonian mass is immutably conserved; that of Einstein is convertible into energy. Only at very low relative velocities can the two masses be measured in the same way, and even then they must not be conceived as if they were the same thing." (Kuhn 1970).

Field takes this idea of incommensurability between the same terms in different theories one step further. Instead of attempting to identify a persistence of the reference of terms in different theories, Field's analysis emphasizes the indeterminacy of reference within individual theories. Field takes the example of the term "mass", and asks what exactly "mass" means in modern post-relativistic physics. He finds that there are at least two different definitions:

1) Relativistic mass: the mass of a particle is equal to the total energy of the particle divided by the speed of light squared. Since the total energy of a particle in relation to one system of reference differs from the total energy in relation to other systems of reference, while the speed of light remains constant in all systems, it follows that the mass of a particle has different values in different systems of reference.

2) "Real" mass: the mass of a particle is equal to the non-kinetic energy of a particle divided by the speed of light squared. Since non-kinetic energy is the same in all systems of reference, and the same is true of light, it follows that the mass of a particle has the same value in all systems of reference.

Projecting this distinction backwards in time onto Newtonian dynamics, we can formulate the following two hypotheses:

HR: the term "mass" in Newtonian theory denotes relativistic mass.

Hp: the term "mass" in Newtonian theory denotes "real" mass.

According to Field, it is impossible to decide which of these two affirmations is true. Prior to the theory of relativity, the term "mass" was referentially indeterminate. But this does not mean that the term "mass" did not have a different meaning than it now has. The problem is not one of meaning but of reference. The reference of such terms as mass is only partially determined: we don't really know how Newton intended his use of this term to be applied. As a consequence, neither of the two terms fully denotes (refers). It follows that it is improper to maintain that a term has changed its reference during a scientific revolution; it is more appropriate to describe terms such as "mass" as "having undergone a denotational refinement."\[16\]

Incommensurability and perception

The close connection between the interpretationalist hypothesis and a holistic conception of beliefs is at the root of the notion of the dependence of perception on theory, a central concept in SSR. Kuhn maintained that the perception of the world depends on how the percipient conceives the world: two scientists who witness the same phenomenon and are steeped in two radically different theories will see two different things. According to this view, our interpretation of the world determines what we see.\[17\]

Jerry Fodor attempts to establish that this theoretical paradigm is fallacious and misleading by demonstrating the impenetrability of perception to the background knowledge of subjects. The strongest case can be based on evidence from experimental cognitive psychology, namely the persistence of perceptual illusions. Knowing that the lines in the Müller-Lyer illusion are equal does not prevent one from continuing to see one line as being longer than the other. This impenetrability of the information elaborated by the mental modules limits the scope of interpretationalism.

In epistemology, for example, the criticism of what Fodor calls the interpretationalist hypothesis accounts for the common-sense intuition (on which naïve physics is based) of the independence of reality from the conceptual
categories of the experimenter. If the processes of elaboration of the mental modules are in fact independent of the background theories, then it is possible to maintain the realist view that two scientists who embrace two radically diverse theories see the world exactly in the same manner even if they interpret it differently. The point is that it is necessary to distinguish between observations and the perceptual fixation of beliefs. While it is beyond doubt that the second process involves the holistic relationship between beliefs, the first is largely independent of the background beliefs of individuals.

Other critics, such as Israel Sheffler, Hilary Putnam and Saul Kripke, have focused on the Fregean distinction between sense and reference in order to defend scientific realism. Sheffler contends that Kuhn confuses the meanings of terms such as “mass” with their references. While their meanings may very well differ, their references (the objects or entities to which they correspond in the external world) remain fixed.

**Eurocentrism**

More recently, criticism from a different direction has been developed by Arun Bala in his study *The Dialogue of Civilizations in the Birth of Modern Science* (Palgrave Macmillan, 2006). He charges that *The Structure of Scientific Revolutions* is itself a profoundly Eurocentric work, although it is often perceived as opening the door to the multicultural turn in historical studies of science. Bala charges that Kuhn ignores the significant impact of Arabic and Chinese science when he writes:

> Every civilization of which we have records has possessed a technology, an art, a religion, a political system, laws and so on. In many cases those facets of civilizations have been as developed as our own. But only the civilizations that descend from Hellenic Greece have possessed more than the most rudimentary science. The bulk of scientific knowledge is a product of Europe in the last four centuries. No other place and time has supported the very special communities from which scientific productivity comes.

—Kuhn, 1962, pp. 167-168

Bala argues that it is precisely Kuhn's postmodern epistemological paradigm that obstructs recognition of non-Western influences on modern science. Bala argues that this leads Kuhn to treat different cultural scientific traditions as separate intellectual universes isolated from each other. Instead, Bala argues, we would have a different multicultural picture of science by including the contributions from Arabic, Chinese, ancient Egyptian and Indian traditions of philosophy, mathematics, astronomy and physics that went into shaping the birth of modern science.

**Editions**

Notes


[3] This overall requirement of increasing the number of problems solved is evident in such remarks as follows: "...the nature of [scientific] communities provides a virtual guarantee that both the list of problems solved by science and the precision of individual solutions will grow and grow." (p169); "The scientific community is a supremely efficient instrument for maximising the number and precision of the problem[s] solved through paradigm change.(p168); "...a community of scientific specialists will do all it can to ensure the continuing growth of the assembled data that it can treat with precision and detail." (p168)


External links

- Summary of book (http://www.emory.edu/EDUCATION/mfp/kuhnsyn.html) by Frank Pajares
- Article on Thomas Kuhn (http://plato.stanford.edu/entries/thomas-kuhn/) by Alexander Bird
- Rhetorical Analysis of chapter 3 (http://docs.google.com/Doc?id=ddmkz59v_657rh75) by Josh Thomas
- Text of chapter 9 and a postscript (http://www.marxists.org/reference/subject/philosophy/works/us/kuhn.htm) at Marxists.org
- "Thomas Kuhn, 73; Devised Science Paradigm" (http://www.des.emory.edu/mfp/kuhnobit.html), obituary by Lawrence Van Gelder, New York Times, 19 June 1996
In science, paradigm (ˈpærədatəm/) describes distinct concepts or thought patterns in any scientific discipline or other epistemological context.

Scientific paradigm

The Oxford English Dictionary defines the basic meaning of the term paradigm as "a pattern or model, an exemplar". The historian of science Thomas Kuhn gave it its contemporary meaning when he adopted the word to refer to the set of practices that define a scientific discipline at any particular period of time. In his book The Structure of Scientific Revolutions Kuhn defines a scientific paradigm as: "universally recognized scientific achievements that, for a time, provide model problems and solutions for a community of researchers", i.e.,

- what is to be observed and scrutinized
- the kind of questions that are supposed to be asked and probed for answers in relation to this subject
- how these questions are to be structured
- how the results of scientific investigations should be interpreted
- how is an experiment to be conducted, and what equipment is available to conduct the experiment.

In The Structure of Scientific Revolutions, Kuhn saw the sciences as going through alternating periods of normal science, when an existing model of reality dominates a protracted period of puzzle-solving, and revolution, when the model of reality itself undergoes sudden drastic change. Paradigms have two aspects. Firstly, within normal science, the term refers to the set of exemplary experiments that are likely to be copied or emulated. Secondly, underpinning this set of exemplars are shared preconceptions, made prior to – and conditioning – the collection of evidence. These preconceptions embody both hidden assumptions and elements that he describes as quasi-metaphysical; the interpretations of the paradigm may vary among individual scientists.

Kuhn was at pains to point out that the rationale for the choice of exemplars is a specific way of viewing reality: that view and the status of "exemplar" are mutually reinforcing. For well-integrated members of a particular discipline, its paradigm is so convincing that it normally renders even the possibility of alternatives unconvincing and counter-intuitive. Such a paradigm is opaque, appearing to be a direct view of the bedrock of reality itself, and obscuring the possibility that there might be other, alternative imageries hidden behind it. The conviction that the current paradigm is reality tends to disqualify evidence that might undermine the paradigm itself; this in turn leads to a build-up of unreconciled anomalies. It is the latter that is responsible for the eventual revolutionary overthrow of the incumbent paradigm, and its replacement by a new one. Kuhn used the expression paradigm shift (see below) for this process, and likened it to the perceptual change that occurs when our interpretation of an ambiguous image "flips-over" from one state to another. (The rabbit-duck illusion is an example: it is not possible to see both the rabbit and the duck simultaneously.) This is significant in relation to the issue of incommensurability (see below).

A currently accepted paradigm would be the standard model of physics. The scientific method would allow for orthodox scientific investigations into phenomena which might contradict or disprove the standard model; however grant funding would be proportionately more difficult to obtain for such experiments, depending on the degree of deviation from the accepted standard model theory which the experiment would be expected to test for. To illustrate the point, an experiment to test for the mass of neutrinos or the decay of protons (small departures from the model) would be more likely to receive money than experiments to look for the violation of the conservation of momentum, or ways to engineer reverse time travel.

Mechanisms similar to the original Kuhnian paradigm have been invoked in various disciplines other than the philosophy of science. These include: the idea of major cultural themes, worldviews (and see below), ideologies, and mindsets. They have somewhat similar meanings that apply to smaller and larger scale examples of disciplined thought. In addition, Michel Foucault used the terms episteme and discourse, mathesis and taxinomia, for aspects of
a "paradigm" in Kuhn's original sense.

**Paradigm shifts**

In *The Structure of Scientific Revolutions*, Kuhn wrote that "Successive transition from one paradigm to another via revolution is the usual developmental pattern of mature science." (p. 12)

Paradigm shifts tend to be most dramatic in sciences that appear to be stable and mature, as in physics at the end of the 19th century. At that time, a statement generally attributed to physicist Lord Kelvin famously claimed, "There is nothing new to be discovered in physics now. All that remains is more and more precise measurement." Five years later, Albert Einstein published his paper on special relativity, which challenged the very simple set of rules laid down by Newtonian mechanics, which had been used to describe force and motion for over two hundred years. In this case, the new paradigm reduces the old to a special case in the sense that Newtonian mechanics is still a good model for approximation for speeds that are slow compared to the speed of light. Philosophers and historians of science, including Kuhn himself, ultimately accepted a modified version of Kuhn's model, which synthesizes his original view with the gradualist model that preceded it. Kuhn's original model is now generally seen as too limited.

Kuhn's idea was itself revolutionary in its time, as it caused a major change in the way that academics talk about science. Thus, it could be argued that it caused or was itself part of a "paradigm shift" in the history and sociology of science. However, Kuhn would not recognize such a paradigm shift. Being in the social sciences, people can still use earlier ideas to discuss the history of science.

**Paradigm paralysis**

Perhaps the greatest barrier to a paradigm shift, in some cases, is the reality of paradigm paralysis: the inability or refusal to see beyond the current models of thinking. This is similar to what psychologists term Confirmation bias. Examples include rejection of Galileo's theory of a heliocentric universe, the discovery of electrostatic photography, xerography and the quartz clock.

**Incommensurability**

Kuhn pointed out that it could be difficult to assess whether a particular paradigm shift had actually led to progress, in the sense of explaining more facts, explaining more important facts, or providing better explanations, because the understanding of "more important", "better", etc. changed with the paradigm. The two versions of reality are thus *incommensurable*. Kuhn's version of incommensurability has an important psychological dimension; this is apparent from his analogy between a paradigm shift and the flip-over involved in some optical illusions. However, he subsequently diluted his commitment to incommensurability considerably, partly in the light of other studies of scientific development that did not involve revolutionary change. One of the examples that Kuhn used was the change in the style of chemical investigation that followed the work of Lavoisier on atomic theory in the late 18th Century as an example of incommensurability. In this change, the focus had shifted from the bulk properties of matter (such as hardness, colour, reactivity, etc.) to studies of atomic weights and quantitative studies of reactions. He suggested that it was impossible to make the comparison needed to judge which body of knowledge was better or more advanced. However, this change in research style (and paradigm) eventually (after more than a century) led to a theory of atomic structure that accounts well for the bulk properties of matter; see, for example, Brady's *General Chemistry*. This ability of science to back off, move sideways, and then advance is characteristic of the natural sciences, but contrasts with the position in some social sciences, notably economics.

This apparent ability does not guarantee that the account is veridical at anyone time, of course, and most modern philosophers of science are fallibilists. However, members of other disciplines do see the issue of incommensurability as a much greater obstacle to evaluations of "progress"; see, for example, Martin Slattery's *Key Ideas in Sociology*. [14][15]
Subsequent developments

Opaque Kuhnian paradigms and paradigm shifts do exist. A few years after the discovery of the mirror-neurons that provide a hard-wired basis for the human capacity for empathy, the scientists involved were unable to identify the incidents that had directed their attention to the issue. Over the course of the investigation, their language and metaphors had changed so that they themselves could no longer interpret all of their own earlier laboratory notes and records.\[16\]

Imre Lakatos and research programmes

However, many instances exist in which change in a discipline's core model of reality has happened in a more evolutionary manner, with individual scientists exploring the usefulness of alternatives in a way that would not be possible if they were constrained by a paradigm. Imre Lakatos suggested (as an alternative to Kuhn's formulation) that scientists actually work within research programmes.\[17\] In Lakatos' sense, a research programme is a sequence of problems, placed in order of priority. This set of priorities, and the associated set of preferred techniques, is the positive heuristic of a programme. Each programme also has a negative heuristic: this consists of a set of fundamental assumptions that — temporarily, at least — takes priority over observational evidence when the two appear to conflict.

This latter aspect of research programmes is inherited from Kuhn's work on paradigms, and represents an important departure from the elementary account of how science works. According to this, science proceeds through repeated cycles of observation, induction, hypothesis-testing, etc., with the test of consistency with empirical evidence being imposed at each stage. Paradigms and research programmes allow anomalies to be set aside, where there is reason to believe that they arise from incomplete knowledge (about either the substantive topic, or some aspect of the theories implicitly used in making observations).

Larry Laudan: Dormant anomalies, fading credibility, and research traditions

Larry Laudan\[18\] has also made two important contributions to the debate. Laudan believed that something akin to paradigms exist in the social sciences (Kuhn had contested this, see below); he referred to these as research traditions. Laudan noted that some anomalies become "dormant", if they survive a long period during which no competing alternative has shown itself capable of resolving the anomaly. He also presented cases in which a dominant paradigm had withered away because its lost credibility when viewed against changes in the wider intellectual milieu.

The concept of paradigm and the social sciences

Kuhn himself did not consider the concept of paradigm as appropriate for the social sciences. He explains in his preface to The Structure of Scientific Revolutions that he concocted the concept of paradigm precisely in order to distinguish the social from the natural sciences (p.x). He wrote this book at the Palo Alto Center for Scholars, surrounded by social scientists, when he observed that they were never in agreement on theories or concepts. He explains that he wrote this book precisely to show that there are no, nor can there be any, paradigms in the social sciences. Mattei Dogan, a French sociologist, in his article "Paradigms in the [Social Sciences]," develops Kuhn's original thesis that there are no paradigms at all in the social sciences since the concepts are polysemic, the deliberate mutual ignorance between scholars and the proliferation of schools in these disciplines. Dogan provides many examples of the non-existence of paradigms in the social sciences in his essay, particularly in sociology, political science and political anthropology.

However, both Kuhn's original work and Dogan's commentary are directed at disciplines that are defined by conventional labels (e.g., "sociology"). While it is true that such broad groupings in the social sciences are usually not based on a Kuhnian paradigm, each of the competing sub-disciplines may still be underpinned by a paradigm,
research programme, research tradition, and/or professional imagery. These structures will be motivating research, providing it with an agenda, defining what is and what is not anomalous evidence, and inhibiting debate with other groups that fall under the same broad disciplinary label. (A good example is provided by the contrast between Skinnerian behaviourism and Personal Construct Theory, PCT, within psychology. The most significant of the many ways in which these two sub-disciplines of psychology differ concerns meanings and intentions. In PCT, these are seen as the central concern of psychology; in behaviourism, they are not scientific evidence at all, because they cannot be directly observed.) These considerations explain the conflict between the Kuhn/Dogan view, and the views of others (including Larry Laudan, see above), who do apply these concepts to social sciences.

Handa [19] introduced the idea of "social paradigm" in the context of social sciences, and identified the basic components of a social paradigm. Like Kuhn, Handa addressed the issue of changing paradigm; the process popularly known as "paradigm shift". In this respect, he focused on social circumstances that precipitate such a shift and the effects of the shift on social institutions, including the institution of education. This broad shift in the social arena, in turn, changes the way the individual perceives reality. Another use of the word paradigm is in the sense of "worldview". For example, in social science, the term is used to describe the set of experiences, beliefs and values that affect the way an individual perceives reality and responds to that perception. Social scientists have adopted the Kuhnian phrase "paradigm shift" to denote a change in how a given society goes about organizing and understanding reality. A "dominant paradigm" refers to the values, or system of thought, in a society that are most standard and widely held at a given time. Dominant paradigms are shaped both by the community's cultural background and by the context of the historical moment. The following are conditions that facilitate a system of thought to become an accepted dominant paradigm: Professional organizations that give legitimacy to the paradigm; Dynamic leaders who introduce and purport the paradigm; Journals and editors who write about the paradigm; They both disseminate the information essential to the paradigm and give the paradigm legitimacy:

- Government agencies who give credence to the paradigm
- Educators who propagate the paradigm's ideas by teaching it to students
- Conferences conducted that are devoted to discussing ideas central to the paradigm
- Media coverage
- Lay groups, or groups based around the concerns of lay persons, that embrace the beliefs central to the paradigm
- Sources of funding to further research on the paradigm

**Etymology**

Paradigm comes from Greek ἄραδείγμα (paradeigma), "pattern, example, sample"[20] from the verb ἀραδείκηνομεν (paradeiknumi), "exhibit, represent, expose"[21] and that from ἀρα (par), "beside, beyond"[22] + ἔδεικνυμι (deiknumi), "to show, to point out" [23].

The original Greek term παράδειγμα (paradigma) was used in Greek texts such as Plato's Timaeus (28A) as the model or the pattern that the Demiurge (god) used to create the cosmos. The term had a technical meaning in the field of grammar: the 1900 Merriam-Webster dictionary defines its technical use only in the context of grammar or, in rhetoric, as a term for an illustrative parable or fable. In linguistics, Ferdinand de Saussure used paradigm to refer to a class of elements with similarities. The Merriam-Webster Online dictionary defines this usage as "a philosophical and theoretical framework of a scientific school or discipline within which theories, laws, and generalizations and the experiments performed in support of them are formulated; broadly: a philosophical or theoretical framework of any kind."[24]
Other uses

Handa, M.L. (1986) introduced the idea of "social paradigm" in the context of social sciences. He identified the basic components of a social paradigm. Like Kuhn, Handa addressed the issue of changing paradigm; the process popularly known as "paradigm shift". In this respect, he focused on social circumstances that precipitate such a shift and the effects of the shift on social institutions, including the institution of education. This broad shift in the social arena, in turn, changes the way the individual perceives reality.

Another use of the word paradigm is in the sense of "worldview". For example, in social science, the term is used to describe the set of experiences, beliefs and values that affect the way an individual perceives reality and responds to that perception. Social scientists have adopted the Kuhnian phrase "paradigm shift" to denote a change in how a given society goes about organizing and understanding reality. A "dominant paradigm" refers to the values, or system of thought, in a society that are most standard and widely held at a given time. Dominant paradigms are shaped both by the community's cultural background and by the context of the historical moment. The following are conditions that facilitate a system of thought to become an accepted dominant paradigm:

- Professional organizations that give legitimacy to the paradigm
- Dynamic leaders who introduce and purport the paradigm
- Journals and editors who write about the system of thought. They both disseminate the information essential to the paradigm and give the paradigm legitimacy
- Government agencies who give credence to the paradigm
- Educators who propagate the paradigm's ideas by teaching it to students
- Conferences conducted that are devoted to discussing ideas central to the paradigm
- Media coverage
- Lay groups, or groups based around the concerns of lay persons, that embrace the beliefs central to the paradigm
- Sources of funding to further research on the paradigm

The word paradigm is also still used to indicate a pattern or model or an outstandingly clear or typical example or archetype. The term is frequently used in this sense in the design professions. Design Paradigms or archetypes comprise functional precedents for design solutions. The best known references on design paradigms are Design Paradigms: A Sourcebook for Creative Visualization, by Wake, and Design Paradigms by Petroski.

This term is also used in cybernetics. Here it means (in a very wide sense) a (conceptual) protoprogram for reducing the chaotic mass to some form of order. Note the similarities to the concept of entropy in chemistry and physics. A paradigm there would be a sort of prohibition to proceed with any action that would increase the total entropy of the system. In order to create a paradigm, a closed system which would accept any changes is required. Thus a paradigm can be only applied to a system that is not in its final stage.

Notes

[7] The attribution of this statement to Lord Kelvin is given in a number of sources, but without citation. It is reputed to be Kelvin's remark made in an address to the British Association for the Advancement of Science in 1900. See the article on Lord Kelvin for additional details and references.
References and links

- Paradigm Presentation (http://www.funonthenet.in/articles/Paradigm-Presentation.html) An interesting look at how a Paradigm is created

[24] paradigm - Definition from the Merriam-Webster Online Dictionary (http://www.m-w.com/dictionary/paradigm)
Imre Lakatos (November 9, 1922 – February 2, 1974) was a Hungarian philosopher of mathematics and science, known for his thesis of the fallibility of mathematics and its 'methodology of proofs and refutations' in its pre-axiomatic stages of development, and also for introducing the concept of the 'research programme' in his methodology of scientific research programmes.

Life

Lakatos was born Imre (Avrum) Lipschitz to a Jewish family in Debrecen, Hungary in 1922. He received a degree in mathematics, physics, and philosophy from the University of Debrecen in 1944. He avoided Nazi persecution of Jews by changing his name to Imre Molnár. His mother and grandmother died in Auschwitz. He became an active communist during the Second World War. He changed his last name once again to Lakatos (Locksmith) in honor of Géza Lakatos.

After the war, from 1947 he worked as a senior official in the Hungarian ministry of education. He also continued his education with a PhD at Debrecen University awarded in 1948, and also attended György Lukács's weekly Wednesday afternoon private seminars. He also studied at the Moscow State University under the supervision of Sofya Yanovskaya in 1949. When he returned, however, he found himself on the losing side of internal arguments
within the Hungarian communist party and was imprisoned on charges of revisionism from 1950 to 1953. More of
Lakatos' activities in Hungary after World War II have recently become known.

After his release, Lakatos returned to academic life, doing mathematical research and translating George Pólya's
*How to Solve It* into Hungarian. Still nominally a communist, his political views had shifted markedly and he was
involved with at least one dissident student group in the lead-up to the 1956 Hungarian Revolution.

After the Soviet Union invaded Hungary in November 1956, Lakatos fled to Vienna, and later reached England. He
received a doctorate in philosophy in 1961 from the University of Cambridge. The book *Proofs and Refutations: The
Logic of Mathematical Discovery*, published after his death, is based on this work.

Lakatos never obtained British Citizenship. In 1960 he was appointed to a position in the London School of
Economics, where he wrote on the philosophy of mathematics and the philosophy of science. The LSE philosophy of
science department at that time included Karl Popper, Joseph Agassi and John Watkins. It was Agassi who first
introduced Lakatos to Popper under the rubric of his applying a fallibilist methodology of conjectures and refutations
to mathematics in his Cambridge PhD thesis.

With co-editor Alan Musgrave, he edited the highly-cited *Criticism and the Growth of Knowledge*, the *Proceedings*
of the International Colloquium in the Philosophy of Science, London, 1965. Published in 1970, the 1965
Colloquium included well-known speakers delivering papers in response to Thomas Kuhn's *"The Structure of
Scientific Revolutions"*.

Lakatos remained at the London School of Economics until his sudden death in 1974 of a brain haemorrhage, aged
just 51. The Lakatos Award was set up by the school in his memory.

In January 1971 he became editor of the internationally prestigious *British Journal for the Philosophy of Science*
until his death in 1974, after which it was then edited jointly for many years by his LSE colleagues John W. N.
Watkins and John Worrall, Lakatos's ex-research assistant.

His last LSE lectures in scientific method in Lent Term 1973 along with parts of his correspondence with his friend
and critic Paul Feyerabend have been published in *For and Against Method* (ISBN 0-226-46774-0).

Lakatos and his colleague Spiro Latsis organised an international conference devoted entirely to historical case
studies in Lakatos's methodology of research programmes in physical sciences and economics, to be held in Greece
in 1974, and which still went ahead following Lakatos’s death in February 1974. These case studies in such as
Einstein's relativity programme, Fresnel's wave theory of light and neoclassical economics, were published by
Cambridge University Press in two separate volumes in 1976, one devoted to physical sciences and Lakatos's general
programme for rewriting the history of science, with a concluding critique by his great friend Paul Feyerabend, and
the other devoted to economics.[2]

**Proofs and refutations, mathematics**

Lakatos' philosophy of mathematics was inspired by both Hegel's and Marx' dialectic, by Karl Popper's theory of
knowledge, and by the work of mathematician George Polya.

The 1976 book *Proofs and Refutations* is based on the first three chapters of his four chapter 1961 doctoral thesis
*Essays in the logic of mathematical discovery*. But its first chapter is Lakatos's own revision of its chapter 1 that was
first published as *Proofs and Refutations* in four parts in 1963-4 in *The British Journal for the Philosophy of Science*.
It is largely taken up by a fictional dialogue set in a mathematics class. The students are attempting to prove
the formula for the Euler characteristic in algebraic topology, which is a theorem about the properties of polyhedra,
namely that for all polyhedra the number of their (V)ertices minus the number of their (E)dges plus the number of their (F)aces is 2: \( V - E + F = 2 \). The dialogue is meant to represent the actual series of attempted proofs which mathematicians historically offered for the conjecture, only to be repeatedly refuted by counterexamples. Often the students paraphrase famous mathematicians such as Cauchy, as noted in Lakatos's extensive footnotes.
What Lakatos tried to establish was that no theorem of informal mathematics is final or perfect. This means that we should not think that a theorem is ultimately true, only that no counterexample has yet been found. Once a counterexample, i.e. an entity contradicting/not explained by the theorem is found, we adjust the theorem, possibly extending the domain of its validity. This is a continuous way our knowledge accumulates, through the logic and process of proofs and refutations. (If axioms are given for a branch of mathematics, however, Lakatos claimed that proofs from those axioms were tautological, i.e. logically true.)

Lakatos proposed an account of mathematical knowledge based on the idea of heuristics. In *Proofs and Refutations* the concept of 'heuristic' was not well developed, although Lakatos gave several basic rules for finding proofs and counterexamples to conjectures. He thought that mathematical 'thought experiments' are a valid way to discover mathematical conjectures and proofs, and sometimes called his philosophy 'quasi-empiricism'. However, he also conceived of the mathematical community as carrying on a kind of dialectic to decide which mathematical proofs are valid and which are not. Therefore he fundamentally disagreed with the 'formalist' conception of proof which prevailed in Frege's and Russell's logicism, which defines proof simply in terms of formal validity.

On its first publication as a paper in *The British Journal for the Philosophy of Science* in 1963-4, *Proofs and Refutations* became highly influential on new work in the philosophy of mathematics, although few agreed with Lakatos' strong disapproval of formal proof. Before his death he had been planning to return to the philosophy of mathematics and apply his theory of research programmes to it. Lakatos, Worrall and Zahar use Poincaré (1893) to answer one of the major problems perceived by critics, namely that the pattern of mathematical research depicted in *Proofs and Refutations* does not faithfully represent most of the actual activity of contemporary mathematicians.

**Cauchy and uniform convergence**

In a 1966 text published as (Lakatos 1978), Lakatos re-examines the history of the calculus, with special regard to Augustin-Louis Cauchy and the concept of uniform convergence, in the light of non-standard analysis. Lakatos is concerned that historians of mathematics should not judge the evolution of mathematics in terms of currently fashionable theories. As an illustration, he examines Cauchy's proof that the sum of a series of continuous functions is itself continuous. Lakatos is critical of those who would see Cauchy's proof, with its failure to make explicit a suitable convergence hypothesis, merely as an inadequate approach to Weierstrassian analysis. Lakatos sees in such an approach a failure to realize that Cauchy's concept of the continuum differed from currently dominant views.

**Research programmes**

Lakatos' second major contribution to the philosophy of science was his model of the 'research programme', which he formulated in an attempt to resolve the perceived conflict between Popper's falsificationism and the revolutionary structure of science described by Kuhn. Popper's standard of falsificationism was widely taken to imply that a theory should be abandoned as soon as any evidence appears to challenge it, while Kuhn's descriptions of scientific activity were taken to imply that science was most constructive when it upheld a system of popular, or 'normal', theories, despite anomalies. Lakatos' model of the research programme aims to combine Popper's adherence to empirical validity with Kuhn's appreciation for conventional consistency.

A Lakatosian research programme is based on a hard core of theoretical assumptions that cannot be abandoned or altered without abandoning the programme altogether. More modest and specific theories that are formulated in order to explain evidence that threatens the 'hard core' are termed auxiliary hypotheses. Auxiliary hypotheses are considered expendable by the adherents of the research programme - they may be altered or abandoned as empirical discoveries require in order to 'protect' the 'hard core'. Whereas Popper was generally read as hostile toward such ad hoc theoretical amendments, Lakatos argued that they can be progressive, i.e. productive, when they enhance the programme's explanatory and/or predictive power, and that they are at least permissible until some better system of theories is devised and the research programme is replaced entirely. The difference between a progressive and a
*Degenerative* research programme lies, for Lakatos, in whether the recent changes to its auxiliary hypotheses have achieved this greater explanatory/predictive power or whether they have been made simply out of the necessity of offering some response in the face of new and troublesome evidence. A degenerative research programme indicates that a new and more progressive system of theories should be sought to replace the currently prevailing one, but until such a system of theories can be conceived of and agreed upon, abandonment of the current one would only further weaken our explanatory power and was therefore unacceptable for Lakatos. Lakatos' primary example of a research programme that had been successful in its time and then progressively replaced is that founded by Isaac Newton, with his three laws of motion forming the 'hard core'.

The Lakatosian research programme deliberately provides a framework within which research can be conducted on the basis of 'first principles' (the 'hard core') which are shared by those involved in the research programme and accepted for the purpose of that research without further proof or debate. In this regard, it is similar to Kuhn's notion of a paradigm. Lakatos sought to replace Kuhn's paradigm, guided by an irrational 'psychology of discovery', with a research programme no less coherent or consistent yet guided by Popper's objectively valid logic of discovery.

Lakatos was following Pierre Duhem's idea that one can always protect a cherished theory (or part of one) from hostile evidence by redirecting the criticism toward other theories or parts thereof. (See Confirmation holism and Duhem-Quine thesis). This difficulty with falsificationism had been acknowledged by Popper.

Popper's theory, Falsificationism, proposed that scientists put forward theories and that nature 'shouts NO' in the form of an inconsistent observation. According to Popper, it is irrational for scientists to maintain their theories in the face of Nature's rejection, as Kuhn had described them doing. For Lakatos, however, "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". [5]

The continued adherence to a programme's 'hard core', augmented with adaptable auxiliary hypotheses, reflects Lakatos' less strict standard of falsificationism.

Lakatos saw himself as merely extending Popper's ideas, which changed over time and were interpreted by many in conflicting ways. He contrasted *Popper*, the "naive falsificationist" who demanded unconditional rejection of any theory in the face of any anomaly (an interpretation Lakatos saw as erroneous but that he nevertheless referred to often); *Popper1*, the more nuanced and conservatively interpreted philosopher; and *Popper2*, the "sophisticated methodological falsificationist" that Lakatos claims is the logical extension of the correctly interpreted ideas of *Popper1* (and who is therefore essentially Lakatos himself). It is, therefore, very difficult to determine which ideas and arguments concerning the research programme should be credited to whom.

While Lakatos dubbed his theory "sophisticated methodological falsificationism", it is not "methodological" in the strict sense of asserting universal methodological rules to which all scientific research must abide by. Rather, it is methodological only in that theories are only abandoned according to a methodical progression from worse theories to better theories - a stipulation overlooked by what Lakatos terms "dogmatic falsificationism". Methodological assertions in the strict sense, pertaining to which methods are valid and which are invalid, are, themselves, contained within the research programmes that choose to adhere to them, and should be judged according to whether the research programmes that adhere to them prove progressive or degenerative. Lakatos divided these 'methodological rules' within a research programme into its 'negative heuristics', IE what research methods and approaches to avoid, and its 'positive heuristics', IE what research methods and approaches to prefer.

Lakatos claimed that not all changes of the auxiliary hypotheses of a research programme (which he calls 'problem shifts') are equally productive or acceptable. He took the view that these 'problem shifts' should be evaluated not just by their ability to defend the 'hard core' by explaining apparent anomalies, but also by their ability to produce new facts, in the form of predictions or additional explanations. [6] Adjustments that accomplish nothing more than the maintenance of the 'hard core' mark the research programme as degenerative.

Lakatos' model provides for the possibility of a research programme that is not only continued in the presence of troublesome anomalies but that remains progressive despite them. For Lakatos, it is essentially necessary to continue on with a theory that we basically know cannot be completely true, and it is even possible to make scientific progress
in doing so, as long as we remain receptive to a better research programme that may eventually be conceived of. In this sense, it is, for Lakatos, an acknowledged misnomer to refer to 'falsification' or 'refutation', when it is not the truth or falsity of a theory that is solely determining whether we consider it 'falsified', but also the availability of a less false theory. A theory cannot be rightfully 'falsified', according to Lakatos, until it is superseded by a better (i.e. more progressive) research programme. This is what he says is happening in the historical periods Kuhn describes as revolutions and what makes them rational as opposed to mere leaps of faith or periods of deranged social psychology, as Kuhn argued.

**Pseudoscience**

According to the demarcation criterion of pseudoscience originally proposed by Lakatos, a theory is pseudoscientific if it fails to make any novel predictions of previously unknown phenomena, in contrast with scientific theories, which predict novel fact(s). Progressive scientific theories are those which have their novel facts confirmed and degenerate scientific theories are those whose predictions of novel facts are refuted. As he put it:

"A given fact is explained scientifically only if a new fact is predicted with it....The idea of growth and the concept of empirical character are soldered into one." See pages 34–5 of *The Methodology of Scientific Research Programmes*, 1978.

Lakatos's own key examples of pseudoscience were Ptolemaic astronomy, Immanuel Velikovsky's planetary cosmogony, Freudian psychoanalysis, 20th century Soviet Marxism, Lysenko's biology, Niels Bohr's Quantum Mechanics post-1924, astrology, psychiatry, sociology, neoclassical economics, and Darwin's theory.

**Darwin's theory**

In his 1973 LSE Scientific Method Lecture 1 he also claimed that "nobody to date has yet found a demarcation criterion according to which Darwin can be described as scientific".

Almost 20 years after Lakatos's 1973 'challenge' on the scientificity of Darwin, in her 1991 *The Ant and the Peacock* (pp31–2), LSE lecturer and ex-colleague of Lakatos, Helena Cronin, attempted to establish that Darwinian theory was empirically scientific in respect of at least being supported by evidence of likeness in the diversity of life forms in the world, allegedly explained by descent with modification. She concluded that "our usual idea of corroboration as requiring the successful prediction of novel facts...Darwinian theory was not strong on temporally novel predictions". She was equivocal about whether it did or did not make any novel predictions, only saying "*For the most part* this evidence was already well known, thoroughly documented by pre-Darwinian natural history.[Italics added]". Cronin did not state what other part of the evidence was not already well known, but did then assert that it was scientific on the weaker Zahar criterion of providing independent novel explanation of old already well known facts. However, she failed to demonstrate that it provided any confirmed nomological-deductive explanation of any old facts of likeness within evolutionary diversity, making an assertion that it did so, without proof.

**The Milton Friedman neoclassical economics case study**

In August 1972, a case study of the methodology of neoclassical economics by Lakatos's London School of Economics colleague Spiro Latsis published in *The British Journal for the Philosophy of Science* found Milton Friedman's methodology to be 'pseudo-scientific' in terms of Lakatos's evaluative philosophy of science, according to which the demarcation between scientific and pseudo-scientific theories consists of their at least predicting testable empirical novel facts or not. Latsis claimed that Friedman's instrumentalist methodology of neoclassical economics had never predicted any novel facts. In defense, Friedman wrote a three-page letter to Latsis in December 1972, counter-claiming that the neoclassical monopoly competition model had in fact shown empirical progress by predicting phenomena not previously observed that were also subsequently confirmed by empirical evidence. The example he gave was a prediction of
Chamberlain's monopolistic competition model that "the standard explanation for the Standard Oil monopoly was wrong", which he said had been theoretically predicted by Aaron Director, his brother-in-law, and empirically confirmed by Magee. Lakatos invited Friedman to submit a discussion note based on his December 1972 letter to Latsis for publication in a symposium on the issue of the scientific status or not of neoclassical economics,[13] but Friedman never took up the invitation.

Three years later, in 1976, Friedman was awarded the Nobel Prize for Economics "for his achievements in the fields of consumption analysis, monetary history and theory and for his demonstration of the complexity of stabilization policy".[14] Friedman's own predictions of an accelerating rate of inflation due to attempts to use expansionary monetary policy in order to attain an unrealistic employment target, as described in his Nobel lecture[15] are cited by others as an example of a novel phenomenon successfully predicted by neoclassical economics.[16] This research ultimately led to a break down of the popular belief in economics in the mid 20th century that there was a long-run trade-off between unemployment and inflation. Robert E. Lucas argued that the Friedman-Phelps model was "as clear cut an experimental distinction as macroeconomics is ever likely to see". Roger Backhouse argued that Friedman and Phelps had predicted novel facts that were corroborated by the events of the 1970s. Mark Blaug argued that Friedman's 1968 paper and its successful prediction of novel facts was itself proof that Friedman's monetarist neoclassical research programme was a progressive research programme.[17]

**Historiographical research programmes**

In his 1973 monograph *History of Science and Its Rational Reconstructions*[18] Lakatos proposed a dialectical historiographical meta-method for evaluating different theories of scientific method, namely by means of their comparative success in explaining the actual history of science and scientific revolutions on the one hand, whilst on the other providing a historiographical framework for rationally reconstructing the history of science as anything more than merely inconsequential rambling. The paper started with his now renowned dictum "Philosophy of science without history of science is empty; history of science without philosophy of science is blind."

However neither Lakatos himself nor his collaborators ever completed the first part of this dictum by showing that in any scientific revolution the great majority of the relevant scientific community converted just when Lakatos's criterion – one programme successfully predicting some novel facts whilst its competitor degenerated - was satisfied. Indeed for the historical case studies in his 1970 *Criticism and the Methodology of Scientific Research Programmes* he had openly admitted as much, commenting 'In this paper it is not my purpose to go on seriously to the second stage of comparing rational reconstructions with actual history for any lack of historicity.'

**Criticism**

**Feyerabend**

Paul Feyerabend argued that Lakatos's methodology was not a methodology at all, but merely "words that *sound* like the elements of a methodology."[19] He argued that Lakatos's methodology was no different in practice from epistemological anarchism, Feyerabend's own position. He wrote in *Science in a Free Society* (after Lakatos's death) that:

Lakatos realized and admitted that the existing standards of rationality, standards of logic included, were too restrictive and would have hindered science had they been applied with determination. He therefore permitted the scientist to violate them (he admits that science is not "rational" in the sense of these standards). However, he demanded that research programmes show certain features *in the long run* — they must be progressive.... I have argued that this demand no longer restricts scientific practice. Any development agrees with it.[20]

Lakatos and Feyerabend planned to produce a joint work in which Lakatos would develop a rationalist description of science and Feyerabend would attack it.
Notes

[2] These were respectively Method and Appraisal in the Physical Sciences: The Critical Background to Modern Science 1800-1905 Colin Howson (Ed) and Method and Appraisal in Economics Spiro J. Latsis (Ed)
[5] Lakatos, Musgrave ed. (1970), Pg. 130
[6] Theoretical progressiveness is if the new 'theory has more empirical content than the old. Empirical progressiveness is if some of this content is corroborated. (Lakatos ed., 1970, P.118)
[7] See/hear Lakatos's 1973 Open University BBC Radio talk Science and Pseudoscience at his LSE website @ www.lse.ac.uk/lakatos
[8] Lakatos notably only condemned specifically Soviet Marxism as pseudoscientific, as opposed to Marxism in general. In fact at the very end of his very last LSE lectures on Scientific Method in 1973, he finished by posing the question of whether Trotsky's theoretical development of Marxism was scientific, and commented that "Nobody has ever undertaken a critical history of Marxism with the aid of better methodological and historiographical instruments. Nobody has ever tried to find an answer to questions like: were Trotsky's utopian and impossible predictions, impossible? Could they have been right, or might they be right if the historical circumstances are changed?" and that "I am not aware of any such attempts to think in these terms."
[9] Published in For and Against Method: Imre Lakatos and Paul Feyerabend Motterlini (Ed) University of Chicago Press 1999
[11] Lakatos's LSE colleague, the econometrician and now Labour parliamentary peer Baron Meighen Desai, evaluated Marxist economics as a progressive scientific research programme in both his 1974 Marxian Economics and its 1979 second edition. But he did not identify any successfully predicted novel fact(s) that had rendered it progressive science. However, in 1976 Feyerabend claimed that contrary to John Worrall's 1975 repetition of Lakatos's claim that Marxism's auxiliary hypotheses to eliminate refuting instances were not content increasing, that in fact such as its theory of imperialism and colonial exploitation to explain the apparent refutation of its law of the falling rate of profit was "accompanied by a wealth of novel predictions (the arrival and structure of monopolies being one of them)." And he continued by claiming both Rosa Luxumburg's and Trotsky's developments of Marxism in his auxiliary theory of colonial exploitation had been 'Lakatos scientific' because it was "accompanied by a wealth of novel predictions (the arrival and structure of monopolies being one of them)."
[12] His 6 December 1972 letter is held in Folder 36 of Box 29 of the Hoover Institution Archives' Milton Friedman Papers (http://www.oac.cdlib.org/findaid/ark:/13030/tf1tnb2hx) at Stanford University, created by Friedman and his wife in 1992. This Folder also contains a two-page reply to Friedman's letter by Latsis dated 27 January 1973
[13] Lakatos's 2 February 1973 letter of invitation is held in Folder 32 of Box 29.
References

- Oxford Dictionary of National Biography
- Zahar, Elie (1973) *Why Einstein's programme superseded Lorentz's* 'British Journal for the Philosophy of Science'

Archives

Imre Lakatos's papers (http://archives.lse.ac.uk/TreeBrowse.aspx?src=CalmView. Catalog&field=RefNo&key=LAKATOS) are held at the London School of Economics. His personal library (http://web.archive.org/web/20080105202458/http://www.lse.ac.uk/library/whinthli/Lakatos.htm) is also held at the School.

Further information

External links

- *Science and Pseudoscience* (http://www.lse.ac.uk/collections/lakatos/scienceAndPseudoscience.htm) — Lakatos' 1973 Open University BBC Radio talk on the subject
- O'Connor, John J.; Robertson, Edmund F., "Imre Lakatos" (http://www-history.mcs.st-andrews.ac.uk/Biographies/Lakatos.html), *MacTutor History of Mathematics archive*, University of St Andrews.
- Lakatos’s Hungarian intellectual background (http://muse.jhu.edu/journals/perspectives_on_science/toc/posc14.3.html) The Autumn 2006 MIT Press journal *Perspectives on Science* devoted to articles on this topic, with article abstracts.
- Economic research programme (http://www.paciolicode.com)
- Official russian page (http://lakatos.ru)

See also

- Scientific Community Metaphor, an approach to programming influenced by Lakatos's work on research programmes.
- List of Eastern Bloc defectors
- Lakatos Award set up in memory of him
Paul Feyerabend

Paul Feyerabend

Born January 13, 1924
Vienna, Austria

Died February 11, 1994 (aged 70)
Genolier, Vaud, Switzerland

Era 20th-century philosophy

Region Western Philosophy

School Epistemological Anarchism

Main interests Philosophy of science, Critiquing Falsificationism, Epistemology, Politics

Notable ideas Epistemological anarchism

Paul Karl Feyerabend (January 13, 1924 – February 11, 1994) was an Austrian-born philosopher of science best known for his work as a professor of philosophy at the University of California, Berkeley, where he worked for three decades (1958–1989). He lived a peripatetic life, living at various times in England, the United States, New Zealand, Italy, Germany, and finally Switzerland. His major works include Against Method (published in 1975), Science in a Free Society (published in 1978) and Farewell to Reason (a collection of papers published in 1987). Feyerabend became famous for his purportedly anarchistic view of science and his rejection of the existence of universal methodological rules.[1] He is an influential figure in the philosophy of science, and also in the sociology of scientific knowledge.

Biography

Early life

Paul Feyerabend was born in 1924 in Vienna, where he attended primary school and high school. In this period he got into the habit of frequent reading, developed an interest in theatre, and started singing lessons. When he graduated from high school in April 1942, he was drafted into the German Arbeitsdienst. After basic training in Pirmasens, Germany, he was assigned to a unit in Quelern en Bas, near Brest (France). Feyerabend described the work he did during that period as monotonous: “we moved around in the countryside, dug ditches, and filled them up again.” After a short leave, he joined the army and volunteered for officer school. In his autobiography, he wrote that
he hoped the war would be over by the time he had finished his education as an officer. This turned out not to be the case. From December 1943 on, he served as an officer on the northern part of the Eastern Front, was decorated with an Iron cross, and attained the rank of lieutenant. After the German army started its retreat from the advancing Red army, Feyerabend was hit by three bullets while directing traffic. It turned out that one of the bullets had hit him in the spine. As a consequence of this, he needed to walk with a stick for the rest of his life and frequently experienced severe pains. He spent the rest of the war recovering from his wounds.

Post–WWII and university
When the war was over, Feyerabend first got a temporary job in Apolda where he wrote pieces for the theatre. He was influenced by the Marxist playwright Bertolt Brecht and was invited by Brecht to be his assistant at the East Berlin State Opera but turned down the offer. Feyerabend took various classes at the Weimar Academy, and returned to Vienna to study history and sociology. He became dissatisfied, however, and soon transferred to physics, where he met Felix Ehrenhaft, a physicist whose experiments would influence his later views on the nature of science. Feyerabend changed the subject of his study to philosophy and submitted his final thesis on observation sentences. In his autobiography, he described his philosophical views during this time as “staunchly empiricist”. In 1948 he visited the first meeting of the international summer seminar of the Austrian College Society in Alpbach. This was the place where Feyerabend first met Karl Popper, who had a "positive" (early Popper), as well as "negative" (later Popper) effect on him. In 1949 he was a founding member of the Kraft Circle. In 1951, Feyerabend was granted a British Council scholarship to study under Wittgenstein. However, Wittgenstein died before Feyerabend moved to England. Feyerabend then chose Popper as his supervisor instead, and went to study at the London School of Economics in 1952. In his autobiography, Feyerabend explains that during this time, he was influenced by Popper: "I had fallen for [Popper's ideas]". After that, Feyerabend returned to Vienna and was involved in various projects; a translation of Karl Popper's *Open Society and its Enemies*, hunting down manuscripts Popper had left in Vienna, a report on the development of the humanities in Austria, and several articles for an encyclopedia.

Academia
In 1955, Feyerabend received his first academic appointment at the University of Bristol, England, where he gave lectures about the philosophy of science. Later in his life he worked as a professor (or equivalent) at Berkeley, Auckland, Sussex, Yale, London, Berlin and ETH Zurich. During this time, he developed a critical view of science, which he later described as 'anarchistic' or 'dadaistic' to illustrate his rejection of the dogmatic use of rules, a position incompatible with the contemporary rationalistic culture in the philosophy of science. At the London School of Economics, Feyerabend met a colleague of K.R. Popper, Imre Lakatos with whom he planned to write a dialogue volume in which Lakatos would defend a rationalist view of science and Feyerabend would attack it. This planned joint publication was put to an end by Lakatos's sudden death in 1974. *Against Method* became a famous criticism of current philosophical views of science and provoked many reactions. In his autobiography, he reveals that the energy in his writings came at great cost to himself:

> The depression stayed with me for over a year; it was like an animal, a well-defined, spatially localizable thing. I would wake up, open my eyes, listen – Is it here or isn’t? No sign of it. Perhaps it's asleep. Perhaps it will leave me alone today. Carefully, very carefully, I get out of bed. All is quiet. I go to the kitchen, start breakfast. Not a sound. TV – *Good Morning America* –, David What's-his-name, a guy I can't stand. I eat and watch the guests. Slowly the food fills my stomach and gives me strength. Now a quick excursion to the bathroom, and out for my morning walk – and here she is, my faithful depression: "Did you think you could leave without me?"

—From his autobiography, *Killing Time*
Feyerabend moved to the University of California, Berkeley in California in 1958 and became a U.S. citizen. Following (visiting) professorships (or their equivalent) at University College London, Berlin, and Yale, he taught at the University of Auckland, New Zealand in 1972 and 1974, always returning to California. He later enjoyed alternating between posts at ETH Zurich and Berkeley through the 1980s but left Berkeley for good in October 1989, first to Italy, then finally to Zurich. After his retirement in 1991, Feyerabend continued to publish frequent papers and worked on his autobiography.

After a short period of suffering from a brain tumor, he died in 1994 at the Genolier Clinic, overlooking Lake Geneva, Switzerland.

**Thought**

**Philosophy of science**

**Nature of scientific method**

In his books *Against Method* and *Science in a Free Society* Feyerabend defended the idea that there are no methodological rules which are always used by scientists. He objected to any single prescriptive scientific method on the grounds that any such method would limit the activities of scientists, and hence restrict scientific progress. In his view, science would benefit most from a "dose" of theoretical anarchism. He also thought that theoretical anarchism was desirable because it was more *humanitarian* than other systems of organization, by not imposing rigid rules on scientists.

For is it not possible that science as we know it today, or a "search for the truth" in the style of traditional philosophy, will create a monster? Is it not possible that an objective approach that frowns upon personal connections between the entities examined will harm people, turn them into miserable, unfriendly, self-righteous mechanisms without charm or humour? "Is it not possible," asks Kierkegaard, "that my activity as an objective [or critico-rational] observer of nature will weaken my strength as a human being?" I suspect the answer to many of these questions is affirmative and I believe that a reform of the sciences that makes them more anarchic and more subjective (in Kierkegaard's sense) is urgently needed. *Against Method*. p. 154.

Feyerabend's position was originally seen as radical in the philosophy of science, because it implies that philosophy can neither succeed in providing a general description of science, nor in devising a method for differentiating products of science from non-scientific entities like myths. (Feyerabend's position also implies that philosophical guidelines should be ignored by scientists, if they are to aim for progress.)

To support his position that methodological rules generally do not contribute to scientific success, Feyerabend provides counterexamples to the claim that (good) science operates according to a certain fixed method. He took some examples of episodes in science that are generally regarded as indisputable instances of progress (e.g. the Copernican revolution), and showed that all common prescriptive rules of science are violated in such circumstances. Moreover, he claimed that applying such rules in these historical situations would actually have prevented scientific revolution.

One of the criteria for evaluating scientific theories that Feyerabend attacks is the *consistency criterion*. He points out that to insist that new theories be consistent with old theories gives an unreasonable advantage to the older theory. He makes the logical point that being compatible with a defunct older theory does not increase the validity or truth of a new theory over an alternative covering the same content. That is, if one had to choose between two theories of equal explanatory power, to choose the one that is compatible with an older, falsified theory is to make an aesthetic, rather than a rational choice. The familiarity of such a theory might also make it more appealing to
scientists, since they will not have to disregard as many cherished prejudices. Hence, that theory can be said to have "an unfair advantage".

Feyerabend was also critical of falsificationism. He argued that no interesting theory is ever consistent with all the relevant facts. This would rule out using a naïve falsificationist rule which says that scientific theories should be rejected if they do not agree with known facts. Feyerabend uses several examples, but "renormalization" in quantum mechanics provides an example of his intentionally provocative style: "This procedure consists in crossing out the results of certain calculations and replacing them by a description of what is actually observed. Thus one admits, implicitly, that the theory is in trouble while formulating it in a manner suggesting that a new principle has been discovered" Against Method, p. 61. Such jokes are not intended as a criticism of the practice of scientists. Feyerabend is not advocating that scientists do not make use of renormalization or other ad hoc methods. Instead, he is arguing that such methods are essential to the progress of science for several reasons. One of these reasons is that progress in science is uneven. For instance, in the time of Galileo, optical theory could not account for phenomena that were observed by means of telescopes. So, astronomers who used telescopic observation had to use ad hoc rules until they could justify their assumptions by means of optical theory.

Feyerabend was critical of any guideline that aimed to judge the quality of scientific theories by comparing them to known facts. He thought that previous theory might influence natural interpretations of observed phenomena. Scientists necessarily make implicit assumptions when comparing scientific theories to facts that they observe. Such assumptions need to be changed in order to make the new theory compatible with observations. The main example of the influence of natural interpretations that Feyerabend provided was the tower argument. The tower argument was one of the main objections against the theory of a moving earth. Aristotelians assumed that the fact that a stone which is dropped from a tower lands directly beneath it shows that the earth is stationary. They thought that, if the earth moved while the stone was falling, the stone would have been "left behind". Objects would fall diagonally instead of vertically. Since this does not happen, Aristotelians thought that it was evident that the earth did not move. If one uses ancient theories of impulse and relative motion, the Copernican theory indeed appears to be falsified by the fact that objects fall vertically on earth. This observation required a new interpretation to make it compatible with Copernican theory. Galileo was able to make such a change about the nature of impulse and relative motion. Before such theories were articulated, Galileo had to make use of ad hoc methods and proceed counterinductively. So, "ad hoc" hypotheses actually have a positive function: they temporarily make a new theory compatible with facts until the theory to be defended can be supported by other theories.

Feyerabend commented on the Galileo affair as follows:

The church at the time of Galileo was much more faithful to reason than Galileo himself, and also took into consideration the ethical and social consequences of Galileo's doctrine. Its verdict against Galileo was rational and just, and revisionism can be legitimized solely for motives of political opportunism. \[2\][3][4]

Together these remarks sanction the introduction of theories that are inconsistent with well-established facts. Furthermore, a pluralistic methodology that involves making comparisons between any theories at all forces defendants to improve the articulation of each theory. In this way, scientific pluralism improves the critical power of science. Pope Benedict XVI has cited Feyerabend to this effect.\[5\]

According to Feyerabend, new theories to be accepted not because of their accord with scientific method, but because their supporters made use of any trick – rational, rhetorical or ribald – in order to advance their cause. Without a fixed ideology, or the introduction of religious tendencies, the only approach which does not inhibit progress (using whichever definition one sees fit) is "anything goes": "anything goes' is not a 'principle' I hold... but the terrified exclamation of a rationalist who takes a closer look at history." (Feyerabend, 1975).

Feyerabend considered the possibility of incommensurability, but he was hesitant in his application of the concept. He wrote that "it is hardly ever possible to give an explicit definition of [incommensurability]" Against Method, p. 225., because it involves covert classifications and major conceptual changes. He also was critical of attempts to capture incommensurability in a logical framework, since he thought of incommensurability as a phenomenon.
outside the domain of logic. In the second appendix of Against Method (p. 114), Feyerabend states, "I never said... that any two rival theories are incommensurable... What I did say was that certain rival theories, so-called 'universal' theories, or 'non-instantial' theories, if interpreted in a certain way, could not be compared easily." Incommensurability did not concern Feyerabend greatly, because he believed that even when theories are commensurable (i.e. can be compared), the outcome of the comparison should not necessarily rule out either theory. To rephrase: when theories are incommensurable, they cannot rule each other out, and when theories are commensurable, they cannot rule each other out. Assessments of (in)commensurability, therefore, don't have much effect in Feyerabend's system, and can be more or less passed over in silence.

In Against Method Feyerabend claimed that Imre Lakatos's philosophy of research programmes is actually "anarchism in disguise", because it does not issue orders to scientists. Feyerabend playfully dedicated Against Method to "Imre Lakatos: Friend, and fellow-anarchist". One interpretation is that Lakatos's philosophy of mathematics and science was based on creative transformations of Hegelian historiographic ideas, many associated with Lakatos's teacher in Hungary Georg Lukács. Feyerabend's debate with Lakatos on scientific method recapitulates the debate of Lukács and (Feyerabend's would-be mentor) Brecht, over aesthetics several decades earlier.

It should be borne in mind that while Feyerabend described himself as an 'epistemological anarchist', he explicitly disavowed being a 'political anarchist', and this perspective on Feyerabend has been shared by other authors offering anarchist critiques of science. However, it has been argued by some that political anarchism is tacitly embedded in Feyerabend's assessment of the proper role of science and its practice within society.

The decline of the physicist-philosopher

Feyerabend was critical of the lack of knowledge of philosophy shown by the generation of physicists that emerged after World War II:

The withdrawal of philosophy into a "professional" shell of its own has had disastrous consequences. The younger generation of physicists, the Feynmans, the Schwingers, etc., may be very bright; they may be more intelligent than their predecessors, than Bohr, Einstein, Schrödinger, Boltzmann, Mach and so on. But they are uncivilized savages, they lack in philosophical depth – and this is the fault of the very same idea of professionalism which you are now defending.

On the other hand, Feyerabend was himself heavily criticized for his misrepresentation of the practices, methods and goals of some of the above-mentioned scientists, esp. Mach and Einstein.

Role of science in society

Feyerabend described science as being essentially anarchistic, obsessed with its own mythology, and as making claims to truth well beyond its actual capacity. He was especially indignant about the condescending attitudes of many scientists towards alternative traditions. For example, he thought that negative opinions about astrology and the effectivity of rain dances were not justified by scientific research, and dismissed the predominantly negative attitudes of scientists towards such phenomena as elitist or racist. In his opinion, science has become a repressing ideology, even though it arguably started as a liberating movement. Feyerabend thought that a pluralistic society should be protected from being influenced too much by science, just as it is protected from other ideologies.

Starting from the argument that a historical universal scientific method does not exist, Feyerabend argues that science does not deserve its privileged status in western society. Since scientific points of view do not arise from using a universal method which guarantees high quality conclusions, he thought that there is no justification for valuing scientific claims over claims by other ideologies like religions. Feyerabend also argued that scientific accomplishments such as the moon landings are no compelling reason to give science a special status. In his opinion, it is not fair to use scientific assumptions about which problems are worth solving in order to judge the merit of other ideologies. Additionally, success by scientists has traditionally involved non-scientific elements, such as inspiration.
from mythical or religious sources.

Based on these arguments, Feyerabend defended the idea that science should be separated from the state in the same way that religion and state are separated in a modern secular society (Against Method (3rd ed.). p. 160.). He envisioned a "free society" in which "all traditions have equal rights and equal access to the centres of power" (Science in a Free Society. p. 9.). For example, parents should be able to determine the ideological context of their children's education, instead of having limited options because of scientific standards. According to Feyerabend, science should also be subjected to democratic control: not only should the subjects that are investigated by scientists be determined by popular election, scientific assumptions and conclusions should also be supervised by committees of lay people. He thought that citizens should use their own principles when making decisions about these matters. He rejected the view that science is especially "rational" on the grounds that there is no single common "rational" ingredient that unites all the sciences but excludes other modes of thought (Against Method (3rd ed.). p. 246.).

**Philosophy of mind**

Along with a number of mid-20th century philosophers (most notably, Wilfred Sellars, W.V.O. Quine, and Richard Rorty), Feyerabend was influential in the development of eliminative materialism, a radical position in the philosophy of mind that holds that our ordinary, common-sense understanding of the mind (what materialist monists call "folk psychology") is false. It is succinctly described by a modern proponent, Paul Churchland, as follows:

"Eliminative materialism is the thesis that our commonsense conception of psychological phenomena constitutes a radically false theory, a theory so fundamentally defective that both the principles and the ontology of that theory will eventually be displaced, rather than smoothly reduced, by completed neuroscience."[12]

In three short papers published in the early sixties,[13][14][15] Feyerabend sought to defend materialism against the supposition that the mind cannot be a physical thing. Feyerabend suggested that our commonsense understanding of the mind was incommensurable with the (materialistic) scientific view, but that nevertheless we ought to prefer the materialistic one on general methodological grounds.

This view of the mind/body problem is widely considered one of Feyerabend's most important legacies. Even though Feyerabend himself seems to have given it up in the late 1970s, it was taken up by Richard Rorty and, more recently, by Patricia Churchland and Paul Churchland. In fact, as Keeley observes,[16] "PMC [Paul Churchland] has spent much of his career carrying the Feyerabend mantle forward" (p. 13).

**Other works**

Some of Feyerabend's work concerns the way in which people's perception of reality is influenced by various rules. In his last book, unfinished when he died, he talks of how our sense of reality is shaped and limited. Conquest of Abundance: A Tale of Abstraction versus the Richness of Being bemoans the propensity we have of institutionalizing these limitations.

The last philosophy book that Feyerabend finished is The Tyranny of Science (written 1993, published May 13, 2011). In it Feyerabend challenges some modern myths about science, including the myth that 'science is successful'. He argues that some very basic assumptions about science are simply false and that substantial parts of scientific ideology were created on the basis of superficial generalizations that led to absurd misconceptions about the nature of human life. Far from solving the pressing problems of our age, scientific theorizing glorifies ephemeral generalities at the cost of confronting the real particulars that make life meaningful.
Popular influence

The book *On the Warrior's Path* quotes Feyerabend, highlighting the similarities between his epistemology and Bruce Lee's worldview.\[^{[17]}\]

Criticism

The rationalist philosopher and popular essayist David Stove claimed that, in his philosophical work, Feyerabend was responsible for "neutralizing success-words" and "sabotaging logical expressions".\[^{[18]}\] The first of these rhetorical devices involves casting success-words like 'progress', 'advance' and 'improvement' in a doubtful light. According to Stove, Feyerabend does this, first of all, by allowing the reader their own understanding of these terms. In Feyerabend's words,

> Everyone can read the terms in his own way and in accordance with the tradition to which he belongs. Thus for an empiricist, 'progress' will mean transition to a theory that provides direct empirical tests for most of its basic assumptions. [...] For others, 'progress' may mean unification and harmony, perhaps even at the expense of empirical adequacy. *Against Method*, p. 18.

This, Stove understands as a "promise to neutralize all success-words". Unfortunately, according to Stove, Feyerabend "does not keep his promise [...] but still he often does neutralize them; and when he does it is often by means of quotation-marks.". Stove explains,

> Thus in certain circumstances a journalist might write "The Minister 'refuted' the allegations", meaning, and being understood to mean, that the Minister did not refute but only denied them. *Popper and After*, p. 9.

The second rhetorical device Stove criticised was the practise of robbing logical statements of their logical force by placing them in epistemic contexts; for example, instead of saying "P is a proof for Q" one would say "It is generally believed by scientists that P is a proof for Q". This produces what Stove calls a "ghost logical statement": it gives the impression that serious statements of logic are being made when they are not - all that is really being made are sociological or historical claims which are immune to criticism on logical grounds.

Having exposed these "literary devices", Stove does not consider Feyerabend's arguments further. Instead, he chooses to show that Hume's inductive scepticism is irrational since, Stove believes, it "operates as a tacit premise, indeed, in the philosophy of Feyerabend". In other words, if Hume's inductive scepticism falls, then so do Feyerabend's arguments of "Against Method".

Stove attempts this by dissecting Hume's argument. He shows that Hume has good grounds for the proposition denoted *N*, that "An inductive argument is invalid, and any validator of it is not a reason or part of a reason to believe its conclusion.". Stove, however, denies that this is grounds for accepting Hume's conclusion, denoted *C*, that "No proposition about the observed is a reason to believe a contingent proposition about the observed.". As he explains,

> [...] some other philosophers (of whom I am one) resist this step from N to C. We have a less exacting standard of reasonable argument than most philosophers incline to. We say that an argument can be invalid, and even incurably so, but still its premise can be a reason to believe its conclusion. It is so, we say, with some inductive arguments in particular. Hume's result *N* we accept, and we admire it, as a profound truth about induction which his argument brought to light. But the sceptical conclusion *C* which Hume drew from *N* does not follow, we say, and is false. *Popper and After*, p. 73.

To bridge the gap from the proposition *N* to the proposition *C* requires an addition proposition which gives deduction as the *only* valid form of reasoning from a premise to a conclusion. If, as Stove believes, inductive scepticism is an unspoken, yet key, assumption in Feyerabend's writing, then it follows that the foundations of Feyerabend's philosophy also suffer this weakness. In Stove's opinion this is sufficient to show that the Feyerabend of "Against Method" is irrational.
Quotes

• ...And it is of course not true that we have to follow the truth. Human life is guided by many ideas. Truth is one of them. Freedom and mental independence are others. If Truth, as conceived by some ideologists, conflicts with freedom, then we have a choice. We may abandon freedom. But we may also abandon Truth.[19]
• ...when sophistication loses content then the only way of keeping in touch with reality is to be crude and superficial. This is what I intend to be.[19]

Selected bibliography

Books


Articles

• "How To Defend Society Against Science". Radical Philosophy no. 11, Summer 03 1975. [21]

References

[5] Understanding Feyerabend on Galileo (http://iuq.sagepub.com/cgi/content/abstract/74/1/89)


Further reading


External links

- Paul Feyerabend (http://plato.stanford.edu/entries/feyerabend) entry in the Stanford Encyclopedia of Philosophy
- De la paradoja en el "todo vale" de Paul Feyerabend a la falacia de la falsa libertad (http://galileo.fcien.edu.uy/de_la_paradoja_en_el__todo_vale.html) Horacio Bernardo en Galileo Número 28. Octubre 2003 (Spanish)
- Voodoo and the monster of science (http://www.dur.ac.uk/i.j.kidd/abundance_files/CooperCOA.html) a review by David E. Cooper of Conquest of Abundance, Times Higher Education Supplement (17 March 2000)
Image Sources, Licenses and Contributors


