THOMAS KUHN ON REVOLUTION
AND
PAUL FEYERABEND ON ANARCHY

This BOOK focuses on Thomas Kuhn and Paul Feyerabend's wholistic variations on the contextual or artifactual thesis of relativized semantics. The classical pragmatists recognized the philosophical significance of the phenomenon of belief. But belief has taken on a much greater importance in contemporary pragmatism, where a universally quantified descriptive discourse believed to be true (what Quine calls the "web of belief") constitutes a context that controls the semantics and thus ontology of descriptive discourse. This is the contextual or artifactual thesis of relativized semantics. Thomas Kuhn and Paul Feyerabend's variants of this artifactual thesis of the semantics of language led these two philosophers as well as others to propose new roles for the phenomenon of prejudicial belief in the history and dynamics of scientific development.

Thomas S. Kuhn (1922-1996) was born in Cincinnati, Ohio. He received a Bachelor of Science degree *summa cum laude* from Harvard University in 1943. His first exposure to history of science came as an assistant to James B. Conant in a course designed to present science to nonscientists. He received his Ph.D. from Harvard in 1949, and has since taught history of science at Harvard University, at the University of California at Berkeley (1961), at Princeton University (1964) and at the Massachusetts Institute of Technology (1979). A transcript of an autobiographical interview is reprinted in *The Road Since Structure* (2000).

Paul K. Feyerabend (1924-1994) was born in Vienna, Austria. He was inducted into the Austrian army during World War II, and was wounded in a retreat from the advancing Russian army in 1945. After the war he studied theater at the Wiemar Institute, and then went to the University of Vienna, where he received a Ph.D. in philosophy in 1951. He then went to England and studied under Popper, whose views he later rejected. He immigrated to the United States in 1959, and for the remainder of his career was at the University of California at Berkeley. In 1993 he wrote a brief autobiography titled *Killing Time*. The story of the historical approach in twentieth-century philosophy of science, however, begins with Conant.

Conant on Prejudice and The Dynamic View of Science

James B. Conant (1883-1978) is the principal influence on the professional thinking of Kuhn. Kuhn dedicated his *Structure of Scientific Revolutions* to Conant, "Who Started It", and Conant acknowledged Kuhn's contributions to the "Case Histories in Experimental Science" course that Conant started at Harvard University. Conant received his doctorate in chemistry at Harvard in 1916, and then taught chemistry at Harvard from 1919 to 1933, when he accepted an appointment as the university's president. In 1953 he resigned his position at Harvard to accept an appointment as U.S. High Commissioner of the Federal Republic of Germany and then later as U.S. Ambassador to Germany. In 1970 he wrote *My Several Lives: Memoirs Of A Social Inventor*, an autobiography describing these three phases of his professional life. Conant's views on the history and nature of science are set forth in a series of books. The earliest is his *On Understanding Science: An Historical Approach* (1947), which he later expanded into *Science And Common Sense* (1951). A year later he published *Modern Science And Modern Man* (1952), which contains "The Changing Scientific Scene: 1900-1950" in which he elaborates his "skeptical approach" to modern quantum theory. In 1964 he published *Two Modes Of Thought*, which contains several references to Kuhn's *Structure of Scientific Revolutions* in context supportive of Kuhn's famous thesis.

Conant advocates what he calls the "dynamic view" of science, and he contrasts it with the "static view", which he identifies with the positivist philosophy and specifically with the philosophy set forth by Karl Pearson in the latter's *Grammar of Science*. The static view represents science as a systematic body of knowledge, while the dynamic view represents science as an ongoing and continuing activity. On the dynamic view the present state of knowledge is of importance chiefly as a basis for further research activity. Conant defines science as an interconnected series of concepts and conceptual schemes that have developed as a result of experimentation, and that are fruitful of further experimentation and observations. He explicitly rejects positivism, which he portrays as a quest for certainty, and he emphasizes that science is a speculative enterprise that is successful only to the degree that it is continuing.

Conant also maintains what he calls his “skeptical” view. On this view microphysical theory does not actually describe reality, but rather is a “policy” that serves as a guide for fruitful future research activity. He maintains that the wave-particle duality thesis in the quantum theory has changed the attitude of physicists, such that science is now viewed in terms of “conceptual schemes”, which arise from experiment and are fruitful of more experiments. The wave-particle duality is one such conceptual scheme, and it justifies his “skeptical” approach, because this conceptual scheme does not describe what light “really” is. Instead modern physics describes the properties of light and formulates them on the simplest possible principles. The history of science is a history of the succession of such conceptual schemes. Conant references the view of the Harvard pragmatist philosopher, William James, who maintained that man’s intellectual life consists almost wholly in the substitution of a conceptual order for the perceptual order from which experience originally comes. Different universes of thought arise as concepts and percepts interpenetrate and “melt” together, “impregnate” and “fertilize” each other. As a result the series of conceptual schemes in the history of science is one in which the conceptual schemes are of increasing adequacy to the perceptions in experimentation.

Conant had initially believed that natural sciences have an accumulative character that reveals progress, but following Kuhn’s *Structure of Scientific Revolutions* (1962) Conant modified his view of the accumulative nature of science. He continues to find accumulative progress in the empirical-inductive generalizations in science and also in the practical arts, but he excludes accumulative progress from the theoretical-deductive method, which admits to scientific revolutions.

Conant identifies the static view with the logical perspective, while he admits the psychological and the sociological perspectives in his dynamic view. The sociological perspective reveals that science is a living organization, which exists due to close communication that enables new ideas to spread rapidly, and that enables discoveries to breed more discoveries. Scientists pool their information, and by so doing they start a process of cross-fertilization in the realm of ideas. As a social phenomenon, science is a recent invention starting with the scientific societies of the seventeenth and eighteenth centuries, and then evolving in the universities in the nineteenth century. Communication was initially through letters, then later through books, and now through journals.

He maintains that historically one of the more important psychological aspects of the development of science is prejudice, a matter toward which he admits he himself has an ambivalent attitude. On the one hand the traditions of modern science, the instruments, the high degree of specialization, the crowd of witnesses that surround the scientist – all these things exert pressures that make impartiality in matters of science almost automatic. If the scientist deviates from the rigorous rôle of impartial experiment or observation, he does so at his peril. On the other hand Conant says that to put the scientist on a pedestal because he is an impartial inquirer is to misunderstand the historical situation. This misunderstanding results both from the dogmatic character of textbooks and from the view of positivist philosophers such as Karl Pearson. Conant emphasizes the stumbling way in which even the ablest of the scientists of every generation have had to fight through thickets of erroneous observation, misleading generalization, inadequate formulations and unconscious prejudice. He notes that these problems are rarely appreciated by those who obtain their scientific knowledge from textbooks and by those who expound on “the” scientific method.

Conant exhibits his thesis in his description of the chemical revolution, in which the phlogiston theory of combustion was replaced by the theory of oxygen. He notes that for one-hundred fifty years an anomaly to the phlogiston theory, the fact the a calx weighs more than its metal, was known to exist, but that the theory itself was never called into question until a better one was developed to take its place, namely Lavoisier’s new conceptual scheme. In the meanwhile the phlogiston theory was an obstruction to the development of the new conceptual scheme, as scientists attempted to reconcile the anomaly to the phlogiston theory.

Conant also notes that even after the new conceptual scheme was advanced to overthrow the phlogiston scheme, there continued to be debate, and that the proponents of the new conceptual scheme were no more shaken by a few alleged facts contrary to the new scheme, than were the advocates of the old scheme by facts anomalous to the earlier scheme. Lavoisier pursued his conceptual scheme in spite of embarrassing experimental findings, which only after his death were found to be erroneous findings.

Conant's thesis in this examination of the chemical revolution is that both sides in the controversy had put aside experimental evidence that did not fit into their respective conceptual schemes. And in his view what is most significant is the frequent fact that subsequent history may show that such arbitrary dismissal of "the truth" is quite justified. He concludes that to suppose that a scientific theory stands or falls on the issue of one experiment is to misunderstand science entirely. Conant characterizes the first fifty years of the nineteenth century that culminated in the chemists' atomic theory of matter, as a period of "the conflict of prejudices".

He notes that one who is not familiar with this episode in the history of science will be amazed to discover that all the relevant ideas and all the basic data for the atomic theory were at hand almost from the outset of the nineteenth century. An analysis of the arguments, pro and con, shows that certain preconceived ideas then current among scientists blocked its development. Still, Conant rejects the view that the scientific way of thinking requires the habit of facing reality quite unprejudiced by any earlier conceptions. In his *Science and Common Sense* he admits that prejudices are emotional and nonlogical reactions. Yet he also maintains that every scientist must carry with him the scientific prejudices of his day – the many vague, half-formulated assumptions which to him seem "common sense". Apparently as a result of his acceptance of prejudice as an inevitable fact in the dynamics of science, Conant unabashedly declares that his dynamic view of science is his "prejudice", and adds that he makes "no attempt to conceal it".

It may be said that one of the differences between Kuhn and Conant is that the latter regards prejudice as merely an inescapable fact in the history of science, while the former regards it as having a contributing function that is inherent in the dynamics of science. In Kuhn's doctrine of "normal science", what Conant calls "prejudice", Kuhn calls by the less pejorative phrase "paradigm consensus". But unlike Conant, Kuhn does not view prejudice as merely an individual phenomenon with one scientist taking one prejudice and another taking some alternative prejudice. In Kuhn's view paradigm consensus is a sociological-semantical phenomenon, and this semantical perspective did not come from Conant. In spite of Conant's dynamic view including reference to William James about percepts being impregnated with concepts, Conant's view of the semantics of language is not dynamic. His static view of semantics led him to his "skeptical approach", just as it likewise led Bohr to his instrumental view of the formalisms of quantum physics, and for the same reason: without a theory of semantical change, neither Bohr nor Conant could admit a realistic interpretation to the wave-particle duality of the modern quantum theory. While Conant was a very important influence on Kuhn, Kuhn also has his own personal formative intellectual experience, which he calls his "Aristotle experience" and which he says is responsible for much that is distinctive and original in his thinking.

Kuhn's "Aristotle Experience"

Most of the twentieth-century philosophers of science who have made influential contributions have been inspired by their reflections on the spectacular developments in twentieth-century physics, notably relativity theory and quantum theory. However, Kuhn reports that his intellectually formative experience was inspired by his reading Aristotle's *Physics*, and he calls this moment of inspiration his "Aristotle experience." His principal account of this experience is published in his "What are Scientific Revolutions?" (1987), and mention is also made in his 1995 autobiographical interview published in *Neusis: Journal for the History and Philosophy of Science and Technology* (1997), which is also published in an edited version as "A Discussion with Thomas S. Kuhn" in *The Road Since Structure* (2000) along with a reprint of "What are Scientific Revolutions?"

Kuhn's "Aristotle experience" was occasioned by his reading the physics texts of Aristotle in 1947 as a graduate student in physics at Harvard University, in order to prepare a case study on the development of mechanics for James B. Conant's course in science for nonscientists. Kuhn reports that he approached Aristotle's texts with the Newtonian mechanics in mind, and that he hoped to answer the question of how much mechanics Aristotle himself had known and how much he had left for people like Galileo and Newton to discover. And he states that having brought to the texts the question formulated in that manner, he rapidly discovered that Aristotle had known almost no mechanics at all, and that everything was left for his successors to discover later. Specifically on the topic of motion Aristotle's writings seemed to be full of egregious errors, both of logic and of observation. Kuhn reports that this conclusion was disturbing for him, since Aristotle had been admired as a great logician and was an astute naturalistic observer.

Kuhn then asked himself whether or not the fault was his own rather than Aristotle's, because Aristotle's words had not meant to Aristotle and his contemporaries what they mean today to Kuhn and his own contemporaries. Kuhn describes his reconsideration of Aristotle's *Physics*: He reports that he continued to puzzle over the text while he was sitting at his desk gazing abstractly out the window of his room with the text of Aristotle's Physics open before him, when suddenly the conceptual fragments in his head sorted themselves out in a new way and fell into place together to present Aristotle as a very good physicist but of a sort that Kuhn had never dreamed possible. Statements that had previously seemed egregious mistakes afterward seemed at worst near misses within a powerful and generally successful tradition.

Kuhn then inverted the historical order; he made his account of scientific revolution describe what Aristotelian natural philosophers needed to reach Newtonian ideas instead of what he, a Newtonian reading Aristotle's text, needed to reach the ideas of the Aristotelian natural philosophers. Thus he maintains that experiences like his Aristotle experience, in which the pieces suddenly sort themselves out and come together in a new way, is the first general characteristic of revolutionary change in science. He states that though scientific revolutions leave much mopping up to do, the central change cannot be experienced piecemeal, one step at a time, but that it involves some relatively sudden and unstructured transformation in which some part of the flux of experience sorts itself out differently and displays patterns that had not been visible previously.

Kuhn's theory of scientific revolutions sparked by his "Aristotle experience" has been called wholistic (or "holistic"). The transition as experienced is synthetic, and Kuhn views it as all of a piece, as it were, denying that it can be understood "piecemeal". In his *Structure of Scientific Revolutions* he labeled the synthetic character of the revolutionary transitional experience with the phrase "*gestalt* switch." But after receiving much criticism from many philosophers of science he eventually attempted a semantical analysis of scientific revolutions.

But before *Structure of Scientific Revolutions* (1962), there was his *Copernican Revolution*, which offers little or no suggestion of his conclusions from his "Aristotle experience." Yet later his examples for semantical analysis routinely come from his *Copernican Revolution*, and seldom come from Aristotle's texts. Consider next Kuhn's views of the historic scientific revolution that benchmarks the beginning of modern science.

Kuhn on the Copernican Revolution

Kuhn's influential and popular *Structure of Scientific Revolutions* was preceded by his *Copernican Revolution: Planetary Astronomy in the Development of Western Thought* in 1957. The earlier work is less philosophical, and it reveals the influence of Conant. The *Copernican Revolution* contains some ideas that reappear in the *Structure of Scientific Revolutions*. One idea is the central feature of scientific revolutions, that old theories are replaced by new and incompatible ones. In the later book this thesis is elaborated in semantical terms, and it is the basis for his describing scientific revolutions as "noncumulative" episodes in the history of science. Kuhn says in his autobiographical interview written years later, that the noncumulative nature of revolutions was the result of his 1947 "Aristotle experience." However, in the 1957 *Copernican Revolution* his semantical view is that scientific observations are indifferent to the conceptual schemes that constitute theories, and that observations must be distinguished from interpretations of the data that go beyond the data, such that two astronomers can agree perfectly about the results of observation and yet disagree emphatically about issues such as the reality of the apparent motion of the stars. He states that observations in themselves have no direct consequences for the cosmological theory. No positivist would object to these statements.

Later, however, he maintains instead that observations depend on the particular theory held by the scientist, a distinctively post-positivist thesis. Thus in his "What are Scientific Revolutions?" (1987) he states that the transition from the Ptolemaic view to the Copernican one involved not only changes in laws of nature like the development of Boyle's gas laws, but also involved changes in the criteria by which some terms in the laws attach to nature, *i.e.*, it involved meaning changes, and that the criteria are in part dependent upon the theory containing those terms. Thus in the Ptolemaic theory the terms "sun" and "moon" refer to planets and "earth" does not, while in the Copernican theory "sun" and "moon" are not referenced as planets and the earth is referenced as a planet like Mars and Jupiter, thereby making the two theories not just incompatible, but what he calls semantically "incommensurable". Nonetheless, as he develops his semantical views over the years, he maintains that astronomers holding either theory can somehow pick out the same referents and identify those celestial bodies, which are described differently in the two contrary theories.

A second idea reappearing in the 1962 book is his thesis that the "logic" of science does not completely control the development of science. The logic that he has in mind is a stereotype of Popper's view, that the occurrence of just one single observation which is incompatible with a theory, dictates that the scientist reject the theory as wrong and abandon it for some other one to replace the wrong one. Kuhn believes that the incompatibility between theory and observation is the ultimate source for the occurrence of scientific revolutions, but he also maintains that historically the process is never so simple, because scientists do not surrender their beliefs so easily. What was to Copernicus a stretching and patching to solve the problem of the planets for the two-sphere theory, was to his predecessors a natural process of adaptation and extension.

Kuhn therefore finds in the history of science what he calls "the problem of scientific beliefs": Why do scientists hold to theories despite discrepancies, and then having held to them in these circumstances, why do they later give them up? The significance that Kuhn gives to this phenomenon reveals the influence of Conant. The "problem of scientific beliefs" is the same as what Conant meant by the phenomenon of "prejudice". Typically historians and philosophers of science did not consider this phenomenon as having any contributing rôle in the development of science, because it is contrary to the received concept of the programmatic aim of science. And in 1957 Kuhn was clearly as ambivalent in his attitude toward the problem of scientific belief as Conant was toward the phenomenon of prejudice in science.

In the 1957 book Kuhn locates part of the reason for the problem of scientific belief in the scientist's education, a reason that he also calls "the bandwagon effect". This reason is carried forward into the 1962 book, where it has a very important place. In the 1957 book, however, he considers it to be of secondary importance. The other and more important part of the reason in the 1957 book is the interdependence of other areas of the culture with the scientific specialty. The astronomer in the time of Copernicus could not upset the two-sphere universe without overturning physics and religion as well. Fundamental concepts in the pre-Copernican astronomy had become strands for a much larger fabric of thought, and the nonastronomical strands in turn bound the thinking of the astronomers.

The Copernican revolution occurred because Copernicus was a dedicated specialist, who valued mathematical and celestial detail more than the values reinforced by the nonastronomical views that were dependent on the prevailing two-sphere theory. This purely technical focus of Copernicus enabled him to ignore the nonastronomical consequences of his innovation, consequences that would lead his contemporaries of less restricted vision to reject his innovation as absurd. In his 1962 book *Structure of Scientific Revolutions*, however, Kuhn does not make the consequences to the nonspecialist an aspect of his general theory of scientific revolutions. Instead he maintains that scientists persist in their belief in theories with observational discrepancies for reasons entirely internal to the specialty.

Kuhn on the Structure of Scientific Revolutions

The *Structure of Scientific Revolutions* is a small monograph of less than one hundred seventy-five pages written in a fluent colloquial style that makes it easily accessible to the average reader. It is the most renown of Kuhn's works; indeed, it was eventually a *succès de scandale* in academic philosophy. It is strategically without any of the mathematical equations that have enabled the modern natural sciences since the historic Scientific Revolution, and is mercifully without any of the pretentious symbolic-logic that retarded examination of the sciences by the logical positivists. It was also a very timely presentation of the ascending pragmatist philosophy of science illustrated with a plethora of apparently exemplifying cases from the history of science, which seemed conclusively to document the book's thesis. Kuhn had previously published many tenants of this 1962 book in his "The Essential Tension" in 1959, later reprinted in a book of the same name in 1977. But the 1962 book was probably the most popular/controversial book pertaining to philosophy and history of science published in the 1960's and indeed for many years afterwards. It was reported in Kuhn's *New York Times* obituary to have sold about one million copies and to have been published in sixteen languages by the time of his death. It was widely read outside the relatively small circles of academic philosophers and historians of science.

In "Reflections on My Critics" in *Criticism and the Growth of Knowledge* (ed. Lakatos and Musgrave, 1970) Kuhn offers some personal insights. He states that in his work as a historian of science he discovered that much scientific behavior including that of the greatest scientists persistently violated accepted methodological canons, and that he wondered why these apparent failures to conform to the canons did not at all seem to inhibit the success of the scientific enterprise. The accepted methodological canons that Kuhn has in mind are not only those of the positivists but also Popper's falsificationist thesis. He states that his altered view of the nature of science transforms what had previously seemed aberrant behavior into an essential part of an law for science's success, and that his criterion for emphasizing any particular aspect of scientific behavior is not simply that it occurs, or merely that it occurs frequently, but rather that it fits a theory of scientific knowledge, a theory which he says may have normative as well as descriptive value. The seemingly aberrant behavior is what he had previously called "the problem of scientific beliefs", the practice of ignoring anomalies.

The thesis of the book offers a coherent description of the historical development of what he calls the "mature" natural sciences. Kuhn portrays the developmental procession as an alternation between two phases, which he calls "normal science" and "revolutionary science", with each phase containing the seeds for the emergence of the other. In the normal-science phase the phenomenon that Conant called "prejudice" and that in 1957 Kuhn had called the "problem of scientific beliefs", reappears as "paradigm consensus" in his 1962 book, where it assumes a positive function without the ambivalence that it formerly had in Kuhn and Conant's minds. In an article remarkably titled "The Function of Dogma in Scientific Research" in *Scientific Change* (ed. Crombie, 1963) Kuhn maintains that advance from one exclusive paradigm to another rather than the continuing competition between recognized classics, is a functional as well as a factual characteristic of mature scientific development. In the revolutionary-science phase the old paradigm around which a consensus had been formed is replaced by a new one, which is "incommensurable" with the old one. Thus Kuhn's work gives new and systematic meaning to the already conventional phrase "scientific revolutions".

Kuhn's thesis is not just an eclectic combination of philosophical and historical ideas. His concepts of normal and revolutionary science are aspects of his distinctive sociological thesis, in which the concept of science as a social institution is fundamental. To sociologists and cultural anthropologists the concept of social institution means a set of beliefs and values shared among the members of a group or community, and internalized by each individual member of the community. The shared beliefs control the individual's understanding of the world in which he lives, and the shared value system regulates his voluntary behavior including his interaction with others. It is in these sociological terms that Kuhn advances his startling new concept of the aim of science. In the normal-science phase the prevailing consensus paradigm by virtue of its consensus status assumes institutional status in its scientific specialty, and the aim of normal science is the further articulation of the paradigm by an incremental or "puzzle-solving" type of research that is uncritical of the paradigm. The paradigm is the scientist's view of the domain of his science, and the institutional valuation that consensus associates with the paradigm makes conformity with it the criterion for scientific criticism. Thus what Kuhn previously called the "problem of scientific beliefs" is no longer problematic; the belief status of the paradigm is explained by its institutional status. This status effectively makes the consensus paradigm what Conant had called a "creed". Research producing scientific change in the normal-science phase is controlled by belief in the consensus paradigm, and the resulting scientific change is always a change within the institutional framework defined by the paradigm.

In striking contrast the revolutionary-science phase is not a change within the institutional framework defined by the paradigm, but rather is a change to another paradigm, and therefore is an institutional change in the sense of a change of institutions. Kuhn maintains that the new and old paradigms involved in such an institutional change are semantically and ontologically incommensurable, such that there can be no shared higher framework to control the revolutionary transition. The term "revolution" in Kuhn's thesis is therefore not a metaphor. Scientific revolutions are no less revolutionary in the literal sense than are political revolutions, because in neither case are there laws to govern these changes. With his sociological thesis in mind, Kuhn's own dynamic view of science may be described as a sequence of five phases, which follows closely the sequence of several of the chapter headings in his book:

(1) ***Consensus Phase.*** Mature sciences are distinguished by "normal science", a type of research that is firmly based in some past scientific achievement, and that the members of the scientific specialty view as

supplying the foundations for research. Unlike early science there are normally neither competing schools nor perpetual quarrels over foundations in a mature science. The achievements that guide normal-science research are called paradigms, which consist of accepted examples that provide models from which spring particular traditions of scientific research. A paradigm is an object for further articulation and specification under new and more stringent conditions, and it includes not only articulate rules and theory, but also the tacit knowledge and pre-articulate skills acquired by the scientist. No part of the aim of normal science is to call forth new sorts of phenomena or to invent new theories. This conformism proceeds both from a professional education, which is an indoctrination in the prevailing paradigm set forth in the student's current textbooks and laboratory exercises, and from a consensus belief shared by the members of the scientific specialty. The consensus belief makes the paradigm seem sufficiently promising as a guide for future research, that acceptance of it is both an obligatory and a justified act of faith. Conformity to the paradigm assumes a recognizable function, which is to focus the group's attention upon a small range of relatively esoteric problems, to investigate these problems in a depth and detail that would not be possible, if quarrels over fundamentals were tolerated, and to restrict the limited research resources of the profession to solvable problems, where the solutions are "solvable" precisely because they agree with the paradigm and are interpretable in its terms.

(2) ***Anomaly Phase.*** Normal science is a cumulative enterprise having as its aim the steady extension of the scope and accuracy of scientific knowledge represented by the prevailing paradigm. Successful normal science does not find any novelties. But anomalies nonetheless occur as the extension of the paradigm proceeds over time. In fact the paradigm is the source of the concepts needed for recognizing the new fact and for giving it anomalous status. The normal reaction to an anomaly is a modification of the articulate rules and theories associated with the consensus paradigm, so that the anomalous fact can be assimilated. Success in such modification is a noteworthy achievement for a normal-science researcher. Isolated anomalies that are not assimilated are normally set aside under the assumption that eventually they will be reconciled, and normal-science research continues with the consensus paradigm. Anomalies do not easily distract scientists from continued exploration of the promise of a generally still satisfactory paradigm. Kuhn rejects Popper's falsificationist

philosophy, stating that if every failure to fit were ground for theory rejection, all theories ought to be rejected at all times.

(3) ***Crisis Phase.*** So long as the consensus paradigm is relatively successful, no alternatives to it are advanced. But eventually the anomalies become more numerous and more serious, and also the modifications necessary to assimilate those anomalies that can be assimilated, produce a certain amount of paradigm destruction. In due course some members of the profession lose faith and begin to propose alternatives. The construction of alternative theories is always possible, because there is an arbitrary aspect to language that permits many theories to be imposed on the same collection of data. When the consensus underlying the prevailing paradigm begins to erode enough that some members begin to exploit this arbitrary element and to create alternative theories, the profession has entered the phase of crisis. Crises are the crossing of the threshold into extraordinary or revolutionary science.

(4) ***Revolutionary Phase.*** Kuhn postulates what he calls a "genetic parallel" between political and scientific revolutions. Just as political revolutions are inaugurated by a growing sense that existing institutions have ceased adequately to meet the problems posed by an environment that they have in part created, so too scientific revolutions are inaugurated by a growing sense that the existing institutionalized paradigm has ceased to function adequately in the exploration of the aspect of nature to which the paradigm itself had previously led the way. Political revolutions aim to change political institutions in ways that those institutions themselves prohibit. Their success therefore necessitates the partial relinquishment of one set of institutions in favor of another, and in the interim society is not fully governed by institutions at all. As alternatives are formulated, society is divided into competing camps, those who support the old institutions and those who support the new. Once this polarization has occurred, political recourse fails; there is no supra-institutional framework for adjudication of differences. Kuhn says that like the choice between competing political institutions, the choice between competing paradigm institutions is a choice between incompatible modes of community life. In a scientific revolution the semantical and ontological incommensurability between rival paradigms excludes the possibility of any common framework for reconciliation or even for communication.

Kuhn does not describe incommensurability in terms of Whorf's linguistic relativity thesis, as did Feyerabend thirteen years later. Instead Kuhn invokes Hanson's thesis of *gestalt* switch, and references Hanson's *Patterns of Discovery* published four years earlier. He compares the change of paradigm to the visual *gestalt* switch. A certain *gestalt* is needed for the physics student to see the world as seen by the scientist, when for example the latter sees the electron's track in the Wilson cloud chamber, and the *gestalt* learned by the student is provided by the prevailing normal-science paradigm. When at times of revolution the normal-science tradition changes, then the scientist's perception of his environment must be re-educated; he must see it with a new *gestalt*. This change of paradigm is not achieved by deliberation and interpretation, but rather by a sudden and unstructured *gestalt* switch. While the members are individually experiencing the *gestalt* switch, the profession is divided and confused, and there is a communication "breakdown" between members having different paradigm *gestalts*.

(5) ***Resolution Phase.*** Kuhn does not believe that issues in scientific revolutions are resolved by crucial experiments or by any other kind of empirical testing. In normal-science testing is never a test of the paradigm, but rather it is a test of a puzzle-solving attempt to extend the paradigm, and involves a comparison of a single paradigm with nature. Failure of the test is not a failure of the paradigm, but rather is a failure of the scientist. In revolutionary-science tests occur as part of the competition between two rival paradigms for the allegiance of the scientific community. However, these tests do not have a compellingly decisive function. There can be no scientifically or empirically neutral system of language or concepts for these tests, since the paradigms are incommensurable, and those who maintain the old paradigm must experience a "conversion" to the new *gestalt*. Tests serve only to persuade the members of the profession that the new paradigm is the more promising guide for future normal-science research. The actual decision about the future performance of the new paradigm is based on faith and opportunism. As early supporters of the new paradigm show success, others follow until there is a new normal-science consensus paradigm. The procession has then come full circle to a new consensus paradigm.

In the final chapter of *Structure of Scientific Revolutions* Kuhn discusses the concept of scientific progress that is consistent with his theory of the historical development of science. He maintains that the semantics of the term "progress" is determined by reference to the research work of normal science and specifically by the puzzle-solving type of work in normal science in the absence of competing schools. Progress occurs in extraordinary science by the transition to a new consensus paradigm, because in the judgment of the specialized scientific community the new paradigm promises to resolve outstanding problems that had occasioned the crisis and transition, and to preserve the community's problem-solving ability to treat the assembled data with growing precision and detail, even though the ability to solve problems cannot be a basis for paradigm choice.

The Evolution of Kuhn's Philosophy

The evolution of Kuhn's central thesis of incommensurability may be divided into three phases. Firstly as in his *Structure of Scientific Revolutions* he described the idea in terms of completely wholistic *gestalt* switches. Some philosophers such as Feyerabend had no problem with the wholistic character of Kuhn's incommensurability thesis, but many others saw in it problematic implications for scientific criticism. In his autobiographical discussion published in *The Road Since Structure* (2000) Kuhn reports that shortly after writing *Structure of Scientific Revolutions* the Cambrian philosopher of science Mary Hesse told him in conversation that he must explain how science is empirical and what difference observations make, and he reports that he had agreed with her, and told her that he had failed to see it that way.

Therefore Kuhn entered a second phase beginning with *Criticism and the Growth of Knowledge* (1970), in which he continued to invoke *gestalt* switches, but he also introduced his idea of partial communication permitted by incommensurability-with-comparability in the attempt to deflect the irrationalism that critics such as Popper and others found in his views. But as Dudley Shapere had complained, Kuhn offered no analysis of meaning to explain meaning change.

Then in his third phase beginning in 1980 Kuhn unsuccessfully attempted language analysis to explain his thesis of incommensurability. His papers dealing with these attempts at linguistic analysis are reprinted in *Road Since Structure* (2000). The sections below will consider firstly Kuhn's criticisms of Popper's views, secondly some of the criticisms by various philosophers of his views expressed in *Structure of Scientific Revolutions* and his replies to these criticisms, thirdly the favorable reception of his views by sociologists, and finally his belated and ineffectual turn to language analysis.

Kuhn's Criticism of Popper's Falsificationist Philosophy

Nearly ten years after *Structure of Scientific Revolutions* Kuhn defended his thesis and replied to his critics in *Criticism and the Growth of Knowledge*. This is not his most mature work, since at this time he had yet to attempt language analysis. One critic that he took very seriously is Karl Popper. Kuhn's philosophy of science is not only a post-positivist philosophy critical of positivism; it is also a post-Popperian philosophy that is critical of Popper's falsificationist thesis of scientific criticism and of Popper's concept of scientific progress. The difference between Kuhn and Popper is explicable in large part by the differences in the episodes in the history of science that had formative influence on their respective thinking. Popper's philosophy of science was principally influenced by the episode in which the physics profession made the transition from Newton's theory of gravitation to Einstein's relativity theory, while Kuhn's philosophy was principally influenced by earlier episodes, his "Aristotle experience" and the transition from Ptolemy's geocentric theory to Copernicus' heliocentric theory. The noteworthy difference between these episodes is that the transition to Einstein's theory is often viewed as involving a crucial empirical test, Arthur Eddington's celebrated eclipse test of 1919, while the transitions to Newton's and Copernicus' theories, like the transition to Lavoisier's oxygen theory of combustion discussed by Conant, are not associated with any crucial tests but involved various nonempirical considerations. Popper views these nonempirical considerations as external impediments to progress in science, while Kuhn views them as internal and integral to the development of science.

Kuhn's explicit criticism of Popper is given in "Logic of Discovery or Psychology of Research?" in *Criticism and the Growth of Knowledge*. In this paper Kuhn begins by describing the similarities between his views and Popper's, which also separate both their views from those of the positivists. He notes that both he and Popper are concerned with the dynamic processes by which scientific knowledge is developed, instead of the logical structure of the products of scientific research, and that therefore both of them look to the history of science. He furthermore notes that both of them draw many of the same conclusions from the history of science particularly about which fields are sciences and which are not, that both are realists, which implies that neither are postmodernist antirealists, and that both reject the positivist thesis of a neutral or theory-independent observation language.

Then Kuhn turns to the contrasts between his views and Popper's. He maintains that even though he and Popper draw the same conclusions about which fields are sciences and which are not, they arrive at their shared conclusions by very different ways that may be contrasted as different *gestalt*s of the same situations. Popper maintains that scientists test theories and attempt to falsify them with a critical attitude. Kuhn maintains his thesis of normal science according to which a theory is not tested critically, but instead functions as a premise for puzzle-solving research with currently accepted theory supplying the rules of the game. Kuhn says that the type of tests that Popper discusses, such as the eclipse test of Einstein's theory of relativity in 1919, is rare in science, and he identifies this rare type of research as extraordinary or revolutionary science. He says that Popper has mistakenly characterized the entire scientific enterprise in terms that apply only to its occasional revolutionary parts.

Kuhn says that he is turning Popper on his head, when Popper demarcates scientific from nonscientific fields, because in Kuhn's view it is the abandonment of critical discourse rather than its adoption that makes the transformation of a field into a science. Once a field has made that transition, critical discourse recurs only at moments of crisis, when the basis of the field is again in jeopardy. Therefore Popper and Kuhn's lines of demarcation coincide only in their outcomes and not in their criteria; for their respective criteria they reference different aspects of scientific activity.

Then Kuhn proceeds to say that even during revolutionary phases of science, the choice between paradigms is not a choice in which critical testing can play a decisive role. Kuhn references Popper's "Truth, Rationality, and the Growth of Knowledge" in *Conjectures and Refutations*, where Popper states that the Ptolemaic theory was replaced before it had been tested. In this article Popper maintains that such instances reveal that crucial tests are decisively important, so that scientists have reason to believe that the new theory replacing the old one is better and nearer to the truth. But Kuhn argues that not only had these theories not been put to the test before they were replaced, but furthermore none of them was replaced before it had ceased adequately to support a puzzle-solving tradition.

Kuhn notes that both he and Popper agree that no theory can be conclusively falsified, that all experiments can be challenged either as to their relevance or to their accuracy, and that every theory can be modified by a variety of *ad hoc* adjustments without ceasing to be the same theory. But he argues that in Popper's philosophy recognition of such things operates merely as an incidental qualification of his philosophy, even though these things occur in the history of science. Kuhn cites as an example that the state of astronomy was a scandal in the early sixteenth century, but most astronomers nevertheless thought that normal adjustments to a basically Ptolemaic model would be sufficient to set the situation aright. In this sense the Ptolemaic theory had not failed any test. However a few astronomers including Copernicus thought that the difficulties must lie in the basic Ptolemaic approach itself rather than in the particular versions of Ptolemaic theory.

Kuhn says that Popper's error is the belief that logical criteria can dictate the falsification of a theory and determine theory choice during revolutions. Logical falsification presumes that a theory can be cast or recast such that all events are either corroborating, falsifying or irrelevant instances. But this cannot be done unless the theory is fully articulated and its terms sufficiently defined, so that it is possible to determine their applicability in every possible case. Kuhn says that no theory can in practice satisfy such a requirement, and that he had introduced the term "paradigm" to underscore the dependence of scientific research on concrete examples, that supply what would otherwise be gaps in the specification of the content and application of scientific theories.

Kuhn illustrates the semantical and pragmatical considerations captured by the term "paradigm" with a discussion of swans and the stereotypic theory that says "Every swan is white". Kuhn says that after a scientist has made his investigation and has found no instances of nonwhite swans, making the generalization explicit adds little or nothing to what is already known from the investigation. And if later one finds a black bird that otherwise appears to be a swan, then one's behavior will be the same whether or not one has made the explicit generalization that all swans are white. With or without the explicit generalization a decision must be made with respect to the possibility of black swans. Observation cannot force a falsifying decision. Only if one had previously committed oneself to a full definition of "swan", one that will specify its applicability to every conceivable object, could one be logically forced to rescind one's generalization. And Kuhn says that there is no good reason for such a commitment to any such explicit generalization; it is an unnecessary risk.

Similarly in science the scientist who is confronted with the unexpected, must always do more research in order to articulate his theory further in the area that has just become problematic. He may reject his theory in favor of another, and may do so for good reason, but no exclusively logical criterion can dictate the conclusion that the theory has been falsified or that it has not been falsified. Just as the investigator of swans need not make the decision as to whether whiteness is a defining characteristic of swans, until he can investigate further the apparently anomalous case of the black but otherwise swan-looking bird, so too the scientist has the same freedom to choose, and is not logically compelled to conclude that current theory has been falsified by apparently anomalous instances and test outcomes. Kuhn says that further empirical investigation is needed to answer such questions as how scientists actually make the choice between competing theories, and how scientific progress should be understood. He says that the type of answer to these questions must in the final analysis be psychological or sociological. He agrees with Popper's rejection of answers given in terms of the scientists' psychological idiosyncrasies, but he advocates investigation of the common elements induced by education of the licensed membership of the scientific group.

Popper's Criticism of "Normal Science"

In his *Thomas Kuhn and the Science Wars* Ziauddin Sardar reports that in 1965 Popper organized an International Colloquium in the Philosophy of Science, which was backed by the British Society for the Philosophy of Science, the London School of Economics and the International Union of History and Philosophy of Science. The intent was to critique Kuhn's theses in his *Structure of Scientific Revolutions*. The critiques and Kuhn's replies were published in 1970 under the title *Criticism and the Growth of Knowledge.*

Popper criticizes the aim of "normal science" as viewed by Kuhn, and he rejects the historical relativism he finds in Kuhn's thesis. His criticism in reply to Kuhn is set forth in "Normal Science and its Dangers" in *Criticism and the Growth of Knowledge*. Popper notes that he and Kuhn agree that the normal work of the scientist presupposes a theory that supplies the scientist with a generally accepted problem situation for his work. Interestingly he also states that he has always said that some dogmatism is necessary, because yielding to criticism too soon may preclude finding out where the real power of a theory lies. And he says that while he has been only dimly aware of the distinction that Kuhn makes between normal and revolutionary science, he admits that normal science in Kuhn's sense does exist.

But Popper maintains that the normal scientist in Kuhn's sense is a scientist who has been badly taught, since he does not think critically, a problem that Popper says he finds in quantum theory today. Popper expresses the opinion that uncritical normal science is dangerous both to science and to our civilization. He also takes exception to Kuhn's view that normal science as Kuhn conceives it is actually normal in the history of science. Kuhn's thesis of a single dominant theory may fit astronomy, but it does not fit the theory of matter or the biological sciences. Popper questions Kuhn's historical accuracy.

Popper is principally disturbed by Kuhn's historical relativism and with the thesis that philosophers of science should look to sociology and psychology of science instead of attempting a logical analysis, as Popper did in his own work. He argues that Kuhn's historical relativist thesis of the dynamics of science is not a sociological or a psychological one but rather a logical one, and he furthermore maintains that Kuhn's view is a mistaken one. He says that Kuhn's thesis that scientists must agree on fundamentals and on the framework of those fundamentals, in order to discourse rationally and critically, is what he calls "The Myth of the Framework". Popper admits that at any moment we are prisoners caught in the framework of our theories, expectations, past experiences, and language. But he adds that we are prisoners only in a Pickwickian sense, because if we try, we can escape our framework into a better and roomier one. He emphasizes that his central point is that a critical discussion and a comparison of the various frameworks are always possible. He denies that different frameworks are like mutually untranslatable languages.

In Popper's view the Myth of the Framework is the principal bulwark of irrationalism, and it merely exaggerates a difficulty into an impossibility. There are difficulties in discussions between people brought up in different frameworks, but Popper says that nothing is more fruitful than such discussions. An intellectual revolution may look like a religious conversion; a new insight may strike one like a flash of lightning. But this does not mean that one cannot evaluate former views critically and rationally in the light of new ones. It is simply false to say that the transition from Newton to Einstein is an irrational leap, and that the two theories of gravitation are not rationally comparable. In science we can say that we have made genuine progress, and that we know more than we did before such transitions occurred. Therefore, Popper says that all of Kuhn's own arguments go back to the thesis that the scientist is logically forced to accept a framework, since no rational discussion is possible between frameworks. This is not an historical, sociological, or psychological argument. It is a logical one and a mistaken one. *Popper concludes that science is "subjectless" in the sense that it is not bound to any framework.*

Popper reaffirms his own thesis that the aim of science is to find theories, which in the light of critical discussion get nearer to the truth and have greater truth content. Popper rejects Kuhn's proposal of turning to psychology and sociology for enlightenment about the aims of science and about the nature of scientific progress. He rejects all psychologistic and sociologistic tendencies, and furthermore says that in comparison to physics, psychology and sociology are riddled with fashions and uncontrolled dogmas. He concludes by answering Kuhn's question, "Logic of Discovery or Psychology of Research?" with the reply that while Logic of Discovery has little to learn from the Psychology of Research, the latter has much to learn from the former.

Feyerabend on Theory Proliferation vs. Kuhn's Consensus Paradigm

Feyerabend also criticizes Kuhn, and says that the doctrine of normal science is an ideology that Kuhn propagandizes among social scientists. His principal methodological criticism of Kuhn's philosophy is that Kuhn's theory cannot explain the transition from a monistic normal science to a pluralistic revolutionary science, since the impossibility of a semantically neutral observation language makes a plurality of alternative theories a precondition for the transition to be brought about. Feyerabend's criticism of Kuhn is given in his "Consolations for the Specialist" in *Criticism and the Growth of Knowledge.*

Firstly Feyerabend notes that he and Kuhn had discussed their views while both were at the University of California at Berkeley. And he says that while he recognizes the problems that interest Kuhn, notably the omnipresence of anomalies, he is unable to agree with Kuhn's theory of science, which he also calls an ideology. Feyerabend maintains that Kuhn's ideology can give comfort only to the most narrow-minded and conceited kind of specialist, that it tends to inhibit the advancement of knowledge, and that it is responsible for such inhibiting tendencies in modern psychology and sociology. He elaborates on his view that Kuhn's theory is an ideology: He states that Kuhn's presentation contains an ambiguity between the descriptive and the prescriptive mode of presentation. As a result more than one social scientist has pointed out to him that after reading Kuhn's book, he at last knows how to turn his field into a "science". Feyerabend reports that the recipe that these social scientists have taken from Kuhn consists of such practices as restricting criticism, reducing the number of comprehensive theories to one, creating a normal science that has one theory as its paradigm, preventing students from speculating along different lines, and making more restless colleagues conform and do "serious work".

He then asks whether or not Kuhn's following among sociologists is an intended effect, whether it is Kuhn's intention to provide an historical-scientific justification for sociologists' need to identify with some group. In criticism of Kuhn, Feyerabend concludes that it is actually Kuhn's intention to provide an ambiguity between the descriptive and the prescriptive modes of presentation, and that Kuhn wishes to exploit the propagandistic potentialities in this ambiguity. He says that Kuhn wants on the one hand to give solid, objective historical support to value judgments, which he and others regard as arbitrary and subjective, while on the other hand Kuhn also wants to leave himself a safe line of retreat. When those who dislike Kuhn's implied derivation of values from facts object, Kuhn's line of retreat consists of telling them that no such derivation can be made, and that the presentation is purely descriptive.

Secondly Feyerabend turns his criticism to Kuhn's thesis as a descriptive account of science. The central thesis of his criticism of Kuhn is that the latter's theory of science leaves unanswered the problem of how the transition from the monistic normal-science period to a pluralistic revolutionary period is brought about. Feyerabend notes that both he and Kuhn admit to what Feyerabend calls the methodological "principle of tenacity", which he defines as the scientist's selection from a number of theories one which promises in the particular scientist's view to lead to the most fruitful results, and then sticking to the selected theory even if the anomalies it suffers are considerable.

He then asks how this principle can be defended, and how it is possible to change allegiance to paradigms in a manner consistent with it. He answers that the principle of tenacity is reasonable, because theories are capable of development and may eventually be able to accommodate the anomalies that their original versions were incapable of explaining. This is because relevant evidence depends not only upon the theory, but also upon other subjects, which are conventionally called "auxiliary sciences". Such auxiliary sciences function as additional premises in the derivation of testable consequences, and these premises "infect" the observation language in which the testable consequences are expressed, thereby providing the very concepts in terms of which experimental results are expressed. But it happens that theories and their auxiliary sciences often develop out of phase, with the result that apparently refuting instances may turn out not to indicate that a new theory is doomed to failure, but instead may indicate only that it does not fit in at present with the rest of science.

Therefore scientists can tenaciously develop methods which permit them to retain their theories in the face of plain and unambiguously refuting facts, even if testable laws for the clash with facts are not immediately forthcoming. The significance of the principle of tenacity, the practice whereby scientists no longer use recalcitrant facts for removing a theory, is that a plurality of alternative theories can coexist in a science at any given time. This pluralism is strategic to Feyerabend, because in his view the fact that theory determines observation implies that theories are not compared with nature, but must be compared with other theories. Alternative theories function to accentuate the differences between one another, such that the principle of tenacity itself may eventually urge the elimination of a theory. Hence, if a change of paradigms is the function of normal science then one must be prepared to introduce alternatives to a given theory. Feyerabend notes that in fact Kuhn himself has described in detail the magnifying effect which alternatives have upon anomalies, and has explained how revolutions are brought about by such magnifications.

Thirdly Feyerabend therefore proposes a second methodological principle, the "principle of proliferation", and he asks rhetorically, why not start proliferating theories at once, and why allow a purely normal science, as Kuhn conceives it, ever to come into existence? Feyerabend replies to his own rhetorical question about theory proliferation vs. normal-science consensus, and switches from a purely methodological perspective to an historical one.

Using his two methodological principles of tenacity and proliferation to examine the history of science, he maintains that normal science is a “big myth”. He argues that even though there are scientists who practice puzzle-solving normal science, there is no temporally separated periods of monistic normal science and pluralistic revolutionary science. He supports a view initially proposed by Imre Lakatos, a professor of logic at the University of London that the practices of tenacity and proliferation do not belong to successive periods in the history of science, but rather are always copresent. Feyerabend says that the interplay between tenacity and proliferation is an essential feature of the actual, historical development of science. It is not the puzzle-solving activity that is responsible for the growth of knowledge, but the active interplay of a plurality of tenaciously held views. It is the continuing intervention of new ideas and the attempts to secure for them a worthy place in the competition that leads to the overthrow of old and familiar paradigms.

Feyerabend furthermore maintains that revolutions are basically matters of appearance, and that during a revolution there is actually no profound structural change such as a transition from normal to extraordinary science as described by Kuhn. Thus, instead of advocating conformity to a monolithic consensus paradigm, as Kuhn does, Feyerabend issues what he calls a “plea for hedonism”, by which he means the continuing practice of the theory-proliferating principle of tenacity.

Feyerabend also took occasion to comment more favorably on Kuhn’s philosophy, and to relate Kuhn’s views to his own where they manifest similarities. One aspect of Kuhn’s philosophy that Feyerabend considers to be important is the concept of paradigm. Feyerabend says that Kuhn expanded on Wittgenstein’s criticism of the logical positivists’ emphasis on rules and formal aspects of language, and that Kuhn made this criticism more concrete. He also says that by introducing the notion of paradigm, Kuhn stated above all a problem. Kuhn explained that science depends on circumstances that are not described in the usual accounts, that do not occur in science textbooks, and that have to be identified in a roundabout way. However, most of Kuhn’s followers, especially in the social sciences, did not recognize the idea as a statement of a problem, but regarded Kuhn’s account as a presentation of a new and clear fact. Feyerabend maintains that by using the term “paradigm”, which is awaiting explication by research, as if explication had already been completed, social scientists started a new and most deplorable trend of loquacious illiteracy.

Feyerabend finds three noteworthy aspects in Kuhn’s treatment of the relations between different paradigms. (1) Different paradigms use sets of concepts that cannot be brought into the usual logical relations of inclusion, exclusion, or overlap, and that incommensurability is the natural consequence of identifying theories with paradigms or, as Feyerabend calls them, traditions. (2) Different paradigms make researchers see things differently, such that researchers in different paradigms not only have different concepts, but also have different perceptions. (3) Paradigms have different methods including intellectual as well as physical instruments for practicing research and evaluation results. Feyerabend says that it was a great advance to replace the idea of theory with the idea of paradigm, which includes dynamic aspects of science. He notes that his earlier work had principally been concerned only with the first of the three mentioned aspects, and then only with theories. As it happens, however, Kuhn later substituted “theory” for “paradigm”. Shapere’s criticism may explain why.

Shapere’s Criticism of Kuhn’s Concept of Paradigm

Dudley Shapere argues that Kuhn’s concept of paradigm is so vague as to be of questionable explanatory value, and he also rejects the relativism he finds in the concept of incommensurability. He wrote a critical review of Kuhn’s *Structure of Scientific Revolutions* in the *Philosophical Review* (July 1964), and shortly later wrote a critique of the philosophies of both Kuhn and Feyerabend in “Meaning and Scientific Change” in *Mind and Cosmos* (ed. R.G. Colodny, 1966). Unlike the criticisms of Popper and Feyerabend that are principally directed at Kuhn’s new concept of the aim of science, Shapere’s criticism is directed at Kuhn’s semantical views, and particularly at Kuhn’s thesis of pre-articulate meaning set forth in the concept of paradigm.

Shapere finds particularly perplexing Kuhn’s thesis that paradigms cannot be formulated adequately or articulated completely. He objects that if all that can be said about paradigms and scientific development can and must be said only in terms of what are mere abstractions from paradigms, as Kuhn maintains, then it is difficult to see what is gained by appealing to the notion of a paradigm. He notes that in most of the cases that Kuhn discusses the articulated theory is doing the job that Kuhn assigns to the paradigm, yet in Kuhn's thesis the theory is not the same as the paradigm.

Shapere says that Kuhn discusses the theory in these cases, because it is as near as he can get in words to the inexplicable paradigm. He therefore asks how can historians know that they agree in their identification of the paradigms in historical episodes, and so determine that the same paradigm persists through a long sequence of such episodes. Where, he asks, does one draw the line between different paradigms and different articulations of the same paradigms? On the one hand it is too easy to identify a paradigm, and on the other hand it is not easy to determine in a particular case what is supposed to have been the paradigm in that case. The inarticulate status of the paradigm makes individuation of the paradigm problematic.

Shapere concludes that in Kuhn's theory anything that allows science to accomplish anything at all can be part of or otherwise somehow involved with a paradigm, with the result that the explanatory value of this concept of paradigm is suspect. He maintains that this idea of shared paradigms, which are purportedly behind historically observed common factors that guide scientific research for a period of years, appears to be guaranteed not so much by a close examination of actual historical cases, as by the breadth of definition of this term "paradigm". He furthermore questions whether such paradigms even exist, since the existence of similarities among theories does not imply the existence of a common paradigm of which the similar theories are incomplete articulations. Shapere thus rejects what he calls the "mystique" of the single paradigm.

In addition to criticizing Kuhn's concept of paradigm Shapere also criticizes the thesis of incommensurability. He maintains that Kuhn offers no clear analysis of meaning, and therefore no clear analysis of meaning change. The principal problem that he finds with the incommensurability thesis advocated both by Kuhn and by Feyerabend is that it destroys the possibility of comparing theories on any grounds whatsoever. He asks: if the incommensurable paradigms differ in all respects including the facts and the problem itself, then how can they disagree? Why do scientists accept one of them as better than the other? Neither Kuhn nor Feyerabend in Shapere's view succeeds in providing any extratheoretical basis for comparing and for judging theories and paradigms. The result he says is historical relativism.

Shapere proposes a resolution. He notes that the thesis of incommensurability requires that two expressions or sets of expressions must either have precisely the same meaning or else they must be utterly and completely different. He proposes what he calls a "middle ground" by altering this rigid notion of meaning. He proposes that meanings may be similar, such that they may be comparable in some respects even as they are different in other respects, and thus may be said to have *degrees* of likeness and difference.

Kuhn Replies

In "Reflections on My Critics" in *Criticism and the Growth of Knowledge* Kuhn replies to his critics. ***Firstly*** Kuhn distances himself from the sociologists. He states that in this matter he agrees with Popper; he says the received theories of sociology and psychology are "weak reeds" from which to weave a philosophy of science, and he adds that his own work no more relies on current sociological theory than does Popper's. But he still maintains that his theory of science is intrinsically sociological, because whatever scientific progress may be, it is necessary to account for it by examining the nature of the scientific group, discovering what it values and what it disdains. Scientists must make decisions. They must decide what statements to make unfalsifiable by *fiat* and which ones will not be considered unfalsifiable. Using probability theory they must decide upon some probability threshold below which statistical evidence will be held to be inconsistent with theory. And they must decide when a research programme is progressive in spite of anomalies, and when it has become degenerative due to them.

He states that answers to such questions require a sociological type of analysis, because they are ideological commitments that scientists must share, if their enterprise is to be successful. So, the unit of investigation is not the individual scientist, but rather is the nonpathological, normal scientific group. He adds that while group behavior is affected decisively by the shared commitments, individuals will choose differently, due to their distinctive personalities, education, and prior patterns of professional research, and that these individual considerations are the province of individual psychology. And he adds that he agrees with Popper in rejecting any rôle for individual psychology in philosophy of science.

Secondly Kuhn addresses what Feyerabend called the ambiguity of presentation, the ambiguity between the descriptive and the prescriptive. He replies that his book should be read in both ways, because a theory of science that explains how and why science works must necessarily have implications for the way in which scientists should behave, if their enterprise is to flourish. He states that if some social scientists have gotten the idea that they can improve the status of their field by firstly legislating agreement on fundamentals and then turning to puzzle solving, they have misunderstood him. Kuhn states that maturity comes to those who know how to wait, because a field gains maturity when it has achieved a theory and technique that satisfy four conditions that he sets forth. (And it might be noted parenthetically that the practices recommended in Kuhn's four conditions are quite different from the practices prevailing in contemporary academic sociology). Those four conditions are as follows:

(1) Popper's demarcation criterion must apply, such that concrete predictions emerge from the practice of the field.
(2) Predictive success must be consistently achieved for some subclass of the phenomena considered by the field.
(3) The predictive technique must have roots in the theory, which explains the limited success, and which suggests means for improvement in both scope and precision.
(4) The improvement in predictive technique must be a challenging task demanding high talent and dedication.

Thirdly the statement of these four conditions leads to Kuhn's defense of his normal-science thesis. He states that these conditions are tantamount to a good scientific theory, and he maintains that with such a theory in hand the time for criticism and theory proliferation has past. The scientist's aim, then, is to extend the range and precision of the match between existing experiment and theory, and to eliminate conflicts both between the different theories employed in their work and between the ways in which a single theory is used in different applications. These are the types of puzzles that constitute the principal activity of normal science. And Kuhn says that the difference between him and Popper on this issue of criticism is only one of emphasis.

Fourthly Kuhn takes up the topic of semantic incommensurability that he used to explain the communication breakdown occurring during revolutionary science. And he also discusses the topics of irrationality in theory choice and of historical relativism that his critics find implied in the incommensurability thesis. His thesis is that the communication problem is not one of complete breakdown and that partial communication occurs. Nevertheless Kuhn maintains a version of the incommensurability thesis. He says that a point-by-point comparison of two successive theories demands a language into which at least the empirical consequences of both theories can be translated without loss or change, and he denies that there exists such a theory-independent, semantically neutral observation language that would enable such a comparison. He states that Popper's basic statements function as if they have this neutral character, and he joins Feyerabend in stating that there is no neutral observation language, because in translating from one theory to another, the constituent words change their meanings and applicability in subtle ways.

But Kuhn adds that for him "incommensurable" does not mean "incomparable", and in this respect he departs from Feyerabend's incommensurability thesis. In Kuhn's view the fact that translation exists, suggests that recourse is available to scientists who hold incommensurable theories. His explanation for the fact that communication is only partial and the fact that translation is difficult is given in terms of his concept of paradigm. The paradigm is pre-articulate knowledge that functions as an example that enables the scientist to recognize similar cases without having to articulate or to characterize the similarity relations explicitly in a generalization. He states that the practice of normal science depends on a learned ability to group objects and situations into similarity classes, which are "primitive" in the sense that the grouping of objects is done without supplying an answer to the question, "similar with respect to what?" In scientific revolutions some of the similarity relations change, such that objects, which are grouped in a set are regrouped into different subsets than before. The example given by Kuhn of grouped objects is the sun, the moon and the stars that were regrouped in the transition from the Ptolemaic to the Copernican celestial theory. As it happens Feyerabend does not consider the transition to the Copernican celestial theory to be a case of semantic incommensurability.

Kuhn states that partial communication occurs, because in such a redistribution of similarity sets two men whose discourse had previously proceeded with full understanding may suddenly find themselves responding to the same stimulus with incompatible descriptions or generalizations. He maintains that scientists experiencing communication breakdown can discover by continued discourse the areas where their disagreement occurs, and what the other person would see and say, when presented with a stimulus to which his visual and verbal response would be different. With his theses of partial communication and of incommensurability-with-comparability, Kuhn believes he can escape his critics' claims that his views of theory choice are irrational and that he is an historical relativist. He still maintains that there is an element of conversion in theory choice, because in the absence of a semantically neutral observation language the choice of a new theory is a decision to adopt a different language, and to deploy it in a correspondingly different world.

In any debate over theory choice neither party has access to an argument that is compelling like logical or mathematical proofs. But their recourse to persuasion is for "good reasons", such as accuracy, scope, simplicity, or fruitfulness. These good reasons are the group's shared values, but not all scientists in the community apply these values in the same way. Consequently there will be variability that occasions revolutions. This is Kuhn's answer to Feyerabend's principal criticism: No principle of theory proliferation need be invoked to explain the transition to crisis and revolution, because unanimity of values will nonetheless produce the multiplicity of views that brings on the transition from normal to revolutionary science. Variability in the application of uniform values produces variability in theories during normal science.

Kuhn, Normal Science, and the Academic Sociologists

Feyerabend's comments about sociologists' uncritical embracing of Kuhn's views are well founded. While Kuhn faced a veritable fusillade from philosophers of science, he was received with unrestrained euphoria by American academic sociologists. Monsieur Jourdain, the *parvenu* in Moliere's comedy, *Le Bourgeois Gentilhomme*, had aspired to write prose, and was delightedly surprised, when he was told that he had been speaking prose for more than forty years without knowing anything about it.

Moliere's play has its analogue in contemporary American academic sociology save for the absence of any comedy. The prevailing opinion among researchers in the more mature scientific professions is that sociology is merely a pretentious *parvenu* with a literature of platitudes expressed in jargon. Academic sociologists have longed to demonstrate the manifest scientific progress that the more mature scientific professions have routinely exhibited in their histories. Consequently like Monsieur Jordain, sociologists were delightedly surprised when Kuhn effectively told them that they have been theorizing about the conditions for scientific progress for years without knowing anything about it. Sociologists did not have to be told how to practice Kuhn's doctrine of enforced consensus; it had long been an accepted practice endemic to their profession. They had only to be told that social conformism is a new philosophy of science that produces progress. Specifically they believed he had told them that his sociological thesis of normal science describes the conditions for the transition of social sciences from "preparadigm" status to "mature" status.

In several places in his writings Kuhn maintains that the social sciences are immature sciences, because they do not have consensus paradigms that enable them to pursue the puzzle-solving type of research that characterizes normal science. In his "Postscript" in *Structure of Scientific Revolutions* he states that the transition to maturity deserves fuller discussion from those who are concerned with the development of contemporary social science. Not coincidentally none were more concerned with such a transition than the professionally insecure and institutionally retarded sociologists. And ironically as the custodians and practitioners of the theory of consensus and conformity, none have thought themselves more professionally and institutionally suited for such discussion. Thus the paradox: notwithstanding the mediocrity of their own science's accomplishments, sociologists deluded themselves into believing that they are experts in the practices of normal basic-scientific research.

Warren O. Hagstrom's *The Scientific Community* (1965) represents a paradigmatic example of Kuhn's influence on sociologists. This book written by a sociologist and referenced later by Kuhn in support of his own views, is a study of how the forces of socialization by professional education and of social control by colleagues within a scientific community, operate to produce conformity to scientific norms and values. The concepts of socialization and social control are as fundamental to sociology as the concepts of supply and demand are to economics. Just as Kuhn attributed institutional status to the prevailing paradigm, so too, Hagstrom identifies the norms and values of science with currently accepted substantive views, and he therefore says that substantive disputes in a scientific community are a type of "social disorganization". "Disorganization" is as pejorative a term in sociology as "depression" is in economics. Hagstrom identifies his theory as a functionalist theory, and in functionalist sociological theory social disorganization is viewed as symptomatic of a pathological condition known as institutional disintegration.

Hagstrom mentions two types of social-control sanctions that operate in the scientific community to produce the requisite conformity to the norms and values. They are firstly refusal to publish papers in the professional journals and secondly denial of opportunity for occupational advancement such as tenure. Kuhn and Hagstrom are a mutual admiration society unto themselves. Hagstrom acknowledges Kuhn's influence in his preface, and he references and quotes passages from Kuhn in several places in the book, particularly where Kuhn discusses professional education in mature sciences. And Kuhn in turn later references Hagstrom's book in "Second Thoughts" and in the "Postscript" in support of his theses.

Kuhn's influence on sociologists was manifested in the sociological journals also. Shortly after Kuhn's *Structure of Scientific Revolutions* there appeared a new sociological journal, *Sociological Methods and Research.* In a statement of policy reprinted in every issue for many years the editor states that the journal is devoted to sociology as a "cumulative" empirical science, and he describes the journal as one that is highly focused on the assessment of the scientific status of sociology. One of the distinctive characteristics of normal science in Kuhn's theory is that it is cumulative, such that it can demonstrate progress.

In "Editorial Policies and Practices among Leading Journals in Four Scientific Fields" in *Sociological Quarterly* (1978) Janice M. Beyer reported her findings from a survey of the editors of several academic journals. These interesting findings reveal three significant differences between the editorial policies of the journals of the physics profession and those of the sociological profession. They are:

(1) the acceptance rate for papers submitted to sociological journals is thirteen percent, while the rate for physics journals is sixty-five percent;
(2) the percent of accepted papers requiring extensive revision and then resubmitted to referees is forty-three percent for sociological journals and twenty-two percent for physics journals; and
(3) the percent of accepted papers requiring no revision is ten percent for sociological journals and forty-six percent for physics journals.

The scientist who is not a sociologist may reasonably wonder whether sociologists are really as professionally ill-prepared to contribute to a professional scientific literature as these findings would indicate, or whether there is something Orwellian in this enforced practice of extensive revision of purportedly scientific findings as a condition for publication. In fact both conditions obtain.

But Beyer explains her findings in terms of Kuhn's thesis of normal science, and attributes the reported differences in editorial practices to differences in paradigm development. She states that sciences having highly developed paradigms use "universalist" criteria for scientific criticism, which she defines as the belief that scientific judgments should be based on considerations of scientific merit, where "merit" in her text is described as conformity with a consensus paradigm. Understood in this manner, universalism is just an imposed conformism that is indifferent to the distinction between contrary evidence and the contrary opinions of author, editor and referees.

Ironically the outcome of the self-conscious attempt to make sociology a "mature" science practicing normal science with an enforced consensus paradigm was something quite different than what Kuhn's philosophy had described. Kuhn's philosophy described a consensus paradigm that is empirical, so that it can produce anomalies which initially are ignored, but which eventually accumulate and spawn revolutionary alternative theories. But as exhibited in Appendix II of BOOK VIII in this web site, what has actually happened is that sociologists impose social controls upon the members of their profession, in order to enforce conformity – not to an empirical theory, but to a philosophy of science, notably the German romantic philosophy introduced into American sociology by Talcott Parsons. This philosophy, which Parsons brought to Harvard University from the University of Heidelberg in Germany, where he was influenced by the views of Max Weber, was to supply the philosophical foundations for his "functionalist" sociology, or at least for his own variation on functionalism. Even though his functionalist sociology has now waned, Parson's romantic philosophy with its social-psychological reductionism to "motivational analyses" continues to haunt American academic sociology.

Not only did the sociologists get things mixed up, when they adopted a philosophy instead of an empirical theory for their consensus paradigm, they furthermore got things backwards. While the natural sciences rejected positivism and then moved forward to the post-positivist philosophy of contemporary pragmatism, sociologists rejected positivism and then moved backward to the pre-positivist philosophy of romanticism. This contrast has its origins in the different histories of physics and sociology. Sociology is a new science with no noteworthy empirical accomplishments to supply its academic culture with precedent. Physics on the other hand has a long and glorious history of accomplishments; the historic scientific revolution started with the astronomy of Copernicus and was consummated with the celestial mechanics of Newton. When the twentieth-century revolutions in physics, namely relativity theory and quantum theory, revealed the inadequacies in the early positivism, the physicists did what they had previously found successful: they embraced the pragmatically more successful theory on the basis of its empirical test outcomes alone, rejected the semantics and ontology described by its predecessor, and attempted to cope with the anything-but-intuitive or commonsense semantical interpretation and ontology of the radically new physics. Furthermore in the twentieth century this practice had become sufficiently routine that the physicists were able to recognize and articulate these reactions.

It took the philosophers of science, however, decades to recognize the physicists' practice of basic research by articulating the new systematic philosophy of language, which today defines the contemporary pragmatist philosophy. The contemporary pragmatist philosophy of science fundamentally differs from both positivism and romanticism, because both of these latter include semantical and ontological considerations in their criteria for scientific criticism. They differ between one another only about which types of ontology they will accept: the positivists (*i.e.*, behaviorists) reject all "mentalism" in social and behavioral science, while the romantics require reference to subjective views and values. The contemporary pragmatists on the other hand subordinate all semantical and ontological commitments to the empirical adequacy of the scientific law or theory, a view now known as "scientific realism", even if some such as Kuhn view empirical criticism to be less conclusively decidable than do earlier philosophers such as Popper. And the result of subordinating semantics and ontologies to the outcomes of empirical criticism is that the semantics and ontologies change as science develops. Science is indeed "subjectless", as Popper said.

Ironically the philosophy of science that the contemporary sociologists impose upon their membership is not only anachronistic but is also at variance with the philosophy that Kuhn uses for his philosophical interpretation of the history and dynamics of science. The followers of

Parsons accepted Weber's *verstehen* concept of social science law, whereby empathetic plausibility that that they find makes theories "convincing" is the principal criterion for scientific criticism. Whatever one may think of Kuhn's solution to the problem of scientific belief and the thesis of the consensus paradigm that constitutes his solution to this belief problem, the issue of freely ignoring empirical anomalies in normal science becomes moot, when there can be no empirical anomalies. The *verstehen* criterion reduces scientific criticism to what one or another particular critic finds intuitively acceptable, empathetically plausible, or otherwise comfortably familiar and "convincing", however covert or idiosyncratic to the particular critic. It reduces criticism to quarrels about intuitions; empirically adequate work is rejected out of hand, if it doesn't "make substantive sense" according to the intuition of the particular critic.

Sociologists' institutional criterion may be contrasted with empirical criticism in modern physics. When modern physicists were confronted firstly with Einstein's relativity theory and then with Heisenberg's indeterminacy relations, their profession in each case decided to accept the new physics, because it is more empirically adequate in spite of the fact that it is anything but intuitively familiar or platitudinous. This is not possible even today in American academic sociology, and consequently sociologists can make no distinction between contrary empirical evidence and contrary intuitive opinion.

Parsons had never referenced Kuhn, and probably never read him; he had his own agenda for sociology long before Kuhn. The enforced consensus about Parson's sociology may be explained in part by the appointment of Parsons to the presidency of the American Sociological Association (ASA). In his *The Coming Crisis of Western Sociology* (1970) the sociologist Alvin W. Gouldner, Max Weber Research Professor of Social Theory at Washington University, St. Louis, observed that Parsons used this position to influence the appointments to other executive positions in the ASA including most notably both the ASA's Publications Committee and the position of editor of its *American Sociological Review*. Gouldner reports a "continuity-convergence ideology" that produced a blanketing mood of consensus that smothers intellectual criticism and innovation.

However, no conspiracy theory involving Parsons could adequately explain the sociologists' willingness to adopt his distinctive "functionalist" sociology and its associated German romantic philosophy of science. The doctrinairism of the American sociological profession and its receptivity to Parson's romanticism is **firstly** explained by the thesis of the functionalist sociological doctrine itself. The central thesis of his functionalist doctrine is that social controls producing conformity to a consensus of views and values explain the existence of social order in any group. And this in turn implies that failure to conform is dysfunctional in a pejorative sense of being disorderly even to the extent of threatening complete disintegration of the group. Advocates of Parsons' functionalist sociology could not easily escape the inclination to apply these concepts to their own profession with Parsonsonian functionalism itself serving as the consensus view, and to persuade themselves that Kuhn's theory of the development of empirical science is a logical extension of the Parsonsonian functionalist sociology. Thus contemporary academic sociologists not only believe that social conformity to a consensus paradigm in the scientific community functions to produce social order in the profession, thanks to Kuhn's philosophy they also believe that it functions to produce scientific progress.

Secondly Kuhn's theory made its appearance at an opportune time. Lundberg's initially popular positivist program for American sociology had waned, because it never got beyond the stage of a programmatic proposal, and years earlier Parsons had launched his distinctive functionalist sociology from the prestigious platform provided by his faculty position as chairman of the sociology department at Harvard University. When Kuhn's sociological thesis of progress in science appeared, the *parvenu* scientific profession seeking acceptance among the empirical sciences was predisposed to impose some progress-producing consensus paradigm. The outcome of this combination of Parsonsonian romanticism and Kuhnian "normal science" has been a chimerical science, a romantic "folk" sociology that is about as normal as the gothic caricature of science depicted by Shelley's character, Victor Frankenstein – a romantic grotesque fully deserving the epitaph "American Gothic" sociology.

As it happens, American Gothic sociology seems to have become the appalling specter both to prospective sociology students and to sociology students' prospective employers. In its *Science and Engineering Doctorates* the National Science Foundation (NSF) has released statistics revealing a thirty-nine percent decline in the number of doctoral degrees in sociology earned annually in the United States since 1976. This compares with a nearly seven percent growth in doctorates for all sciences during the same period. The NSF also reports that the median age of receipt of the doctorate in social science is between thirty-two and thirty-three years. And since the post-World War II "baby-boom" years of rising aggregate number of births did not end until 1961, it is clear that American academic sociology has been in decline during a period in which the pool of potential students has been rising. Thus sociology's decline is not merely a demographic phenomenon circumstantial to the history of the profession. It is the result of a pathological condition intrinsic to the American sociological profession's institutional values, normative standards, and research practices.

More recently in "Education for Unemployment" Margaret Wente reported in the *Globe and Mail* (15 May 2012) that there are currently three sociology graduates for every sociology job opening, and she concludes that sociology students have been "sold a bill of goods". And later (1 January 2015) she lamented sociology professors who are fooled into thinking they might have a shot at the ever-shrinking tenure track, and who if successful will be "masters of pulp fiction". For those who have gone into debt to earn the sociology Ph.D., the credential is a white elephant and the debt he is carrying is a dead horse. Any student who assumes heavy financial debt for a doctorate is tragically naïve.

Kuhn's Linguistic Analysis of Incommensurability

Philosophers of science such as Feyerabend typically start with linguistic analysis. But Kuhn was a historian of science, and he firstly wrote his interpretative description in history of science. Only after many years did he attempt any language analysis to explain and defend his thesis of semantic incommensurability, even though it is a thesis in philosophy of language. In the years following *Structure of Scientific Revolutions* his incommensurability thesis evolved considerably, but Kuhn never repudiated it, because it is the keystone for his philosophy of science, without which his metatheory collapses. It is the keystone that separates and supports his correlative ideas of normal and revolutionary science together with all their philosophical, methodological, and sociological concomitants. Pull away this keystone and his normal-revolutionary dichotomy would differ only in degree without the discontinuity that incommensurability supplies, thus collapsing his distinctive thesis of scientific revolution.

Kuhn's attempts at language analysis expressed in his later papers have been collected and published as a volume titled *The Road Since Structure* (2000), and in the chapter titled "Afterwords" he states that his efforts to revise and refine his incommensurability thesis have been his primary and increasingly obsessive concern for thirty years, during the last five of which (since 1987) he has made what he calls a rapid series of "significant breakthroughs". Thus it is in his later papers that his definitive statements are to be found. But Kuhn seems not to have been comfortable with philosophers' linguistic analysis. The knowledgeable reader of *Road Since Structure* will find himself struggling through Kuhn's lengthy, laborious, loquacious successive re-inventions of his incommensurability thesis, much as Kuhn himself struggled with language analysis to recast, revise and rescue his semantic incommensurability thesis.

In his autobiographical interview in 1999 he reports that he took the idea of incommensurability from mathematics, where he firstly encountered it in high school while studying calculus and specifically while pondering the proof for the irrationality of the square root of the number two. In another statement of the idea set forth in his "Commensurability, Comparability, Communicability" (1987) reprinted in *Road Since Structure* he gives other common examples of incommensurability from mathematics: The hypotenuse of an isosceles right triangle is incommensurable with its side; the circumference of a circle is incommensurable with its radius. He notes that these cases are incommensurable because there is no unit of length contained without residue an integral number of times in each member of the pair. Mathematicians say these magnitudes have no common integer divisor except the number one. In mathematics "incommensurability" means there is no common measure, and for his semantic incommensurability Kuhn substitutes "no common language" for "no common measure" for metaphorical use in his *Structure of Scientific Revolutions*.

Initially in *Structure of Scientific Revolutions* Kuhn's discussions of incommensurability were vague. He reports that he relied on intuition and metaphor, on the double sense – visual and conceptual – of the verb "to see." In his "Commensurability, Comparability, Communicability" he noted that his view of revolutionary change has been increasingly modified. He said that his concept of a scientific revolution originated in his discovery that to understand any part of the science of the past, the historian must first learn the language in which it was written, and that the language-learning process is interpretative. And he maintains that success in interpretation is achieved in large chunks involving the sudden recognition of the new patterns or *gestalts*, and that the historian experiences revolutions. In the autobiographical interview he noted that in *Structure of Scientific*

Revolutions he had very little to say about meaning change, and instead following Russell Hanson he relied on the idea of *gestalt* switch, but now (as of the time of the 1999 interview) he says that incommensurability is *all* language [italics in the editor's text], and also that it is associated with change of values since values are learned with language. Early reviewers of *Structure of Scientific Revolutions* understood Kuhn's use of "incommensurability" to mean that it is not possible to define *any* of the terms of one theory into those of the other. And Kuhn admits that careful reading of *Structure of Scientific Revolutions* reveals nothing other than this wholistic view, because he explicitly rejected the positivist theory-neutral observation language thesis, and incommensurability strategically precludes any neutral, *i.e.*, theory-independent, observation language.

But as critics noted in *Criticism and the Growth of Knowledge*, the wholistic interpretation makes both scientific communication and scientific criticism insolubly problematic. In response to these criticisms in *Criticism and the Growth of Knowledge* Kuhn announced his thesis of partial or "local" incommensurability, which enables continuity, comparability, and partial communication between theories outside the area of incommensurability in episodes of revolutionary change. In the "Postscript" to his "Possible Worlds in History of Science" reprinted in *Road Since Structure* he explicitly denies in response to a later critic that the change from one theory to another is a discontinuous change, and he says that he has since reformulated his past view which had invoked discontinuity.

Kuhn believes that historians dealing with old scientific texts can and must use modern language to identify the referents of the out-of-date terms. In "Metaphor in Science" reprinted in *Road Since Structure* he explained the referential determination that offers continuity with his "causal theory of reference". The causal theory of reference denies that proper names have definitions or are associated with definite descriptions. Instead a proper name is merely a label or a tag, and to identify the individual, one must point it out ostensively, or use some contingent fact about it, or locate its lifeline. Kuhn extends this theory to naming natural kinds by adding that multiple ostensions (examples) are needed instead of just one, in order to see similarities and contrasts with other individuals. Illustrating his thesis again in the Copernican revolution he says the techniques of dubbing and of tracing lifelines permit astronomical individuals, *e.g.*, the earth, and the moon, Mars, and Venus, to be traced through episodes of theory change.

The lifelines of these four individuals were continuous, but they were differently distributed among natural families as a result of that change.

Kuhn does not further elaborate the causal theory of reference, and in his autobiographical interview he said that the causal theory of reference does not work for common nouns, but it has some survivals in his philosophy of meaning. Thus in “Afterwords” he says that one of the characteristics of “kind words” is that they are learned in use by being shown multiple examples of the referent that supply expectations of things and general concepts of properties of the world. He acknowledges many philosophers maintain that reference is not possible without using concepts to characterize the referent.

Later he further elaborates his theory of referential determination in his “Commensurability, Comparability, and Communicability” reprinted in *Road Since Structure*, where he distinguishes reference determination from translation. He says that “no common language” means that there is no language for which either theory in a revolutionary transition can be translated into the other. While most of the terms common to the successive theories function in the same way for both theories, such that their meanings are preserved and admit to translation, there is a small group of mutually interdefined terms that are incommensurable. The terms that preserve their meanings across a revolutionary transition provide a sufficient basis for discussions of differences and for comparisons for theory choice. But he acknowledges that it is not clear that incommensurability can be restricted to a local region of discourse, because the distinction between terms that change meaning and terms that preserve meaning is difficult to explicate.

He then attempts to evade this problem with his thesis of coreferencing discussed below, but he does not solve it. In “The Trouble with the Historical Philosophy of Science” reprinted in *Road Since Structure* he states that the rationality for the scientist’s conclusions requires only that the observations invoked be neutral for or shared by the members of the group making the decision, and for them only at the time the decision is being made. This thesis offers a neutral language of preserved meanings, which supplies historical continuity and is neutral relative to the time of the revolutionary transition and for the affected scientific group. But he says that this neutral language is not the same as the positivist observation language, and he rejects the existence of any “Archimedean platform outside space and time”. In “Afterwords” he states that it is “kind words” that enable identification of referents, things that between their origin and demise have a lifeline through space and time. "Kind" words constitute the "lexicon" that is strategic to his thesis of incommensurability.

Kuhn offers two reasons for incommensurability. The ***first*** reason is stated in his rejection of translatability stated in his "Commensurability, Comparability, Communicability", where he defines translation as something done by a person who knows two languages, and who systematically substitutes words or strings of words in one language into the other, in order to produce an equivalent text – *i.e., salva veritate*. He denies that the two successive theories in a scientific revolution can be translated into one another. This is obviously true in the sense that the two theories make contrary claims, but Kuhn's reason is not contrariety but rather incommensurability, and the thrust of his thesis is that one theory cannot even be *expressed* in the vocabulary of its successor nor vice versa. Kuhn maintains that the new theory must be "interpreted", which in his terminology means "learned." The interpreter needs to know only one language, and he confronts another language as unintelligible noises and inscriptions. Quine's radical translator in *Word and Object* is not a translator but an interpreter, because successful interpretation is learning a new language. The interpreter must learn to recognize distinguishing features initially unknown to him, and for which his own language supplies no descriptive terminology. Thus incommensurability is due to semantics that is unavailable in one language but available in another.

Kuhn attempts to illustrate this kind of incommensurability in the transition from the phlogiston theory of combustion to the modern oxygen theory. In the phlogiston theory the phrase "dephlogisticated air" can mean either oxygen or oxygen-enriched air, while the phrase "phlogisticated air" means air from which oxygen has been removed. In the phrase "phlogiston is emitted during combustion", the term "phlogiston" refers to nothing, although in some cases it refers to hydrogen. Kuhn maintains that for the historian of science incommensurability in this case is dealt with by learning the meanings in the old texts by reference determination. He agrees that historians dealing with old scientific texts can and must use modern language to identify referents of out-of-date terms. Like the native's pointing out "gavagai" referents in the radical translation situation described by Quine in his *Word and Object*, such reference determinations may provide concrete examples from which the historian can hope to learn the meanings of problematic expressions in the old texts. Presumably in the case of “phlogiston” the reference situation is a repetition of the eighteenth-century chemists’ experiments and the comparison of the old language and the modern one describing the observable experimental outcomes.

But there are some difficulties with this example as described by Kuhn, because he says that translation is impossible since phlogiston is nonexistent, an approach that is nominalist, while Kuhn accepts concepts and rejects nominalism with its purely referential theory of meaning. The existence of a referent is neither the same as nor a condition for meaningfulness, and Kuhn says that he joins Hesse in maintaining that any extensional theory of meaning is “bankrupt.” Furthermore translation is not relevant, since the new and old theories express contrary claims and cannot both be true. But the issue is expressibility, for which both referenceable existence and truth are irrelevant. The expressibility problem due to incommensurability is that the semantical resources needed for the modern theory are not available in the older one. Kuhn does not discuss this first reason for incommensurability again after this paper, which was initially delivered at the Philosophy of Science Association annual meeting in 1982.

Kuhn’s ***second*** reason is that incommensurability is due to semantical or “lexicon restructuring”. Kuhn’s initial statement of this reason is found in his “Commensurability, Comparability, Communicability” in the section titled “The Invariants of Translation.” Here he distinguishes and describes two characteristics of language:

1. Coreferencing. This means that two users of the same language can employ different criteria for identifying the referents of its descriptive terms. Coreferencing requires that each user associate each descriptive term with a “cluster of criteria” including contrast sets of terms. He adds that the sets of terms must be learned together by interpretation, and that this having to learn together is the “holistic” aspect essential to local incommensurability.

2. Structures of criteria. For each language user a referencing term is a node in a lexical network, from which radiate labels for the criteria he uses in identifying the referents of the nodal term. Those criteria tie some terms together and at the same time distance them from other terms, thus building a multidimensional structure within the lexicon. That structure mirrors aspects of the structure of the world, which the lexicon can be used to describe, and it also simultaneously limits the phenomena that can be described with the lexicon. If anomalous phenomena arise, their description and possibly even their recognition will require altering some part of the language, thus restructuring previously constitutive linkages between terms.

In discussing translation Kuhn says that "homologous" structures mirroring the same world may be fashioned using different sets of criterial linkages. What such homologous structures preserve is the "taxonomic categories" of the world and the similarity/difference relationships between them. Different languages impose different structures on the world, and what members of the same language community share is homology of lexical structures, in which the "taxonomic structures" match. The invariants of translation are matching co-referential expressions and identical lexical structures. Translation is impossible if taxonomy cannot be preserved, to provide both languages shared categories and relationships. And when translation is impossible, interpretation, *i.e.*, language acquisition, is required. Finally revolutionary developments in science are those that require taxonomic change, *i.e.*, change in lexical taxonomic structure thus producing incommensurability.

In his "The "Road Since Structure" also reprinted in *Road Since Structure* Kuhn states that the "lexical taxonomy" might be called a "conceptual scheme", which is not a set of beliefs, but rather an "operating mode" of a "mental module" prerequisite to having beliefs, a module that supplies and bonds what is possible to conceive. He also says that the taxonomic module is prelinguistic and possessed by animals. In this respect he calls himself a "post-Darwinian Kantian", because like the Kantian categories the lexicon supplies preconditions of possible experience, while unlike Kantianism the lexicon can and does change. And he adds that underlying these changes there must be something stable and permanent that is located outside space and time, and like Kant's *Ding an sich* is ineffable, inscrutable, and indiscernible.

In "Road Since Structure" and in "Afterwords" Kuhn elaborates further on his idea of lexicon with his thesis of "kind words" or "taxonomic terms", the vocabulary terms contained in the lexicon. He states that they have two properties: 1) they are identifiable by their lexical characteristics, notably their occurrence with an indefinite article, and 2) they are subject to Kuhn's "no-overlap" principle, which is that no two terms with the kind label may overlap in their referents, unless they are related as species to genus, *e.g.*, "male" and "horse" may overlap, but not "horse" and "cow."

Kuhn illustrates his thesis of taxonomic terms and his principle of no overlap in the language of the Copernican revolution. He says that the content of the Copernican statement "planets travel around the sun" cannot be expressed in a statement that invokes the celestial taxonomy of the Ptolemaic statement "planets travel around the earth", and that the difference between the two statements is not simply a matter of fact. The term "planet" appears in both statements as a kind term, and the two kind terms overlap in membership without either containing all the celestial bodies contained in the other (a genus-species relation), such that there is a change in taxonomic categories that is fundamental. Kuhn believes that such overlap could not endure, and says that a redistribution of individuals among natural kinds with its consequent alteration of features salient to reference, is the central feature of the episodes he calls revolutions.

Kind words supply the categories prerequisite to description of and generalization about the world. Periods in which a speech community deploys overlapping kind words end in one of two outcomes: 1) one meaning entirely displaces the other or 2) the community divides into two groups. In the resolution of scientific revolutions the former outcome occurs as a result of the crisis phase. And in the specialization and speciation of new disciplines the latter outcome occurs. The lexicon of various members of a speech community may vary in the expectations that the lexicons induce, but they must all have the same structure or else mutual incomprehension and breakdown of communication will result. What is involved in incommensurability – different lexical structure – can only be exhibited ostensively by pointing out examples; it cannot be articulated, *i.e.*, expressed linguistically.

The term "incommensurability" is also central to the philosophy of Paul Feyerabend, and neither he nor Kuhn had claimed priority for its use. In his autobiographical interview Kuhn claims to have used it independently. In his "Commensurability, Comparability, Communicability" Kuhn relates his use of the term to Feyerabend's. He stated that his use of "incommensurability" was broader than Feyerabend's, while Feyerabend's claims are more sweeping. Kuhn noted that each was led to use the term by problems encountered in interpreting scientific texts, that both were concerned to show that the meanings of scientific terms and concepts such as "force", "mass", "element" and "compound", often changed with changes in the theories that contained them, and that when such theory changes occur it is not possible to define *all* the terms of one theory into the vocabulary of the other. In a footnote Kuhn adds that he restricted incommensurability to a few specific terms. Kuhn said Feyerabend restricted incommensurability to language, while Kuhn initially spoke also of differences in methods, problem-field, and standards of solution. Later in comparing his views with Feyerabend's, Kuhn modified his original idea of incommensurability with his thesis of "local incommensurability."

Kuhn's Philosophy of Science

Although a historian of science, Kuhn said that he had intended his *Structure of Scientific Revolutions* for philosophers of science, and that he was disappointed to find that they did not receive it sympathetically. In response to the philosophers he modified and rather awkwardly evolved his philosophy several times over succeeding decades. But while the sociologists have been smitten with his consensus-conformist criterion, the incommensurability thesis is a semantical thesis, and Kuhn was out of his depth for the linguistic analysis demanded by philosophers.

Of the four basic questions in philosophy of science the most radical aspect of Kuhn's philosophy is his idea of the aim of science due to his view on scientific criticism and his thesis of semantic incommensurability. His historical thesis is twofold: In the "normal" science phase the consensus paradigm, which he later identifies with articulate theory, assumes institutional status, such that scientists aim to conform to the consensus view. Thus conformism is the criterion for scientific criticism, and by ignoring anomalies the empirical criterion is subordinated to this institutionalized criterion of conformism to the prevailing paradigm. "Scientific progress" therefore is understood as uncritical extension of the consensus paradigm.

On the other hand in the revolutionary phase, which is an unintended outcome of the conformist-consensus aim of science, semantic incommensurability between old and new successive theories makes the revolutionary transition such that the empirical criterion for theory choice cannot operate. In response to critics' questions about the decidability of scientific criticism of revolutionary new theories he later developed his thesis of "local incommensurability", which permits incommensurable theories to be compared conceptually and empirically by means of the common vocabulary that somehow falls outside of the range of incommensurability. However, within the area of incommensurable vocabulary the language of the new theory must be learned by multiple ostensive demonstrations and/or by approximate paraphrase.

In response to philosophers' demand that he supply a linguistic analysis explaining his incommensurability thesis, he had evolved his position substantially over the thirty years following *Structure of Scientific Revolutions*. Throughout his life, however, he continued to defend his semantic incommensurability thesis. He gave two reasons for incommensurability: The first is that the language of the new theory contains descriptive semantics incorporating features of the world not recognized by the earlier preceding theory. The second is that the contextual determination of the descriptive terms in the statements of a theory results in a restructuring of the semantics of those terms, the "lexicon" of "kind words" *i.e.*, common nouns, when those same terms are carried into the context of the new succeeding theory.

Kuhn says little about the topic of scientific discovery. He says that he disagrees with Hanson's thesis that there is a logic for scientific discovery, and Kuhn prefers to speak of the circumstances of discovery. He makes no comments about the nature of scientific explanation. Consider next Feyerabend's philosophy of science and specifically his theses of meaning variance and semantic incommensurability.

Nagel and Feyerabend on Meaning Variance

Semantic incommensurability is a special case of the more general semantic phenomenon that Feyerabend calls "meaning variance", the phrase that he uses to refer to semantic change. Accordingly it is instructive to consider firstly Feyerabend's thesis of meaning variance. This thesis is argued in his "Explanation, Reduction, and Empiricism" in *Minnesota Studies in the Philosophy of Science* (1962), where he opposes it to the contrary thesis that he calls meaning invariance, which he finds characteristic of the neopositivist philosophy and specifically of the positivist views of Carl Hempel and Ernest Nagel. Together with Paul Oppenheim, Carl Hempel set forth the "nomological-deductive" thesis of scientific explanation in "Logic of Explanation" in *Philosophy of Science* (April, 1948), and a later statement by Hempel is given in chapters five and six of his *Philosophy of Natural Science* (1966). Nagel set forth his thesis of reduction of theories in chapter eleven of his *Structure of Science* (1961). Hempel and Oppenheim emphasize the logical-deductive nature of scientific explanation of individual events, while Nagel addresses more explicitly the semantical aspect of theoretical explanation and reduction. Since the semantical aspect is at the center of Feyerabend's thesis of meaning variance, a brief consideration of Nagel's discussion of the reduction of theories is in order, to understand what Feyerabend is opposing. As it happens, Nagel might also be said to have a thesis of meaning variance, but his positivist view of semantical change is not the same as Feyerabend's.

Initially the logical positivist interest in reduction was part of the Vienna Circle's Unity of Science program. When it became evident that this program is unmanageably ambitious, the reductionist program was limited to the characteristically logical positivist problem of relating theoretical terms in theories to an observation-language reduction basis. This type of reduction is accomplished by what Carnap called "reduction sentences" and by what Hempel called "bridge principles". Nagel is in the logical positivist tradition, but his treatment of logical reduction is somewhat less programmatic and more closely related to episodic developments in the history of science. And he is more interested in those cases in the history of science, in which a relatively autonomous theory is absorbed by or "logically reduced to" some other more inclusive theory, a type of development that he believes is a recurrent feature of the history of modern science. In this type of episode the set of theoretical statements or experimental laws, as the case may be, that is reduced to another theory is called the "secondary science", while the theory to which the reduction is effected is called the "primary science".

Reductionism is a type of explanation in science, and Nagel explicitly defines it as the explanation of a theory or of a set of experimental laws established in one area of inquiry to a theory formulated in some other domain. He is principally interested in those types of reduction in which concepts are required for describing phenomena in one area that were not formerly employed in the other area, even when the two areas were described with the same vocabulary. He refers to this type of reduction as a "heterogeneous" reduction, because it describes a qualitative dissimilarity between the phenomena in the domains of the two theories involved in the reduction. On the other hand a reduction without different vocabulary and describing a qualitative similarity is what he calls a "homogeneous" reduction. Nagel finds only the heterogeneous type to be problematic.

Nagel employs a theory of meaning in which a descriptive term may have as many meanings as there are explications, which proliferates equivocations. He illustrates his thesis in his examination of the heterogeneous reduction of thermodynamics to statistical mechanics and of the semantics of the term “temperature”, as that term’s meaning is affected by the successful reduction. Even before the reduction is made, there is much to be said about the semantics of the terms involved, because a term such as “temperature” has several meanings resulting from overtly performed instrumental operations. Nagel exemplifies the multiple meanings of the term “temperature” by noting that a person who understands temperature in terms of an ordinary mercury thermometer would have difficulty understanding what is meant by a temperature of fifteen thousand degrees, if he also knew that no mercury thermometer could be used to measure such an extreme temperature. But if the person had studied physics, he would discover that the term “temperature” in physics has a broader application from a more embracing set of rules of usage describing other measurement procedures.

Nagel invokes Paul W. Bridgman’s idea of “operational definitions” set forth in the latter’s *Logic of Modern Physics* (1927), and states that such rules of usage are explications aimed at specifying the meanings of descriptive expressions such as “temperature” in terms of other observable ones, which in any given context must be traced to certain descriptive expressions that are selected to be observable primitive expressions. It is noteworthy that in Nagel’s theory of semantical specification as in Bridgman’s, each such specification describing an alternative measurement procedure constitutes a cognitively distinct meaning of the observation term. Yet these multiple meanings are not unrelated, since the diverse measurement procedures will yield the same measurement values where more than one is deemed applicable. Thus the term is *empirically* unambiguous while at the same time it is *cognitively* (*i.e., semantically*) equivocal. Nagel extends Bridgman’s semantical thesis for observation terms to theoretical terms. He gives as examples of theoretical explications of “temperature”, the explication in the science of heat with the help of statements describing the Cournot cycle of heat transformation, and therefore in terms of such theoretical primitives as “perfect nonconductors”, “infinite heat reservoirs” and “infinitely slow volume expansions”.

Nagel emphasizes that while the term “temperature” is explicated in the science of heat in terms of both theoretical and observational primitives, it is not the case that the term understood in the sense of the first explication is cognitively synonymous with “temperature” construed in the sense of the second. This is one way in which the thesis of multiple meanings serves the logical positivist well: the positivist does not want the meanings of observation terms to be contaminated with the meanings of theoretical terms. It is therefore important to him that the set of meanings supplied by the various theoretical explications and the set supplied by the observational explications be separate and distinct. The thesis that multiple explications do not result in cognitive synonymy but rather in empirically unambiguous cognitive equivocation, enables him to say that even when a revolutionary new theory is developed, it will produce a new set of theoretical explications but will not revise the set of observational explications. In this way there is meaning variance in the theoretical meanings, and yet there is also meaning invariance in the observational meanings. It is interesting that Nagel's approach is different from Carnap's, because the latter distinguishes theoretical terms as having "incomplete" semantics, such that theoretical terms could change their meanings by becoming more complete even in a heterogeneous reduction. Carnap did not employ any thesis of empirically unambiguous equivocation like Nagel; Nagel is more faithful to Bridgman.

Nagel next considers the formal conditions for a heterogeneous reduction. In the reduction of thermodynamics to statistical mechanics, the Boyle-Charles law is made a logical consequence of the principles of mechanics, when these principles are supplemented by a hypothesis about the molecular constitution of a gas, a statistical assumption about the motions of molecules, and a postulate connecting the experimental notion of temperature with the mean kinetic energy of the molecules. Nagel sets forth two formal conditions for the reduction: the condition of connectability and the condition of derivability.

The first condition, connectability, requires that assumptions be introduced which postulate suitable relations between what is signified by a descriptive term (*e.g.*, "temperature") in the secondary science, and traits represented by theoretical terms already present in the primary science (*e.g.*, the kinetic energy of molecules). This is done by "coordinating definitions" or "correspondence rules", as Nagel also calls them, which have the same functions as what Carnap called "reduction sentences", and what Hempel calls "bridge principles". By whatever name, these are the sentences that connect theoretical terms occurring in a theory with the observation terms in the empirical statements the theory explains. Both the primary and secondary theories involved in a reduction are presumed to have whatever coordinating definitions they need before the reduction is effected.

The second condition, derivability, requires that together with the above mentioned assumptions all the laws of the secondary science including those containing the connected terms, must be logically derivable from the theoretical premises in the primary science and their associated "coordinating definitions". When both of these conditions are satisfied, the reduction can be effected, and the experimental and theoretical laws of the secondary science are made logical consequences of the theoretical assumptions including the coordinating definitions of the primary science.

After his discussion of the formal conditions, Nagel extends his semantical thesis of multiple meanings to reduction. After the reduction of thermodynamics to statistical mechanics is accomplished, the term "temperature" can be explicated in terms of the mean kinetic energy of molecules, and it thereby acquires still another meaning. This is the outcome of satisfying the condition of connectability. He explicitly denies that the connection made by the assumptions employed in the reduction are "logical" connections between established meanings of expressions, because the assumptions would then assert that there is either a synonymy or a one-way entailment in the relation to a theoretical expression in the primary science. Nagel maintains that the connecting assumptions are initially conventions that merely assign the additional meaning, and which later become empirical statements, because further development of the theory makes it possible to calculate the temperature of the gas in some indirect fashion from experimental data other than the temperature value obtained by actually measuring the temperature of the gas. He rejects as "unwitting double talk" the objection to his thesis that the reduction occurs due to a redefinition of the term "temperature". He maintains that the term "temperature" cannot be cognitively synonymous with the phrase "mean kinetic energy of molecules". He says that the terms in each of the two sciences have meanings unambiguously fixed by codified rules of usage or by established procedures appropriate to each discipline, and that these established meanings are not lost or changed as a result of the reduction.

Feyerabend is critical of the views of Hempel and Nagel, and he takes a fundamentally different view, fundamental because Feyerabend advances his "pragmatic theory of observation" in opposition to the positivist naturalistic view of observation. This point of departure places Feyerabend in the same company as Einstein, Heisenberg, Popper and Hanson, all of whom reject the positivist separation of theory and observation. On the positivist view observation statements are the products of natural processes that supply the observation language with its distinctive semantics. Feyerabend on the other hand affirms an artifactual theory of meaning, when in “Explanation, Reduction, and Empiricism” he bases his pragmatic theory of observation on the distinction between nature and convention. In his view this distinction implies, contrary to the positivist view, that the observational status of a statement must be separated from its meaning. Thus Feyerabend says that an observation sentence is distinguished from other sentences of a theory not by its meaning content but by the “cause of its production”, by which he means that its production conforms to certain behavioral patterns. This distinction begs elaboration that Feyerabend does not provide, but his pragmatic theory of observation gives Feyerabend an alternative to any reductionist thesis such as Nagel’s.

Feyerabend maintains that when a transition is made from one theory to another theory of wider scope, which Nagel calls the secondary and primary sciences respectively, what actually happens is semantically much more radical than the incorporation of an unchanged theory into the context of the primary theory, unchanged, that is, with respect to the meanings of the secondary theory’s main descriptive terms as well as to the meanings of the terms of its observation language. What happens is not a reduction, but the **complete replacement** of the ontology and perhaps the formalism of the secondary science by the ontology and the formalism of the primary science, and a corresponding change in the meanings of the descriptive elements of the formalism of the secondary theory, providing that these elements of the formalism of the secondary theory are still used.

Feyerabend states that contrary to the positivist reductionist thesis, the replacement affects not only the theoretical terms of the secondary science, but also at least some of the observational terms occurring in its test statements. He opposes the positivist thesis that a comprehensive theory merely orders facts, and maintains that a general theory has a deeper influence on thinking. This deeper influence is the semantical influence of the context of the primary theory on the empirical statements and vocabulary of the secondary theory. The consequence of the distinction between nature and convention, which separates observability and meaning, is what Feyerabend calls the “contextual theory of meaning”. Other philosophers refer to this idea as relativized semantics. His theory of meaning description implies a wholistic approach, because he says that the contextual determination of meaning is not confined to a single scientific theory or even to a single language. Thus the unit of language involved in the test of a specific theory is not just the theory taken together with its own consequences, but rather is a whole class of mutually incompatible and factually adequate theories. This class is the context by which meanings are to be made clear.

Feyerabend's rejection of the positivist naturalistic causal theory of meaning and his proposal of his conventionalist contextual theory of meaning, lead him to attack two basic assumptions that he finds in Nagel's theory of reduction and explanation. These assumptions are (1) deducibility and (2) meaning invariance. Meaning variance is one of the reasons that deducibility is impossible, but in addition to meaning variance, there are purely quantitative reasons why deducibility is impossible. In his treatment Nagel gave the reduction of Galileo's physics to Newton's physics as an example of a homogeneous reduction, one in which there is no meaning change resulting from the reduction. But Feyerabend says that there is a quantitative deviation between the Galilean and the Newtonian physics, an inconsistency due to the fact that one and the same set of observational data is compatible with very different and mutually inconsistent theories.

This inconsistency that makes deduction logically impossible has two sources. Firstly universal theories always make claims about phenomena that are beyond those that have actually been observed or that might be available at any particular time; it is this characteristic that makes them universal. Secondly the truth of any observation statement, such as a statement reporting a measurement reading, can be asserted only within a certain margin of error. The first reason allows for theories that differ in domains where experimental results are not yet available. The second reason allows for such differences even in those domains where observations have been made, provided that the differences are restricted to the margin of error in the observations.

The principal reason that deducibility is impossible in explanation and reduction of general theories is the inconsistency produced by the meaning variance, the semantical change resulting from the change of context. To illustrate this Feyerabend considers the purported reduction of the Aristotelian theory of motion to Newton's theory. He says that in this case Newton's theory offers the same quantitative measurements as Aristotle's, so there is no quantitative inconsistency. The reduction is achieved in the apparently simple manner of equating the concept of impetus in the Aristotelian theory with the concept of momentum in Newton's theory. On this approach the procedures and assumptions of Newton's theory supposedly fix the meanings of the descriptive terms in the impetus theory. But Feyerabend maintains that the concept of impetus as fixed by the usage established in the Aristotelian theory of motion cannot be defined contextually in a reasonable way in the Newtonian theory, because the Aristotelian usage involves laws that are inconsistent with Newtonian physics. Thus contrary to Nagel, the concept of impetus is not logically explicable in terms of the theoretical primitives of the primary science in a reduction, even if equating impetus with momentum is proposed as a physical hypothesis instead of an analytical one. Such a physical hypothesis merely says that wherever momentum is present, then impetus will also be present, and the measurements will be the same in both cases.

Feyerabend also finds meaning variance in the purported reduction of phenomenological thermodynamics to the kinematic theory of gases, the heterogeneous reduction case considered in detail by Nagel. He describes Nagel's view as a claim that the terms in the statements that have been derived from the kinetic theory with the help of correlating hypotheses will retain the same meanings that they originally had within the phenomenological theory. And he states that Nagel repeatedly emphasizes that these meanings are each fixed by its own procedures that is by the procedures of the phenomenological theory, whether or not the theory has been or will be reduced to some other discipline. Thus the term "temperature" as fixed by the established usages of phenomenological thermodynamics, as Nagel says, is such that its application to concrete situations entails the strict nonstatistical law. Feyerabend states that the kinematic theory does not offer such a concept. There does not exist any dynamical concept in the phenomenological law, while on the statistical account fluctuations between two levels of temperature is allowed. He therefore says that the thermodynamic concept and the kinetic statistical concept of temperature are "incommensurable", and that replacement rather than incorporation or derivation characterizes the transition from a less general theory to a more general one.

Feyerabend notes that both he and Nagel say that incorporation into the context of the statistical theory changes the meanings of the main descriptive terms of the phenomenological theory, but he adds that this is "double talk" by Nagel, because the law that has been reduced is no longer the same law. He says Nagel's view of change of meanings is somehow supposed to leave untouched the meanings of the main descriptive terms of the discipline to be reduced.

There is a sense in which Nagel's view indeed involves double talk. This double talk is not an inconsistency in Nagel's thesis, but rather is a logical consequence of his semantical thesis, the view that the terms in science are equivocal and have multiple meanings. But Feyerabend prefers to reject any such equivocation that would permit semantical continuity through the reduction. Instead he prefers to retain the univocity in the terms at any point in time, and to affirm a change from one meaning of a univocal term to another new one, even at the expense of a semantical continuity in the empirical explications. Consideration of the nature of this semantical discontinuity introduces the roles of inconsistency and especially "incommensurability".

In his "Explanation, Reduction, and Empiricism" Feyerabend describes two ways in which theories can be related to each other such that meaning variance may occur. Those two ways are inconsistency and incommensurability. Given two historically successive theories denoted **T** and **T'** respectively, the theory **T** will differ from the theory **T'**, either (1) if **T** is inconsistent with **T'** in the domain of deduced empirical laws where **T** and **T'** overlap, or (2) if the set of empirical laws that follow from theory **T'** are incommensurable with those following from **T**. When the relation is that of inconsistency, the two theories are commensurable, which is to say semantically comparable. Feyerabend references Popper saying that the new and superior theory **T'** implies laws that are different from and superior to those implied by theory **T**. In this case the laws deduced from theory **T'** correct and replace those deduced from **T**, just as occurred in the case of Newton's theory correcting and replacing Kepler's and Galileo's laws.

When theories **T** and **T'** are incommensurable, however, they do not have any comparable observational consequences. It is not even possible to say that the empirical laws that are deduced from one are superior or inferior to those that are deduced from the other. This semantic incommensurability is admitted by Feyerabend's wholistic pragmatic theory of observation. On this theory of meaning nature does not determine the content of thought and therefore does not guarantee consistency or even comparability of meaning. Instead the content of thought is a human artifact not unlike any work of art, and there may result differences between people's thinking that are so fundamentally different, that they may admit no basis for comparison or common denominator; they may be incommensurable.

In his "On the 'Meaning' of Scientific Terms" reprinted in *Realism, Rationalism, and Scientific Method*, Feyerabend describes a theory and its predecessor to be incommensurable, if prior to the time the theory is proposed, there exists no more general concept having an extension that includes the extensions of the concepts of the two theories. He considers Einstein's relativity theory to be incommensurable with Newtonian celestial mechanics, because prior to Einstein the Reimann metric did not include time, and he says that this change in the transition to Einstein's theory was drastic enough to exclude common elements between the two theories. He also considers quantum theory to be incommensurable with classical physics, because prior to its advent the conservation laws were not applied to virtual states.

Later Feyerabend further elaborated on his concept of semantic incommensurability by drawing upon the Sapir-Whorf hypothesis and specifically upon Whorf's thesis of linguistic relativity. Both Kuhn and Feyerabend briefly reference Whorf in their works published in the 1960's, and Feyerabend's elaboration of his thesis of semantic incommensurability is to be found in his *Against Method* published in 1975. But before turning to this work, a summary of the Sapir-Whorf hypothesis is in order.

The Sapir-Whorf Hypothesis

Benjamin Lee Whorf (1897-1941) was a cultural anthropologist and linguist by avocation, who received a BA degree in chemical engineering in 1918, and spent his career with an insurance company eventually becoming Assistant Secretary, an officer of the corporation. He became interested in linguistics in 1924 and was almost completely self-educated in linguistics except for some nondegree courses that he took from Edward Sapir, a cultural anthropologist and linguist at Yale University. Sapir encouraged Whorf to study the language of the Hopi American Indians, and he financed Whorf's field studies. These studies occasioned Whorf's formulation of the Sapir-Whorf hypothesis, the thesis of linguistic relativity for which Whorf is now best known. This thesis is still controversial, and is in conflict with such absolutist views as Chomsky's thesis of innate linguistic universals. Whorf wrote many articles, but few of those that he submitted to academic journals were accepted and published in his lifetime in spite of the intrinsic merit of the papers. A posthumous anthology of his writings, *Language, Thought and Reality,* was published in 1956 (ed. Carroll, MIT Press).

It may be said that there is an earlier and a later, expression of Whorf's thesis. The earlier statement made in the 1930's is his thesis of "cryptotypes" or "covert categories", while the more mature statement is the explicit statement of linguistic relativity made in "Science and Linguistics" in 1940. Whorf exemplifies the idea of the cryptotype with grammatical categories for gender. Gender may be manifested either by overt or by covert indicators. They are overtly manifested by morphemes, which are formal markers that occur in such languages as Latin or German. They are covertly manifested in English by what Whorf calls their "reactance", their association with definite linguistic configurations such as lexical selection, word order that is also class order, or in general by some kind of patterning. More precisely: overt categories are those having a formal mark that is present in every sentence containing a member of the category, while covert categories are all others, even those that are marked nonphonetically but occur only in certain types of sentences. And he defines his idea of reactance as a special type of "rapport", an idea that is roughly equivalent to the general idea of structure in language.

Rapport is the linkage between the elements of language that enables these elements to have semantical effect. It is governed by what Whorf calls "an invisible central exchange". This invisible central exchange of linkage bonds is what gives rise to the covert categories, since they are submerged, subtle and elusive meanings corresponding to no actual word, but having a functionally important rôle in the grammar of a language. Words of a covert category are not distinguished by a formal mark but rather by a semantical class, by an idea that gives the grammatical class its unity, which is manifested by "common reactance". Semantically the covert category is what Whorf calls a deep persuasion of a principle behind some phenomenon, like the ideas of inanimation, substance, force, or causation.

The later and more relevant expression is the thesis of linguistic relativity, the thesis that language structure controls thought. Whorf locates his development of linguistic relativity in the history of cultural anthropology in the lineage of Franz Boas and Edward Sapir. Boas had shown that a language could be analyzed *sui generis*, that is without forcing upon the language the categories of the "classical" tradition. Then in 1921 in his book *Language* Sapir inaugurated the linguistic approach to thinking, demonstrating the importance of linguistics to cultural anthropology. According to Whorf comparative linguistics now reveals that the background linguistic system, the grammar of each language, is not merely a sentence-producing instrument for voicing ideas but rather is **the shaper of ideas**. And this is the essence of his thesis of linguistic relativity. The human mind cuts up nature, organizes it into concepts, and ascribes significance, because men are parties to an agreement that holds throughout the speech community, and that is codified in their language. **Not all observers are led by the same physical evidence to the same picture of the universe, unless their linguistic backgrounds are similar or in some way can be "calibrated".** For Whorf's term "calibrated" one is tempted to substitute Feyerabend's term "commensurated", except that Feyerabend does not believe that semantically incommensurable theories can ever be commensurated.

Whorf further elaborates on his linguistic relativity thesis in his "Language, Mind and Reality" (1942). In the context of a discussion of the Mantric Art of India he distinguishes two great levels: the realm or level of meaning or lexication, and the higher and controlling level of patterning of sentence structure that guides words which occur at the lexical level and that is more important than words. Lexication, the partitioning of the whole manifold of experience and the assigning of the parts to words, makes the parts stand out in artificial and semifictitious isolation. This process of lexication is controlled by the patterning function of sentence structure and thus by the organizing at a higher level, where the combinatory scheme occurs. These patterns are not individual sentences, but rather are schemes of sentences and designs of sentence structure. The patterns are manifested by using the mathematical or grammatical formulas into which words, values or quantities may be substituted. Each language does this partitioning and patterning in its own way, and each has its own characteristic form principles, that make consciousness a mere puppet, whose linguistic maneuverings are held in unsensed and unbreakable bonds of pattern.

These passages suggest similarities between Whorf's view and Feyerabend's contextual theory of meaning, save for the fact that Feyerabend does not restrict the term "meaning" to a lexical function. As it happens, Whorf explicitly states in several of his later articles that his thesis of linguistic relativity applies to empirical science. He views it as applicable not only because science including mathematics consists of language, but also because an awareness of the effect of language on the foundations of thought will facilitate what he describes as "science's next great march into the unknown". He expresses regret that philosophers and mathematicians do not even have apprenticeship training in linguistics, and he states the opinion that further development in logic will proceed with the investigation of the structures of diverse languages.

Like later philosophers, Whorf views the various specialized sciences as different languages, because he finds that there exist communication problems among the researchers in the different specialties, just as there are such problems among the speakers of different natural languages. He maintains that these communication problems do not simply breed confusion about details that the expert translator could resolve. The problems are much more perplexing, since the language of science is a "sublanguage", which incorporates certain points of view and certain patterned resistances to widely divergent points of view.

These resistances not only isolate artificially the particular sciences from one another, but they also operate to restrain the scientific spirit from taking the next great step in its development, a step which entails viewpoints unprecedented in science and involving a complete severance from tradition. This great episode will unify the diverse sciences, and will be based on the discovery of the aspect of language consisting of patterned relations. The approach to reality through mathematics as used in science today is merely one special case of this.

Whorf proposed that there is a premonition in language of an unknown and vaster world, which is quite different from the world as it is currently understood through the structure of the Indo-European languages, which insist on substantives. The apparent necessity of substances is purely a result of the "Ayrian grammar". The logic of Aristotle is provincial, because it is based on the ideology of substantives, while modern physics with its emphasis on fields casts doubt on this ideology. Whorf prognosticates the emergence of a new type of language for science that is even more universal than that presently used, because it will be a transcendental logic of relations of pure patternment.

Whorf's premonition of an unknown and vaster world was more prescient than he probably knew. Today he might have referenced the phenomenon of nonlocality, had he known of J.F. Clauser, M.A. Horne, A. Shimony, and R.A. Holt's experiment, implementing John Bell's inequality for the famous EPR thought experiment, to say nothing of string theory. If there is a language of pure patternment, it is the mathematical statement of the modern quantum theory, which does not translate unambiguously into the substantive language of ordinary discourse. Even the practice of scientific realism does not conclusively resolve the issue of whether the electron's wave and particle aspects are instantiated as two aspects of one and the same entity, as Heisenberg maintains, or whether they are instantiated as two separate entities, as Bohm maintains, because mathematics does not contain substantive syntactical categories. The "individual" in mathematics is the measurement instance and not the substantive entity, and Heisenberg had to conjure a peculiar substantive "entity" he called a "*potentia*".

If Bohm is correct that the duality issue occurs in what he calls the "informal" language and not in the mathematical formalism, then Hanson's observation that the mathematical expressions of the wave mechanics and the matrix mechanics can be transformed into one another does not necessarily support his thesis that independence through such transformation implies any semantics or ontology for the Copenhagen duality interpretation, unless perhaps one redefines "entity" in terms of Max Born's ontological criterion of invariance, *i.e.*, quantities having the same value for any system of reference independently of transformations. This might be construed as ontological relativity, if one excludes Quine's requirement that any theory subordinate to our initial "home language" must be interpreted by reference to this home language, which Whorf views as our Indo-European language of substantives.

Feyerabend on Semantic Incommensurability

Feyerabend's later and more comprehensive statement of his incommensurability thesis is set forth in chapter seventeen and in a brief appendix in his *Against Method.* The centrality of the incommensurability thesis to his philosophy is indicated by the fact that this chapter and its immediately following appendix pertaining to his incommensurability thesis, take up approximately seventy pages of this three hundred page book. Later in his *Science and a Free Society* (1978) he emphasizes that his intent in the discussion of incommensurability is to understand the changes that take place when a new world view enters the scene, and that this requires examining it from the perspective of the concerned parties, and not as it appears or is projected onto a later ideology years afterwards.

The significance of incommensurability is that the concerned parties experiencing it cannot subject the new idea to what they regard as rationality, and they must allow the idea of "reason" that is accessible to them to be violated. He views this analysis "from the inside" to be of the utmost practical importance, because it is what occurs in a scientific revolution, and every researcher should be prepared for such events, which would otherwise catch the researcher by surprise.

In the opening sentence of chapter seventeen of *Against Method* Feyerabend says that he has much sympathy with the clearly and elegantly formulated view of Whorf, and he gives a brief summary of Whorf's principle of linguistic relativity. In the appendix following the chapter he notes that Whorf's principle admits to two alternative interpretations. On one interpretation it means that observers using widely different languages will posit different facts in the same physical circumstances in the same physical world. On the other interpretation it means merely that observers using widely different languages will arrange similar facts in different ways.

The former interpretation is the one that Feyerabend says he uses for his own incommensurability thesis, and he justifies this interpretation on the basis of the great influence that Whorf ascribes to grammatical categories and especially to the hidden "rapport system" of language. The covert classifications that result from this hidden rapport system or "central exchange" create patterned resistances to widely divergent points of view. Feyerabend says that if these resistances oppose not just the truth of the resisted alternative views, but the presumption that an alternative has been presented, then we have in instance of incommensurability. This is the closest that Feyerabend comes to a definition of incommensurability, because as he says, it is hardly ever possible to give explicit definition of it, since it depends on covert classifications and major conceptual changes.

The body of Feyerabend's chapter discussing incommensurability is organized into three theses, which are summarized at the end. His ***first*** thesis is that there are in fact frameworks of thought which are incommensurable, and he emphasizes that this is an anthropological thesis. Whorf was an amateur although accomplished cultural anthropologist. Feyerabend maintains that Whorf's principle of linguistic relativity applies to scientific theories such as Aristotle's theory of motion, the theory of relativity, the quantum theory and classical and modern cosmology, because they are sufficiently "deep" and have developed in sufficiently complex ways that they may be viewed as widely divergent and incommensurable natural languages. He therefore also maintains that philosophy of science is anthropology of science and not logic of science as both the positivists and Popper had maintained.

In the examination of the incommensurable theories, where facts asserted by each cannot be compared side by side even in memory, it is necessary to take the approach of the field linguist and learn the new theory from scratch. The irrationality of the transition to the new theory is overcome by the determined production of nonsense until the material produced is rich enough to permit recognition of new universal principles. The initial madness turns to sanity provided that it is sufficiently rich and sufficiently regular to function as the basis of a new world view. There is no translation involved; instead there is a learning process. This is how Feyerabend sees the transition from classical mechanics to quantum mechanics and from Newtonian mechanics to relativity theory.

His ***second*** thesis is that incommensurability has an analogue in the psychology of perception, and that the development of perception and thought in the individual passes through stages that are mutually incommensurable. This is contrary to the positivist philosophy of observation, and Feyerabend references Piaget's work with perceptual development in children.

His ***third*** thesis is that scientific theories may be incommensurable even when they apparently treat of the same subject matter and the same problem. On a realistic interpretation, as opposed to an instrumentalist interpretation, incommensurable theories do not treat the same subject matter. A new theory such as relativity theory in physics does not treat the same problem that is treated by its predecessor, Newtonian mechanics, when the former replaced the latter. The new theory does not "solve" problems confronting the old theory, but rather it "dissolves" them and removes them from the domain of legitimate inquiry, because the new incommensurable theory has an ontology that replaces that of the older theory. When the faulty ontology of the older theory is comprehensive, as in the Newtonian physics, then every description inside the domain must be changed; it must be replaced by a different statement in the new theory or it may be replaced by no statement at all. The new ontologies of relativity theory and quantum theory do not just deny the existence of classical states of affairs; they do not even permit us to formulate statements expressing such states of affairs.

Crucial experiments are therefore impossible, because one theory cannot establish or refute another theory incommensurable with the former. Each incommensurable theory has its own facts, and it can be refuted only by reference to its own kind of experience, that is to say, by discovering its internal contradictions. Their contents cannot be compared. Aside from internal inconsistency, the only basis for preference for one of several mutually incommensurable theories is subjective, such as the scientist's metaphysical prejudices, religious convictions, or personal tastes.

Feyerabend on Scientific Anarchy

In *Science and a Free Society* Feyerabend says in a section containing some autobiographical notes, that Carl Friedrich von Weizsacker has "prime responsibility" for Feyerabend's change to his anarchistic view. In the days that Feyerabend was supporting Bohm's views, he met with von Weizsacker in Hamburg in 1965 and discussed the foundations of quantum theory. Feyerabend complained that alternatives to quantum theory have been ignored, but Weizsacker showed how quantum mechanics arose from concrete research. Feyerabend relates that it then became clear to him that general methodological rules imposed without regard to circumstances are a hindrance rather than a help, and that a person must be given complete freedom with no restrictions by any norms or demands regardless of how plausible they may seem to logicians and philosophers. Feyerabend concluded that such norms and demands must be checked by research, and not by appeal to ideas of rationality. Thus did Feyerabend come to advocate scientific anarchy.

In *Against Method* (1975), Feyerabend's first book, he expounds his philosophy in terms of this political phrase, "scientific anarchy", which he fully intends to be intellectually more radical than Kuhn's phrase, "scientific revolution". Feyerabend's phrase includes his principles of tenacity and theory-proliferation to which he adds an antimethodological practice which he calls "counterinduction", a concept of scientific development that is opposed both to the logical positivist critical method of confirmation and also to Popper's critical method of corroboration. Counterinduction is opposed to all concepts of scientific rationality and methodology in which criticism is intended to eliminate some scientific theories as incorrect.

Feyerabend advocates scientific anarchy, because he denies that there is any method or concept of rationality that is adequate to the history of successful science in any sense of the term. He is against all methodologies, because there is no methodological rule that has not been violated, and these violations are necessary for the advancement of science. The only rule that he admits is "anything goes". There is no institutional aim of science in his view, but instead each scientist may formulate his own individual aim of science, and "progress" may mean anything that one may wish.

In Feyerabend's view scientific knowledge is an ever-increasing "ocean" of mutually incompatible and even incommensurable theories with each theory forcing the others into greater articulation. In this view counterinduction aims to introduce and to elaborate hypotheses, which are inconsistent with well established theories and with well established facts. This perpetual pluralism is possible, because even the worthiest theory has many anomalies where it does not fit the facts, while at the same time all factual statements contain theoretical assumptions. Not only is every factual description dependent on some theory, but there are also facts that cannot be unearthed except with the help of alternatives to the theory to be tested. These facts are unavailable so long as such alternative theories are excluded. In Feyerabend's view the practice of scientific research must not contain any rules requiring either consistency with so-called confirmed theories or with the choice between falsified and nonfalsified theories. The ocean of anomalies that always surrounds every theory is concealed by *ad hoc* hypotheses and by *ad hoc* approximations that are not the result of limited measurement accuracy, but which are adjustments to cope with for complicated cases.

Feyerabend illustrates counterinduction in the history of science with an examination of Galileo's defense of the Copernican theory against Aristotelian critics. In *Science and a Free Society* Feyerabend says that his views on Galileo expressed in *Against Method* are influenced ironically by Philipp Frank, a logical positivist and member of the Vienna Circle. The relevant Aristotelian criticism is the "tower argument", according to which a stone dropped from a high tower would not fall vertically to the ground if the earth were in motion as Copernicus' theory says it is, because the movement of the earth during the time of free fall would make the object fall at an angle away from the direction of the earth's rotational movement. Feyerabend calls the observation of vertical fall of the stone a "natural interpretation" of the observation statement describing the motion of a falling stone, because the observational sensations are firmly associated with the linguistic expression of the observation statement. And he says that it is very difficult to detect error in natural interpretations without alternative statements. In his examination of Galileo's reply to the tower argument Feyerabend maintains that Galileo used the Copernican theory to supply an alternative observational interpretation, and that Galileo's reply was a reinterpretation of the Aristotelian natural interpretation. In this manner Galileo appealed to the "real" motion of the falling stone, by which he meant the stone's movement relative to absolute space. Galileo distinguished between Copernican and Aristotelian motion, and characterized them as "real" and "apparent" motions respectively, arguing they are not the same.

Galileo's reply to the tower argument is an example of counterinduction. When a theory such as the Copernican theory is contradicted by facts, the counterinductive response is to turn around the situation and to use the theory as what Feyerabend calls a "detection device". This procedure consists firstly of affirming the truth of the new theory, and then of inquiring what changes in the facts will remove the contradiction between fact and theory. In this way hidden ideological components in the observation language expressing the facts are disclosed counterinductively. Once these ideological components are disclosed, the next step is to create a new observation language for the new theory. This is what Galileo did, and he used some propaganda to disguise that fact that he had invented the new observation language himself. His propaganda consisted in arguing that the human senses notice only relative motion, while the senses fail to notice motion that is common to such objects as falling stones and the earth, and he also used the *ad hoc* hypothesis based on the Copernican theory that the earth is in permanent motion. Galileo believed in the truth of the Copernican theory, and he looked for facts that supported that theory. One such supporting fact is the one resulting from his reinterpretation of observed experience, such as the falling stone. Galileo changed the conceptual component in observed fact.

Another revision of fact results from Galileo's invention and use of the telescope. Feyerabend says that Galileo did not know enough optical theory to enable the telescopic phenomena to function as independent evidence for the Copernican theory. Use of the telescope for celestial observation was also problematic to the Aristotelians, and what Galileo did was to use the agreement between the Copernican theory and the telescopic observation to argue on behalf of both of these views. The use of telescopic phenomena as evidence for the Copernican theory had to await the further development of the auxiliary science of optics.

Neither the telescopic phenomena nor the new idea of relative motion were acceptable to common sense at the time or to the Aristotelians, and the two associated ideas both seemed false. Yet these seemingly false and unacceptable phenomena were distorted by Galileo, and converted into strong support for Copernicus. Galileo replaced old facts with a new type of experience, which he simply invented for the purpose of supporting Copernicus, and he let apparently refuted theories support one another, in order to create a new world view. Feyerabend maintains that Galileo's arguments violate basic rules of scientific method, which were invented by Aristotle and canonized by the positivists, such as Carnap and Popper. (Feyerabend occasionally calls Popper a positivist.) And he states that Galileo succeeded precisely because he did not follow these rules. Had Galileo followed these methodological rules, he would have failed.

Feyerabend's general thesis is that every methodological rule is associated with cosmological assumptions, so that using that rule implies that the cosmology in which it originates is correct. The rule that the Copernican theory must be tested is reasonable, but requiring that it be tested by confronting it with the *status quo* is not reasonable. What is reasonable is the purportedly "irrational" practice of waiting and ignoring large masses of critical observations and measurements, because the Copernican theory is an entirely new worldview. It is necessary to retain the new cosmology, until it has been supplemented with the necessary auxiliary sciences, so that the language in which observations are expressed may be revised.

Feyerabend finds what he illustrates with Galileo to be no less applicable today. He says that today's rational sciences survived, because irrational "prejudices" were permitted to have their way, and that it is advisable to let one's inclinations go against reason in many circumstances. Propaganda is of the essence. Science is more sloppy and irrational than its methodological image. Anarchistic deviations from rationality are necessary for progress. The image of twentieth-century science is created by technological successes together with a "fairy tale" of how these technological miracles were accomplished. The fairy tale is that science is not an ideology, but rather is an objective measure for all ideologies. Feyerabend maintains that science is an ideology, and that successful science is very much a result of good luck and false beliefs. His thesis of scientific anarchy moves him far along in the direction of historical relativism. But the centrality of historical relativism in his philosophy of science is not fully evident without examination of the lengthy evolution of his philosophy of quantum theory and of realism.

Feyerabend on Quantum Theory

From the time of his writing his dissertation in 1951, Feyerabend's philosophy of science was centered on the reconciliation of metaphysical realism with modern microphysics. The development of his thought on this matter might be viewed as a case of the moth and the flame, where the circling moth is Feyerabend's realistic philosophy and the consuming flame is Bohr's peculiar Copenhagen interpretation of the quantum theory. Initially he was critical of the Copenhagen interpretation, and particularly both of Bohr's instrumentalist view of the quantum theory's formalism and of Bohr's complementarity thesis. Feyerabend received his views on metaphysical realism from Popper, but Feyerabend did not agree with Popper's attempt to supply the current quantum-theoretic formalism with the propensity interpretation, although it is an interpretation in classical physics. Instead Feyerabend defended the possibility of an altogether new microphysical theory.

In the 1960's Feyerabend became involved in a long debate with Norwood Russell Hanson. As a result he reconsidered the merits of the current quantum theory, and the accepted likelihood of its duality thesis and its quantum postulate being carried forward into a future microphysics. Then instead of continuing to advocate the revision of the current quantum theory into a microphysics that would be compatible with Popper's universalist-realism, Feyerabend revised his concept of realism in a manner that no longer requires the universalism that Popper demands. Generalizing on Bohr's thesis of the relational character of quantum states when describing experimental findings with classical concepts, Feyerabend formulated his nonuniversalist, regional and historical relativist realism.

Feyerabend sets forth his statement of Popper's universalist realist philosophy in his "Attempt at a Realistic Interpretation of Experience" in *Proceedings of the Aristotelian Society* (1958). This paper is an abbreviated statement of his doctoral dissertation written in 1951 at the University of Vienna. The thesis of this paper, which he calls "Thesis I", is that the semantical interpretation of an observation language is determined by the theories that we use to explain what we observe, and that the interpretation changes as soon as those theories change. But he also states in this paper that one of the consequences of Thesis I is that we must distinguish between appearances or phenomena on the one hand and the real things appearing on the other hand. In Feyerabend's view this distinction is fundamental to realism. On Thesis I the real things appearing are those that are referenced by the observational sentences in a certain interpretation given by a realistic explanatory theory. In both this paper and in his "Complementarity" in *Proceedings of the Aristotelian Society* he criticizes the complementarity thesis of Bohr's interpretation of the modern quantum theory.

Unfortunately in all discussions of the quantum theory Feyerabend always takes Bohr's statements and views to be authoritative and representative of the Copenhagen interpretation. In these earlier papers he acknowledges the influence of Bohm and of Popper upon his thinking. He notes that Bohr's idea of complementarity is based partly upon empirical investigations in physics and partly upon philosophical analyses, and he accordingly distinguishes between the experimental "fact of duality" and the philosophical thesis of complementarity. The fact of duality is the result of experimental findings. Experiments displaying interference effects can be explained by wave concepts, but they contradict explanations in terms of particle concepts. Conversely experiments displaying absorption and emission can be explained by particle concepts, but they contradict explanation in terms of wave concepts.

Feyerabend therefore maintains that there is no system of physical concepts, that can explain all these experimental facts about light and matter, which is to say, there is no universal theory of light and matter. He states that for a physicist who views wave and particle as aspects of the same objective entity, the fact of duality proves that the theories available at the moment are inadequate. Such a physicist will search for a new theory and conceptual scheme, which satisfies two requirements: Firstly the new theory must be empirically adequate, and secondly it must be universal. Such a theory conforms to what Feyerabend calls the "classical ideal", which is to say that it conforms to Thesis I, because it does not just describe appearances under certain experimental conditions, but rather it describes what light is and what matter is, the things appearing, in reality.

Feyerabend got the universalist concept of realism from Popper. In "Complementarity" (1958) he references Popper's "The Aim of Science" published in *Ratio* (1957), and says it is an excellent characterization of the classical ideal of scientific explanation and its connection with realism. In this article Popper affirms that explanations in science are given in terms of universal laws of nature, which are conceived as conjectural descriptions of the structural properties of nature, that is of the world itself. He explains that by “universal” he means that scientific laws and theories must make assertions about all spatiotemporal regions of the world. Popper also speaks of different levels of universality, which he exemplifies by the greater universality of Newton’s laws relative to Kepler and Galileo’s laws. But Popper also rejects a reductionist relation between Newton and Galileo’s physics. He states that whenever a new empirical theory of higher level of universality successfully explains an older theory, it does so by correcting the older theory. He adds that the idea of independent evidence can hardly be understood without the idea of discovery, of progressing to deeper layers of explanation. Independent evidence cannot be understood without the idea that there is something to be discovered and to be discussed critically, where “deeper layers” means explanation by means of more universal laws and theories, as exemplified by Newton’s laws, which are deeper relative to Galileo or Kepler’s laws. This is the universalist-realism that Feyerabend maintained until he embraced relativism.

Feyerabend characterizes Bohr’s philosophical thesis of complementarity as the opposite of the classical ideal of scientific explanation, and he says that the difference between the classical ideal and complementarity is an instance of the age-old issue between realism and positivism. Bohr’s complementarity thesis is an instance of positivism, because Bohr maintains that the account of all evidence must be expressed in classical terms, and that it is not possible to dispense with what Bohr called the “forms of perception”. Some such as Heisenberg consider Bohr’s “forms of perception” to be neo-Kantian, and Feyerabend notes that positivists do not customarily consider phenomena to have any forms. Feyerabend therefore describes Bohr as positivist of a “higher order”. He also states that Bohr’s instrumentalist view of current quantum theory, which Bohr calls a “natural generalization of classical physics”, is merely the result of retaining classical concepts. Both the retention of classical concepts and the instrumentalist view of quantum theory are contrary to Thesis I. He therefore says that complementarity is a statement of the fact of duality and is the way in which the classical concepts appear within the predictive schemes, which replace classical laws on the atomic level. He references passages contrary to Thesis I, in which Bohr states that the difficulties of atomic theory cannot be evaded by replacing the concepts of classical physics by new nonclassical conceptual forms. At the same time while Feyerabend views complementarity to be the result of retaining classical concepts, he does not simply deny the fact of duality, or that duality will be eliminated merely by philosophical reflection with Thesis I.

With his distinction between the fact of duality on the one hand and the statement of complementarity expressing the fact of duality with classical concepts on the other hand, Feyerabend considers two approaches to a realistic microphysics. The ***first*** approach is to reinterpret the formalism of the modern quantum theory, which is a mathematical statement of the fact of duality. He admits that if the quantum theory is viewed as a predictive theory like celestial mechanics, then a realistic interpretation does not seem to be possible. But he adds that if the quantum theory is viewed as a theory containing new concepts for the description of nature, then a **realistic interpretation "of a rather unusual kind" is definitely possible.** This amounts to a proposal to construe the contemporary quantum theory with its duality thesis in accordance with Thesis I. Such a reinterpretation will not retain classical concepts, and will express the fact of duality without expressing complementarity. He also says that the quantum theory thus used to form new concepts about the nature of physical systems, may permit some features of the macrophysical level to be derived from quantum mechanics, and thus make duality compatible with the universality condition for realism.

As it happens in his *Understanding Quantum Mechanics* (1999) physicist Roland Omnès reports that recent conceptual developments using the Hilbertian framework have enabled all the features of classical physics to be derived directly from Copenhagen quantum physics. And apparently unbeknown to Feyerabend, Heisenberg had opted for this first approach, when he accepted Einstein's thesis that the theory decides what the physicist can observe. But this first approach does not seem to have been Feyerabend's preferred way to interpret microphysics realistically, and he says explicitly that the possibility of a realistic microphysics does not depend on supplying a realistic interpretation for the current quantum theory with its duality thesis.

His ***second*** and preferred approach is to develop an entirely new microphysical theory. This new theory would satisfy two conditions: Firstly it would be universal, and secondly it would be empirically adequate. As a universal theory it will have a unified conceptual apparatus, which when applied to the domain of validity of classical physics, will be just as comprehensive as the classical apparatus. In other words the new microphysical theory will be of a higher level of universality, such that it will also be a macrophysical theory, yet different from classical physics. Feyerabend explicitly compares the relation between the new universal microphysical physics and the classical physics, to the relation between the relativity theory of gravitation and the Newtonian theory of gravitation. The empirical adequacy criterion will be satisfied, when this realistic, universal macrophysical theory contains the current elementary quantum theory as an approximation. It may therefore contradict quantum mechanics without violating the universality criterion for realism. Feyerabend affirms that for a realist, the solution of the problem of duality need not be found in alternative interpretations of the current quantum theory, which he says is in all probability nothing but a predictive scheme anyway. Instead it can be found in the attempt to derive a completely new universal theory, which need not contain the duality thesis or complementarity. This new microphysical theory will supply new concepts for reinterpreting duality.

For ten years following these 1958 papers Feyerabend wrote a series of articles defending and advocating attempts to develop a new microphysics without duality. In these papers he contrasts his view that there can be a realistic microphysics without duality, with Bohr's view that all future microphysics must contain the duality thesis. In "Niels Bohr's Interpretation of the Quantum Theory" in *Current Issues in the Philosophy of Science* (1959) he discusses what he calls the "dogmatic elements" in Bohr's approach. He objects that Bohr treats duality as an unalterable experimental fact that must be included in any future microphysical theory; on his Thesis I description of experiments is not unalterable. Feyerabend argues that the only condition that need be satisfied by a future microphysical theory, is that it be compatible with experimental findings to a certain degree of approximation and within a certain degree of accuracy that is required for the dogmatic elements of Bohr's approach.

In this and other papers written during this period Feyerabend sets forth his interpretation of Bohr's philosophy, according to which all state descriptions of quantum mechanical systems are relations between the system and measuring devices in action, that is to say, between microscopic system and macroscopic apparatus. This relational character of quantum state descriptions results from the need to restrict the application of any set of concepts to a certain experimental domain due to the wave-particle duality. Bohr's relational view is contrasted with both the classical view and with Heisenberg's view of measurement in quantum theory. Feyerabend says that both classical physics and Heisenberg's view are variations on an "interactionist" view. In classical physics the interaction between the apparatus and the system can be explained in terms of the theory used to describe the system. And on Heisenberg's view the measurement of a quantum mechanical system involves an interaction that disturbs the system in unpredictable ways.

Feyerabend says that Bohr's relational view enabled Bohr to reply to the argument by Einstein, Podolsky and Rosen (EPR), who defended the thesis that quantum mechanical systems have definite classical states instead of indefinite states described by the indeterminacy relations. This argument postulates two systems which are separated to such an extent that no interaction can occur between them, and therefore measurement disturbance in one cannot affect the other. Bohr made his thesis of indefiniteness of state descriptions compatible with the EPR argument by assuming that states are relations between systems and devices rather than properties of the systems.

The point is that while a property of the system cannot be changed except by interaction with the measurement device, a relation can be changed without such interaction. Bohr therefore views position and momentum as relations rather than as properties of the quantum-mechanical system. Bohr attempts to express this by his distinctive use of the term "phenomenon", which he uses to refer to the observations obtained under specific circumstances including an account of the experimental arrangement. Therefore phenomena cannot be subdivided, and dynamical variables cannot be separated from the conditions of their application. Physical attributes no longer apply to the object *per se*, but apply to the whole experimental arrangement with different assertions (wave or particle descriptions) appropriate in different circumstances. Bohr relativized the dynamical variables in the quantum theory to the circumstances of the experimental situation, and years later following Bohr, Feyerabend would relativize all reality to the circumstances of the knower's situation.

But in 1962 in "Problems of Microphysics" in *Frontiers of Science and Philosophy* Feyerabend was still defending the possibility of a universal and therefore realistic microphysical theory without duality. He says that between 1935 and 1950 the Copenhagen interpretation had become a dogmatic "creed", and that the objections of a few opponents such as

Einstein and Schrödinger were taken less and less seriously. But he notes that more recently there has occurred the development of a counter movement, which demands that the assumptions of the Copenhagen interpretation be given up and be replaced by a different philosophy. These "revolutionaries", as Feyerabend calls them, have shown not only that the empirical adequacy of the complementarity thesis is in doubt, but also that even empirical success is not sufficient reason to say that there can be no valid alternative to complementarity. He insists that future researchers need not and indeed should not be intimidated by the restrictions that some "high priests" of complementarity would impose.

One such revolutionary that Feyerabend has in mind is the physicist, David Bohm. Initially Bohm had accepted the Copenhagen interpretation, but later he advanced an alternative thesis in his "Quantum Theory in Terms of Hidden Variables" in *Physical Review* (1951), and in more detail in his books, *Causality and Chance in Modern Physics* (1957) and *The Undivided Universe* (1993). His "hidden-variable" thesis postulates the existence of a subquantum domain at a much lower and presently experimentally inaccessible (therefore hidden) order of magnitude than the quantum domain that is described by quantum theory.

In "Professor Bohm's Philosophy of Nature", a review of Bohm's 1957 book in *British Journal for Philosophy of Science* (1961), Feyerabend says that complementarity can be interpreted in either of two ways. The way he finds acceptable is that in which it functions to provide an intuitive picture for wave mechanics, and as a "heuristic principle" guiding future research. He says that this first way is undogmatic, since it admits the possibility of alternatives including preferable alternatives, even though no satisfactory alternative exists presently. The second and unacceptable view is that of Bohr, who maintained complementarity as a basic philosophical principle incapable of refutation, and to which future microphysical theory must conform. In his review of Bohm, Feyerabend says that Bohm argues against Bohr's dogmatic view by affirming a rôle for speculation in modern empirical physics. In a discussion of the rôle of speculation in "Problems of Microphysics" Feyerabend rejects demands by Hanson that Bohm's theory must be set forth as an algebraically detailed and experimentally acceptable theory. He admits that such criticism is appealing to the great majority of physicists. But he maintains that such criticism puts the cart before the horse. The discussion among physicists of alternatives to the current theory plays a most important rôle in the development of physics, and a complicated physical theory cannot be invented in its full formal splendor without some preparation. Feyerabend later elaborated upon this thesis in his discussion of theoretical pluralism and counterinduction. At this stage of his thinking he advocates these ideas in order to encourage the development of a new microphysical theory not containing duality.

Norwood Russell Hanson, an academic philosopher of science, was an influential critic of Feyerabend's philosophy of quantum physics. In an article memorializing Hanson's death in 1967, and appearing in *Boston Studies in the Philosophy of Science,* Vol. III, Feyerabend says that he changed his views about the Copenhagen interpretation as a result of a series of debates with Hanson, and that by 1966 he had become persuaded of Hanson's view. Hanson brought a different agenda to the philosophy of microphysics than did Feyerabend. Hanson was not driven to defend the possibility of a universalist-realist microphysics, but rather was attempting to explain how the quantum theory as well as other theories are discovered. More specifically he focused on the rôle of semantics of observation and of theory language in the discovery process.

Pursuit of their two agendas brought Feyerabend and Hanson into conflict. Integral to Hanson's agenda was the belief that the duality thesis will be contained in any future microphysical theory. This belief, which Hanson held with strong conviction, was due to the personal influence of P.A.M. Dirac, the Nobel-laureate physicist who developed the field quantum theory in 1928. On the other hand Feyerabend's agenda at that time was that a universalist-realistic microphysical theory is possible, precisely because the duality thesis need not be contained in any future microphysics, since according to Thesis I the observed fact of duality can be revised by a new microphysical theory.

Hanson's principal statement of his philosophy of science is set forth in his *Patterns of Discovery* (1958). In this work he recognizes the interdependence of observation and theory in a manner similar to Feyerabend's Thesis I, and Hanson describes observation as "theory-laden". In the "Introduction" to his *Realism, Rationalism and Scientific Method* (1981) Feyerabend comments that his Thesis I is not exactly the same as Hanson's doctrine that observation is theory-laden, because unlike Hanson, Hesse and others, he maintains that observation terms are fully theoretical and have no purely observational core. Feyerabend's view is thus slightly different from Hanson's thesis of "phenomenal seeing". Nonetheless

Hanson was no more sympathetic than Feyerabend to Bohr's view that the concepts of classical physics must be used for observation in all of physics.

Hanson criticizes Feyerabend by maintaining that duality is stated by the quantum theory formalism itself, and that duality is not merely a philosophical thesis appended to the formalism, which might be replaced by an alternative interpretation not expressing duality. Hanson finds the duality thesis stated by the mathematics of the de Broglie-Einstein relations and also by the Dirac operator calculus, which enables any wave-mechanical description to be transformed into an equivalent matrix-mechanical one. Feyerabend seems not actually to have maintained the position that Hanson criticizes, even in the first of his two approaches to a realistic microphysics given in "Complementarity" (1958).

However, Hanson repeats this line of attack nearly ten years later in "Physical Implications of Quantum Physics" in *The Encyclopedia of Philosophy* (1967), where he characterizes Feyerabend as maintaining that the metaphysical views in the Copenhagen interpretation should be abandoned as indefensible, and that the minimal scientific content consisting of algebraic transformations and factual data is quite compatible with some interpretation markedly different from the Copenhagen one. Perhaps this is just the way in which Hanson viewed Feyerabend's call for a new microphysics without duality, even though Feyerabend was very clear in stating that his preferred second approach is not just an alternative interpretation of the elementary quantum theory, but rather is an entirely new microphysical theory related to elementary quantum theory as Einstein's relativity theory is to Newtonian physics.

Nonetheless, the thrust of Feyerabend's attack is against Bohr's thesis that classical concepts in the complementarity description of the fact of duality must occur in microphysics including any future microphysics. In "Comments on Feyerabend's 'Niels Bohr's Interpretation of the Quantum Theory'..." (1959) Hanson sets forth what he considers to be the minimal essentials of the Copenhagen interpretation: Firstly he maintains that past and present microphysical experience make it probable but in no sense necessary, that any future microphysical theory will incorporate the quantum postulate and the duality principle. Secondly he notes that there presently exists no coherent, currently workable and fully articulated conception of a microphysical theory, which can do without the quantum postulate and the duality principle. He maintains that Feyerabend is correct to score the strident statements of Bohr and Rosenfeld, when they violate the history of physics by suggesting that any future microphysics will of necessity guarantee things like complementarity.

But Hanson adds that Bohr's metaphysics is not an indispensable part of the Copenhagen interpretation, and he therefore distinguishes the "Copenhagen interpretation" from the "Bohr interpretation". He states that if the Bohr interpretation is "cut away", then what remains is a "liberalized Copenhagen interpretation", which is entirely defensible. And he maintains that there are good contingent arguments in support of the expectation that any future microphysics will incorporate the quantum postulate and the duality principle, and emphasizes that presently there exists no working alternative to the current quantum theory notwithstanding all its awkward features. But Feyerabend's response to Hanson's criticisms did not result in a "liberalized Copenhagen interpretation". What Feyerabend produced is an elevation of the Bohr interpretation to a generalized and quite radical relativistic philosophy of knowledge. It seems unlikely that Feyerabend understood what Hanson wanted to "cut away".

Feyerabend on Relativism, Historicism, and Realism

The consequential outcome of the lengthy debate between Hanson and Feyerabend results less from their discussion about current quantum theory than from their discussion about the future of microphysics, if not also the future of Feyerabend's philosophy. Feyerabend found himself in the unenviable position of having to wait for some future physicist to produce a future scientific revolution in future microphysics that would obligingly comply with his current philosophical specifications. And it may have occurred to Feyerabend that he might have to wait a very very long time, even assuming that future physics were ever to accommodate him at all. In any event he was led to reconsider quite radically his agenda for a realistic microphysics, and so instead of philosophizing to accommodate future physics to his Popperian universalist-realist agenda, he decided to philosophize to accommodate realism to the current quantum theory. Therefore he accepted Hanson's conviction that any future microphysics will very likely contain duality.

But Feyerabend construed this to mean that duality must be expressed by complementarity, and in making his accommodation he did not 'cut away' the Bohr interpretation and proceed with a "liberalized" Copenhagen interpretation, as Hanson had advocated. Instead Feyerabend drew upon Bohr's thesis of the relational nature of quantum states, which Feyerabend saw as contradicting universalist realism, and then generalized on Bohr's relational thesis to affirm a nonuniversalist, relativized realism. Just as either the wave or particle manifestations of microphysical reality are conditioned upon respectively either one or another experimental arrangement, so more generally scientific knowledge is conditioned upon the historical situation and regional circumstances of the scientist. And even more generally all truth and knowledge including the particular Western tradition known as science, must be viewed in this historicist perspective.

It may be noted that Feyerabend had apparently been sympathetic to relativism even before his views on quantum theory had been influenced by Hanson. In 1962 he proposed his thesis of semantic incommensurability at the same time that Kuhn had used the same term to describe scientific revolutions. When critics pointed out the historical relativism implied in Kuhn's use of the incommensurability thesis, Kuhn began to modify the concept so as to evade the relativistic implications. But Feyerabend made no such concession, when he defended use of the idea. In "Consolations for the Specialist" in *Criticism and the Growth of Knowledge* (1971) he defended the relativistic implications of Kuhn's use of incommensurability, saying that the choice between incommensurable cosmologies is a matter of taste. In 1978 in his *Science in a Free Society* Feyerabend references Bohr's relational interpretation of the quantum theory, which Bohr had devised in response to the criticism by Einstein, Podolsky and Rosen, as an example of an incommensurable theory relative to classical physics. In this context he says that the change from one world view described by a theory to another world view described by another theory that is incommensurable with the first, is a change in universal principles, such that one no longer speaks of an objective world that remains unaffected by one's epistemic activities, except when moving within a particular world view. In this 1978 work Feyerabend continues to invoke universal principles. Bohr's relational thesis is referenced merely as an example of incommensurability, and seems not yet to have become integral to Feyerabend's cultural relativism.

But later in his "Introduction" to his *Realism, Rationalism and Scientific Method* (1981) Feyerabend states that quantum theory offers good reason to resist the universal application of his Thesis I and its realistic metaphysics. Logically to reject Thesis I is to reject common sense, and to announce that objectivity is a metaphysical mistake. But what physicists have actually done in effect is to reject the universal application of Thesis I, while still retaining in quantum theory some fundamental properties of common sense. In all but Bohm's hidden-variables quantum theory, a universal realistic interpretation of the quantum theory has been replaced by a "partial instrumentalism". Feyerabend explains that the transition to a partial instrumentalism contains two elements that are not always clearly separated.

The first element is the existence of multiple metaphysical traditions. One tradition usually associated with common-sense arguments in physics is the fact that there actually are relatively isolated objects in the world, and that physicists are capable of describing them. But there are also other metaphysical traditions, such as the Buddhist exercises, that create an experience, which neither distinguishes between subject and object nor recognizes distinct objects.

The second element in the transition to a partial instrumentalism is the choice by the physicist of one or another of these metaphysical traditions, and then the turning of the choice into a boundary condition for research. And this choice of metaphysical traditions, furthermore, is one between different sets of facts, because there are no tradition-independent facts.

He then states that the choice of metaphysical traditions is a choice among "forms of life". Realism itself is thereby relativized to prior choices proceeding from cultural and social values. This is because a people decide to regard those things as real, which play an important rôle in the form of life they prefer. Thus the decision about what is real and what is not, begins with a choice of one or another form of life, and a people reject a universal criticism affirming a realistic interpretation of theories not in agreement with their chosen life form. Conversely realism merely reflects the preference for ideas accepted as foundational for their civilization and even for life itself. In this context instrumentalism is incidental to the choice of one or another theory for realistic interpretation. Instrumentalism is what is not culturally agreeable, and it no longer has the characteristics of a failure or defect. This resembles a thesis in sociology of knowledge in Peter Berger and Thomas Luckmann's *Social Construction of Reality* (1966).

Feyerabend concludes that what has failed is not realism, but rationalism with its universalist criterion for realism. He welcomes the failure of rationalists to explain science in terms of tradition-independent standards and methodologies, because it is a failure to put an end to attempts to adapt science to chosen forms of life. The failure of rationalism has freed science from irrelevant restrictions. He adds that it is furthermore in agreement with the Aristotelian philosophy, which also limits science by reference to common sense, except that in Feyerabend's philosophy the conceptions of the individual philosopher are replaced by the political decisions emerging from the institutions of a free society. This is Feyerabend's thesis of "democratic relativism".

His most mature and elaborate statement of his historicist and relativist philosophy is set forth in his *Farewell to Reason* (1987). In the "Introduction" to this book he writes that science has undermined the universal principles of research, and he asks rhetorically: who would have thought that the boundary between subject and object would be questioned as part of a scientific argument, and that science would be advanced thereby? And yet, as he notes in his next sentence, this is precisely what happened in the quantum theory. Feyerabend explicitly states that he does not deny that there are successful theories using abstract concepts. What he denies is that knowledge should be based on universal principles or theories. Echoing Conant, perhaps without even recognizing so, Feyerabend says that science is a living enterprise as opposed to a body of knowledge, and that it is an historical process, although unlike Conant, Feyerabend's view is historicist and relativist, and also realist.

An important distinction that emerges from Feyerabend's historical relativist philosophy is his distinction between "historical" or "empirical" traditions on the one hand and "theoretical" traditions on the other. This distinction is made in "Historical Background" in *Problems of Empiricism* and later in "Knowledge and the Rôle of Theories" and in "Trivializing Knowledge" in *Farewell to Reason*. His earlier philosophical views are clearly in the theoretical tradition, while his later views are clearly in the historical tradition. However, the distinction is not a fundamental one, because the thesis of his later view is that modern science with its theoretical tradition is a new historical tradition.

All theoretical traditions are really historical traditions according to Feyerabend's later view. On the one hand the members of a theoretical tradition identify knowledge with universality, and they attempt to reason by means of a standardized logic. They distinguish the "real" world from the world of appearances, because they identify the reality with what their universal theories can describe as law-like and stable. And when their universal laws fail, the members of the theoretical tradition issue the "battle cry" stating: "we need a new theory!" In theoretical traditions true knowledge and logic are viewed as universal and independent of cultural traditions or regional circumstances.

On the other hand the members of an historical tradition emphasize what is particular including particular regularities such as Kepler's laws. It produces knowledge that is restricted to certain regions, and which depends on conditions specifying the regions. And this knowledge is relative knowledge of what is true or false. Instead of using a standardized logic, they organize information by means of lists and stories, and they reason by example, by analogy and free association. They emphasize the plurality of knowledge, and consequently the history dependence and culture dependence of knowledge and of all logical standards. Feyerabend notes in this context that the complementarity thesis in modern quantum theory even contains the idea of relative knowledge, due to the relational character of quantum states.

In a discussion on the semantical interpretation of theories in his "Knowledge and the Rôle of Theories" Feyerabend bases his historical relativism on an artifactual theory of the semantics of language. He rejects the idea that there is any truth that is capable of superseding or transcending all traditions and cultures, an idea that he traces to Parmenides. He argues that this belief confounds the properties of ideas with their subject matter. The subject matter remains unaffected by human opinions, and the erroneous implication is that scientific statements describing the subject matter are supposed to be expressions of facts and laws, which exist and govern events no matter what anyone thinks of them. He maintains that the statements themselves are not independent of human thought and action; they are human products. They were formulated with great care to select only the "objective" ingredients of our environment, but they still reflect the peculiarities of the individuals, groups, and societies from which they arose. For example the validity of Maxwell's equations is independent of what people think about electrification. But it is not independent of the culture that contains them; it needs a very special mental attitude inserted into a very special structure combined with quite idiosyncratic sequences of historical developments.

Theoretical traditions are opposed to historical traditions in intention, but not in fact. Scientists trying to create a knowledge that differs from "merely" historical or empirical knowledge, succeeded only in finding formulations which seemed to be objective, universal and logically rigorous, but which in fact are used and interpreted in use in a manner that conflicts with the properties the formulations only seem to have. Modern science is a new historical tradition that has been carried along by a false consciousness. Feyerabend similarly criticizes the metaphysics of scientific realism of the theoretical traditions of science. He construes scientific realism as accepting as real only what is lawful or may be connected by laws, and thereby regards the real to be what exists and develops independently of the thoughts and wishes of researchers.

Feyerabend argues that connecting reality with lawfulness is to define reality in a rather arbitrary manner. Moody gods, shy birds, and people who are easily bored would be unreal, while mass hallucinations and systemic errors would be real. The success of science cannot be a measure of the reality of its ingredients. He notes that to support their view, the scientific realists say that while scientific statements are the result of historical processes, the features of the world are independent of those processes. But he argues that we either consider quarks and gods to be equally real, or we cease to talk about real things altogether. And he adds that to say that quarks and gods are equally real is not to deny the effectiveness of science as a provider of technologies and of basic myths; he intends only to deny that scientific objects and they alone are real. And he adds that the equal reality of quarks and gods does not mean that we can do without the sciences; he acknowledges we cannot. Feyerabend's equating scientific realism with scientism is to set up a straw man.

Feyerabend's Criticism of Popper

Consider firstly Feyerabend's general view toward Popper's philosophy. Initially sympathetic to Popper's philosophy, Feyerabend became one of its most relentless and truculent critics. In *Against Method* he rhetorically describes Popper's views as "ratiomania" and "law-and-order science". As his historical-relativist philosophy became more mature, Feyerabend described the technical procedures of Popper's "critical rationalism" – the hypothesizing, testing, falsification, and new hypothesizing to produce new theories having greater empirical content – as merely rules of thumb, that cannot be taken as necessary conditions for science.

In opposition to Popper, Feyerabend takes sides with Kuhn by maintaining that science is an historical tradition having practices that are not always recognized as explicit rules, and that may change from one historical period to the next. He compares understanding a period in the history of science to understanding a stylistic period in the history of the arts. In both science and the arts periods have an obvious unity, but it is one that cannot be summarized in a few simple rules, and the practices that guide it must be found by detailed historical studies. The general notion of such a unity, which Kuhn calls a "paradigm" and which Lakatos calls a "research programme", will therefore be poor in content. Feyerabend rejects the demands for precision made by some technical philosophers, saying that they are on the wrong track.

Consider secondly Feyerabend's specific criticisms of Popper's views on quantum theory. Feyerabend seems never to have been sympathetic to Popper's propensity interpretation, which represents the participation by the philosopher in the work of the physicist. Even while he was sympathetic to Popper's general philosophy, Feyerabend preferred to encourage physicists rather than to join them as Popper did. Later when Feyerabend reconciled himself to the Copenhagen interpretation, he became explicitly critical of Popper's propensity interpretation. His criticisms of Popper are set forth in his "On A Critique of Complementarity" in *Philosophy of Science* (1968-1969), which he later had reprinted as "Niels Bohr's World View" in *Realism, Rationalism, and Scientific Method* (1981). Popper had offered two interpretations of the statistical quantum theory during his career. The earlier interpretation offered in *Logic of Scientific Discovery* involved a variation on the frequency interpretation of probability, and the later interpretation first advanced in his "Quantum Mechanics without the Observer" (1967) was based on his propensity interpretation of probability. Feyerabend criticizes both these interpretations.

Feyerabend criticizes of Popper's frequency interpretation of Born's statistical quantum theory. He admits that it is not unreasonable, if physicists already know what kinds of entities are to be counted as the elements of the collectives, and if they know that those elements are classical entities. And he agrees with Popper that one cannot draw inferences about the individual properties of the elements. But Feyerabend argues that Popper's view – that the elementary particle always posses a well defined value for all its magnitudes, *i.e.*, position and momentum – is precisely what has been found to be inconsistent with the laws of interference and of the conservation laws. He therefore maintains that a new interpretation of the elements of quantum-mechanical collectives is needed, and that what is being counted as elements is not the number of systems possessing a certain well defined property. Rather what is counted is the number of transitions from certain partly ill defined states into other partly ill defined states, as Bohr had maintained.

Feyerabend's criticizes of Popper's propensity interpretation. Popper viewed probability as a propensity, a physical property comparable to physical forces and pertaining to a whole experimental arrangement for repeatable measurements. The wave function determines the propensity of the states of the particle, in the sense that it gives weights to its possible states. Thus in the two-slit experiment a change in the experimental arrangement such as shutting one of the slits, affects the distribution of the weights for the various possibilities, and thus produces a different wave function. Such a change in the experimental arrangement is analogous to tilting a pin board with the result that a new distribution curve of the rolling balls will differ from the distribution prior to the tilting of the pin board. Popper therefore views quantum mechanics as a generalization of classical statistical mechanics of particles together with the propensity interpretation of probability. Feyerabend says that Popper's propensity interpretation is much more similar to Bohr's view, which Popper attacks, than to Einstein's view, which Popper attempts to defend. He says Popper's thesis that the experimental conditions of the whole physical setup determine the probability distribution is precisely Bohr's relational thesis, when Bohr proposed defining the term "phenomenon" to include the whole experimental arrangement.

But Feyerabend's thesis is furthermore that Bohr's idea of complementarity goes beyond the propensity interpretation by attributing to the experimental arrangement not only probability but also the dynamical variables of the physical system, notably position and momentum. Therefore Popper's thesis that a change in experimental conditions implies a change in probabilities alone is not adequate to account for the kind of changes involved in the two-slit experiment. In other words complementarity asserts the relational character not only of probability, but also of all dynamical magnitudes. Feyerabend agrees with Popper that a change of experimental conditions changes probabilities, but he also says that what led to the Copenhagen interpretation is not merely the fact that there is some change in distribution with a change of experimental arrangement, but also the kind of change encountered: trajectories which from a classical view are perfectly feasible, are forbidden to the particle.

This is because the conservation laws apply not only on the average, so that one could postulate a redistribution without asking for some dynamical cause, but furthermore they apply in each single interaction. Thus a purely statistical redistribution is inadequate; each single change of path must be accounted for. Bohr's resolution consists of the renunciation of particle trajectories, the denial that particles possess well defined position with well defined momenta according to the indeterminacy relations. Feyerabend maintains that Popper confused classical waves with quantum waves, because he neglected the dynamics of the individual particle and construed quantum theory as pure statistics. Popper's claim that the reduction of the wave packet is not an effect characteristic of quantum theory, but rather is an effect of probability in general, and that Popper's claim is incorrect in Feyerabend's view. And Popper's claim that duality is the "great quantum muddle" is in Feyerabend's words nothing but a piece of fiction.

Feyerabend also has a number of other specific criticisms of Popper's philosophy of science, which are summarized in "Historical Background" in *Problems of Empiricism,* the second volume of Feyerabend's collected papers. There are eight such specific criticisms, which may be summarized as follows:

1. Feyerabend notes that theory exchange has not always proceeded by falsification. Noteworthy examples include the transition from the celestial theory of Ptolemy and Aristotle to that of Copernicus, and the transition from Lorentz's theory to Einstein's theory of special relativity. In these cases there were no refuting facts to explain rejection of the preceding theory.

2. The meaning of a hypothesis often becomes clear only after the process that led to its elimination has been completed. The force of this objection seems to be that falsification brings about meaning change, that the decision to accept a test outcome as a falsification is also a decision that affects the semantics of the language involved in the test. Feyerabend

elaborates on this thesis in his "Trivializing Knowledge" in *Farewell to Reason*, a paper criticizing Popper's philosophy. In this paper Feyerabend says that the content of theories and experiment are constituted by the refutations performed and accepted by the scientific community, rather than being the basis on which falsifiability can be decided and refutation determined. He exemplifies this point with the stereotypic theory "every raven is black", and he says that while a white raven falsifies this theory, the refutation depends on the reason for whiteness. A decision must be made as to whether a raven whose metabolic processes make it white, or whose genetic make up has been altered to make it white, or which has been dyed white, constitutes a falsifying instance. Feyerabend says that such decisions are not independent of falsification. He also uses this example to illustrate Lakatos' philosophy of science in "Popper's Objective Knowledge" a critical review of Popper's book in *Problems of Empiricism*. Here he states that what is needed is some insight into the causal mechanism that brought about whiteness, a theory of color production in animals. He also notes that this illustration shows the need for alternative theories in the process of testing.

3. The transition to a new theory may involve a change of universal principles, which breaks the logical links between the theory and the content of its predecessor. This break produces the semantic incommensurability that Feyerabend discussed at length in *Against Method* and in earlier papers. Incommensurability is not only the principal basis for his historical relativism, which Popper opposes, but is also inconsistent with Popper's thesis of scientific progress through increasing empirical content and verisimilitude.

4. Feyerabend rejects Popper's thesis of increasing content for reasons in addition to the occurrence of semantic incommensurability. This is a criticism that Feyerabend discusses at length in *Against Method*, where he states that a new period in the history of science commences with a "backwards movement" to a theory with less empirical content, that gives scientists the time and freedom needed for developing the main thesis of the new theory in greater detail, and also for developing related auxiliary sciences. Scientists are persuaded to follow this backward movement by such "irrational" means as propaganda and *ad hoc* theories that sustain a blind faith in the new theory until it turns into what comes to be regarded as sound knowledge. This is what Feyerabend saw in Galileo's defense of the

Copernican theory, where the relevant auxiliary science needing further development at the time was optics.

5. A closely related criticism of Popper's philosophy is Feyerabend's thesis that *ad hoc* adaptation of a theory may be the right step to take. The *ad hoc* adaptation may be made either to the theory or to the statements of observation. In Popper's philosophy these *ad hoc* adaptations are objectionable as content-decreasing stratagems. But Feyerabend maintains that they disguise the inadequacy of a new theory until the relevant auxiliary sciences can be developed, so that refutation ultimately might not occur.

6. The demand that the scientist look for refutations and take them seriously, will lead to an orderly development only in a world in which refuting instances are rare and turn up at large intervals. But this is impossible since an ocean of anomalies surrounds theories, unless we modify the stern rules of falsification using them only as rules of thumb, and not as necessary conditions for scientific procedure. Feyerabend frequently states elsewhere in his literary corpus that strict falsification would wipe out science as it presently exists, and would never permit it to have come into existence.

7. Popper's demand for increasing content makes sense only in a world that is infinite both quantitatively and qualitatively. On the other hand in a finite world containing a finite number of basic qualities or elements, the aim is firstly to find these elements, and then secondly to show how novel facts can be reduced to them with the help of *ad hoc* hypotheses. He adds that genuine novelty counts as an argument against the methods that produce it. Feyerabend gives no further explanation of what he means by this peculiar criticism, nor does he give any reference to any other part of his corpus for explanation.

8. Finally Feyerabend objects that content increase and the realistic interpretation of the idea that brings it about, restrain human freedom.

Feyerabend's Philosophy of Science

Of the four functional topics considered in philosophy of science the place to begin an overview of Feyerabend's philosophy of science is with scientific criticism.

Scientific Criticism

Given Feyerabend's critique of Popper, it might be said at the outset and at the risk of oversimplification that Popper's philosophy of criticism admits that test-design statements can be revised, but takes as its point of departure the acceptance and agreement about test-design language as a necessary condition for decidable criticism and thus for progress in science. Kuhn and Feyerabend on the other hand choose to examine the practices of criticism and the conditions for progress, where test-design statements are revised or anomalies are ignored, such that tests are nonfunctional as decision procedures. Central to Kuhn and Feyerabend's philosophies is the thesis that the choice of scientific theories is **not** fully decidable empirically, and this thesis is the basis for their attacks on Popper's falsificationism or "critical rationalism".

But Feyerabend and Kuhn also differ. Feyerabend attacks Kuhn's sociological thesis of how the empirical undecidability is resolved. The arbitrariness in criticism permitted by this empirical indeterminacy has been described in various ways. Conant called it "prejudice", Kuhn called it "paradigm consensus", and Feyerabend called it "tenacity". Conant was simply dismayed by the phenomenon he observed in the history of science, but he took it more seriously than did his contemporaries, the positivist philosophers, who preferred to dismiss it as simply unscientific. Conant found that prejudice is too frequently practiced by contributing scientists to be dismissed so easily. He also explicitly admitted the strategic rôle of his own prejudices in his preference for an historical examination of science.

Kuhn did not merely accept prejudice as a frequent fact in the history of science. He saw it as integral to science due to a sociological function that it performs within a scientific community, a function that is a condition for scientific progress. Prejudice, which Kuhn had earlier referred to as the "problem of scientific beliefs", is the sociologically enforced consensus about a paradigm, that is necessary for the scientific community to function effectively and efficiently for solving detailed technical problems referred to by Kuhn as "puzzles". Without the consensus the community could not marshal its limited resources for the exploration or "articulation" of the promises of the paradigm. In Kuhn's concept of science professional discipline becomes synonymous with conformity to the prevailing view defined by the paradigm. The phase during which this conformity is a criterion for criticism and is effectively enforced by social control, is "normal" science.

Feyerabend rejects Kuhn's thesis that prejudice functions by virtue of a sociologically enforced uniformity. In Feyerabend's view any such uniformity is indicative of stagnation rather than progress. Instead, prejudice understood as the principle of tenacity is strategically functional, because it has just the opposite effect that Kuhn thought: it promotes diversity and theoretical pluralism, which in Feyerabend's view are necessary conditions for scientific progress. It might be said that Feyerabend views Kuhn's sociological thesis of normal science as an instance of the fallacy of composition, the fallacy of incorrectly attributing to a whole the properties had by its component parts: just as houses need not have the rectangular shape of their component bricks, so too whole scientific professions need not have the monomaniacal prejudices of their individual members. The prejudice or tenacity practiced by the individual member scientist performs a function that does not obtain, if his whole profession were unanimously to share in his prejudice, his tenaciously held view.

The process by which the individual scientist's tenacity is strategically functional is counterinduction. Its functional contribution occurs due to Thesis I, which says that theory supplies the concepts for observation. Tenacious development of a chosen theory results in the articulation of new facts, which enhance empirical criticism. New facts produced by counterinduction can both falsify currently accepted theories and revitalize previously falsified theories. The revitalization may occur because the new facts occur in sciences that are auxiliary to the falsified theory. This possibility of revitalization justifies the scientist's prejudicial belief in a falsified theory, his apparently "irrational" rejection of falsifying factual evidence.

Aim of Science

Feyerabend's views on scientific criticism lead to the topic of the aim of science. Popper has a well defined and explicit thesis of the aim of science. The aim of science in Popper's view is the perpetual succession of conjectures and refutations, in which each successive conjecture or theory can explain both what had been explained by its falsified predecessor and the anomalous cases that falsified the predecessor. The new theory is therefore more general than its predecessor, while it also replaces and corrects its falsified predecessor. Popper saw the process of refutation as involving a deductive procedure having the logical form of *modus tollens*. And because it is a procedure in deductive logic, it is not subject to cultural or historical change. Popper admits that application of the logic in the sense of experimental identification of the falsifying instances may be problematic and may take several years. But he maintains that the logic of falsification isolates the conditions for scientific progress, and that it represents adequately how science has proceeded historically, when it has proceeded successfully. He maintains that this procedure may be said to have become institutionalized, but its validity, which is guaranteed by deductive logic, does not depend on its institutional status. Its validity is ahistorical, and will never be invalidated by historical or institutional change; it is tradition independent.

Both Kuhn and Feyerabend deny that Popper's vision of the development of science is historically faithful. The principal deficiency in the Popperian vision is its optimistic assessment of the decidability of falsification. Not only do they view the range of nondecidability of scientific criticism to be greater than Popper thinks, but they also view it as having an integral rôle in the process of scientific development. This nondecidability gives the scientist a range of latitude, which he is free to resolve by his strategic choices. Kuhn and Feyerabend disagree on which aims influence these choices, but they agree that they are historical or institutional in nature and may change. Furthermore, such changes involve semantical changes, which introduce an additional dimension to the scientist's freedom of choice, when they involve an incommensurable semantic discontinuity.

Kuhn views incommensurable change as characteristic only of occasional scientific revolutions, with sociologically enforced consensus resisting such change and defining the aim of science during the interrevolutionary periods of normal science. Feyerabend also views incommensurable changes as infrequent, but he does not regard the interim periods as an enforced consensus contributing to scientific progress; instead he views normal science as Kuhn defined it as an impediment to progress. He therefore advocates a much more individualistic aim of science, which he refers to as scientific anarchy. Ironically both Popper and Feyerabend explicitly invoke Trotsky's refrain "revolution in permanence", but their meanings are diametrically opposed. Popper means perpetual conjectures and refutations occurring within an ahistorical institutionalized logical framework for conclusive refutation, while Feyerabend means perpetual institutional change with no controlling tradition-independent framework.

Scientific Explanation

Feyerabend's discussion of scientific explanation contains much more criticism of other philosophers' views than elaboration of his own views. From the outset of his professional career he criticized the deductive-nomological concept of scientific explanation and the logical reductionism advocated by the logical positivists. Initially Feyerabend also considered Bohr's concept of explanation to be a "higher kind of positivism", but he later preferred to view Bohr as a kind of historicist philosopher, due to Bohr's distinctive relationalist interpretation of complementarity in quantum theory. As it happens, Bohr was so naïvely eclectic a philosopher, that positivist, neo-Kantian and historicist characterizations can all find support in his works.

For nearly the first two decades of his career Feyerabend subscribed to Popper's philosophy of science, which contains a concept of scientific explanation requiring universal statements. Popper's philosophy of explanation also contains the idea of deeper levels of explanation, where the depth is determined by the scope or extent of universality of the explanation. Initially Popper proposed his thesis of verisimilitude, according to which the deeper explanations are said to be closer to the truth. Later he does not mention the idea of verisimilitude, but he continued to describe explanations as having greater or lesser depth according to the extent of their universality. And he also continued to describe the universal laws and theories occurring in explanations as having greater or lesser corroboration, because science cannot attain truth in any timeless sense of truth.

After Hanson had persuaded Feyerabend to reconsider the merits of the Copenhagen interpretation of quantum theory, Feyerabend rejected Popper's concept of explanation by logical deduction from universal laws, and instead accepted historicism. He was led to this conclusion both by his incommensurability thesis and by the nonuniversalist implications he found in Bohr's relationalist interpretation of quantum theory. Popper had stated that scientific theories are merely conjectures that may be highly corroborated, but may never be true in any timeless sense. Feyerabend agrees but furthermore says that theories have an even more historical character, since the complementarity thesis in quantum theory demonstrates their regional character. Complementarity makes quantum theory nonuniversal at all times, because it is conditional upon mutually exclusive experimental circumstances; it is not even temporarily universal. Feyerabend thus concluded that universal science, *i.e.*, science containing universal laws and theories, is only apparently universal, and that it is actually a special and recent historical tradition.

Feyerabend's historicist philosophy of scientific explanation is in need of greater elaboration. For example he never related his views to the genetic type of explanation that is characteristic of historicism. Although this type of explanation had been dismissed by positivists as merely an elliptical deductive-nomological explanation, it was discussed seriously by Hanson in "The Genetic Fallacy Revisited" in *American Philosophical Quarterly* (1967). Hanson distinguishes different levels of language, one for historical fact and one for conceptual analysis. He says that the distinction differentiates history of science from philosophy of science, and that the genetic fallacy consists of attempting to argue from premises in the historical level to conclusions in the analytical level. It is clear that given his distinction between the theoretical and historical traditions and the way he relates them, Feyerabend would not admit Hanson's "genetic fallacy" thesis.

Scientific Discovery

The topic of discovery may be taken to refer either to the development of new theories or to the development of new facts. Feyerabend's thesis of counterinduction is a thesis of the development of new facts. Thesis I enables the scientist to use the concepts supplied by new theory to make revised observations. Counterinduction is a thesis of observation according to the artifactual philosophy of the semantics of language, which Feyerabend set forth in his Thesis I. It is unfortunate that Feyerabend never examined Heisenberg's use of Einstein's aphorism for reinterpreting the Wilson cloud chamber observations as an example of counterinduction. But Feyerabend virtually never references anything written by Heisenberg, and it is unlikely that he had an adequate appreciation for the differences between Heisenberg and Bohr's philosophies of quantum theory.

Feyerabend addresses the problem of developing new theories in "Creativity" in his *Farewell to Reason.* In this brief article he takes issue with what other philosophers have often called the heroic theory of invention, the idea that creativity is a special and personal gift. He criticizes

Einstein for maintaining a variation on the heroic thesis. Einstein wrote that theory development is a free creation, in the sense that it is a conscious production from sense impressions. And he renders Einstein as saying that theories are “fictions”, which are unconnected with these sense impressions, even though theories purport to describe a hidden and objective world. Feyerabend maintains that at no time does the human mind freely select special bundles of experience from the labyrinth of sense impressions, because sense impressions are late theoretical constructs and not the beginnings of knowledge.

Feyerabend expresses much greater sympathy for Mach’s treatment of scientific discovery. Mach advanced the idea of instinct, which Feyerabend contrasts with Einstein’s idea of free creation. Feyerabend renders Mach as offering an analysis of the discovery process, according to which instinct enables a researcher to formulate general principles without a detailed examination of relevant empirical evidence. Instinct seems not as such to be inherent, but rather is the result of a long process of adaptation, to which everyone is subjected. Many expectations are disappointed during this process of adaptation, and the human mind retains the results of consequently altered behavior. These daily confirmations and disappointments greatly exceed the number of planned experiments. They are used to correct the results of experiments, which are in need of correction because they can be distorted by alien circumstances. Feyerabend says that according to Mach empirical laws developed from principles proceeding from instinct are better than laws developed from experiment.

In concluding his discussion of the topic of creativity Feyerabend advocates a return to wholeness, in which human beings are viewed as inseparable parts of nature and society, and not as independent architects. He rejects as conceited the view that some individuals have a divine gift of creativity. Feyerabend therefore apparently subscribes to the social theory of invention, as would be expected of a historicist.

Comments and Conclusion

On Kuhn

Consider firstly Kuhn’s attempts at linguistic analysis. As mentioned above Kuhn postulates a structured lexical taxonomy, which he also calls a conceptual scheme, and maintains that it is not a set of beliefs. He calls it instead an “operating mode” of a “mental module” prerequisite to having beliefs, a “module” that supplies and bonds what is possible to conceive. He also says that this taxonomic module is prelinguistic and possessed by animals, and he calls himself a post-Darwinian Kantian, because like the Kantian categories the lexicon supplies preconditions of possible experience, while unlike Kantian categories the lexicon can and does change. But Kuhn’s woolly Darwinist neo-Kantianism is a needless *deus ex machina* for explaining the cognition and communication constraints associated with meaning change through theory development and criticism.

There certainly exists what may be called a conceptual scheme, but it is beliefs that bond and structure it. And what they bond and structure are the components of complex meanings for association with the sign vehicle, morpheme or individual descriptive term. The elementary components are semantic values. These complexes of components function as do Kuhn’s “cluster of criteria” for referencing individuals including contrast sets of terms that he says each language user associates with a descriptive term. Their limits on what can be conceived is Pickwickian, because when empirical testing or informal experience occasion a reconsideration of one or several beliefs, the falsifying outcome can always be expressed with the existing vocabulary and its semantics by articulating the contradiction to the theory’s prediction. The empirically based contradiction partly disintegrates the bonds and structures due to belief in the theory, but not those due to the statements of test design. Semantical reintegration by the formation of new hypotheses is constrained psychologically by language habit. Formulating new hypotheses that even promise to solve the new scientific problem is a task that often demands high intelligence and fertile imagination. And the greater the disintegration due to more extensive rejection of current beliefs, the more demanding the task of novel hypothesizing.

Incommensurability

Two reasons for incommensurability can be distinguished in Kuhn’s literary corpus. The *first* is due to semantic values that are unavailable in the language of an earlier theory but that is contained in the language of a later one. The *second* reason for incommensurability is the semantic restructuring of the taxonomic lexicon. However, only the first reason seems to compel anything that might be called incommensurability in the sense of inexpressibility. Language for a later theory containing descriptive vocabulary enabling distinguishing features of the world for which an earlier theory's language supplies no semantic values seems clearly to make impossible the expression of those distinctions in the earlier theory's language. Obvious examples may include features of the world that are distinguishable with the aid of microscopes, telescopes, X-rays or other observational instruments not available at the time the earlier theory was formulated, but which supply semantics that is expressed in the language of a later theory. However even for some of these novelties Hanson recognized "phenomenal seeing", which may supply some semantical continuity.

This reason for incommensurability can be couched in terms of semantic values, because the meanings attached to descriptive terms are not atomistic; they are composite and have component parts that can be exhibited as predicates in universally quantified affirmations. Belief in the universal affirmation "every raven is black" makes the phrase "black ravens" redundant, thereby indicating that the idea of blackness is a component part of the meaning of the concept of raven. However, all descriptive terms including the term "black" also have composition, because it has a lexical entry in a unilingual dictionary. The smallest distinguishable features available to the language user in his descriptive vocabulary are not exclusively or uniquely associated with any descriptive term, but are elementary semantical components of descriptive language. These elementary distinguishable features of the world recognized in the semantics of a language *at a given point in time* are its "semantic values." Thus semantic incommensurability may occur when theory change consists of the introduction of new semantic values not formerly contained in the language of an earlier theory addressing the same subject.

Kuhn's second reason for incommensurability, lexicon restructuring, does not occasion incommensurability prohibiting expressibility; there is no missing semantics, but instead there is only the reorganization of previously available semantic values. The reorganization is due to the revision of beliefs, which may be extensive and result in correspondingly difficult adjustment not only for the developer of the new theory formulating the new set of beliefs but also for the members of the cognizant profession who must assimilate the new theory. The composite meanings associated with each descriptive term common to both old and new theories are disintegrated to a greater or lesser degree into their elementary semantic values, and then are reintegrated by the statements of the new theory. And concomitant to this restructuring, the users' old language habits must be overcome and new ones acquired. An ironic aspect to this view is that semantic incommensurability due to introduction of new semantic values occurs in developmental episodes that appear least to be revolutionary, while those involving extensive reorganization and thus appear most to be revolutionary introduce no new semantic values and thus have no semantic incommensurability.

Revolutions

In his "Commensurability, Comparability and Communicability", Kuhn says that if scientist's moving forward in time experience revolutions, his gestalt switches will ordinarily be smaller than the historian's, because what the historian experiences as a single revolutionary change will usually have been spread over a number of such changes during the interim historical development of the science. And Kuhn immediately adds that it is not clear that those small incremental changes need have had the character of revolutions, although he retains his wholistic thesis of gestalt switch for revolutionary cases. Clearly the time intervals in the forward movement of the theory-invention must be incremental subject only to the time it took the inventing scientist to formulate his new theory, while the time intervals in the comparative retrospection may be as lengthy as the historian chooses, such as the very lengthy interval considered by Kuhn in his "Aristotle experience" comparing the physics of Aristotle and Newton.

But more than duration of time interval is involved in the forward movement. On the one hand the recognition and articulation of any new semantic values and on the other hand the disintegration and reintegration of available semantic values in the meaning complexes in a lexical restructuring are seldom accomplished simultaneously, since the one process is an impediment to the accomplishment of the other. Attempted reintegration of disintegrating semantics is probably the worst time to attempt introduction of new semantic values. Throwing new semantic values into the existing confusion of conceptual disorientation could only exacerbate and compound the difficulties involved in conceptual reintegration and restructuring. For this reason scientists will attack one of these problems at a time.

Furthermore as noted above new semantic values can at times be articulated with existing descriptive vocabulary, as Hanson exhibited with his thesis of "phenomenal seeing" exemplified by the biologist describing a previously unobserved microbe seen under a microscope for the first time, and for which there is yet no classification. Then later the product of phenomenal-seeing description may be associated with a new “kind word”, *i.e.*, descriptive term that functions as a label for classification of the new phenomenon. And the new “kind word” may then later acquire still more semantics by incorporation into a larger context. Scientific revolutions are reorganizations of available semantic values, and incommensurability due to new semantic values is not found in revolutions except in the periods created by the historian’s sweeping retrospective choices of time intervals for comparison. In the forward movement the new semantic values (or “kind words” based on them) introduced into the current language may be accommodated by a relevant currently accepted law by the extension of that law. Or their introduction may subsequently occasion a modification of the current law by elaborating it into a new and slightly different theory. And new semantic values may eventually lead to revolutionary revisions of current law.

On Feyerabend

Turn next to the philosophy of Feyerabend, which is more elaborate than Kuhn’s. Feyerabend’s began with an agenda for modern microphysics: to show how a realistic microphysics is possible. Initially the conditions that he believed a realist microphysics must satisfy were taken from Popper’s philosophy of science, and these conditions are contained in Popper’s idea of universalism. However, there is an ambiguity in Popper’s “universalism”, and that ambiguity was not only brought into Feyerabend’s agenda while he had accepted Popper’s philosophy, it was also operative in his philosophy after he rejected Popper’s philosophy, because he rejected universalism in *both* senses. The **first** meaning of “universal” refers to the greater scope that a new theory should have relative to its predecessors, and the **second** meaning refers to the universal logical quantification of general statements. Feyerabend’s acceptance of Bohr’s interpretation of the quantum theory led him to reject universalism in both of Popper’s senses, and consequently to advance his radical historicist philosophy of science.

Feyerabend had adequate reason to reject universalism in Popper’s first sense, the sense of greater scope. If it is not actually logically reductionist, as Feyerabend sometimes says, it does gratuitously require an inclusiveness that demands that a new theory explain the domain of the older one. But there are historic exceptions that invalidate such a demand. Feyerabend notes explicitly in his *Against Method* for example that Galileo’s theory of motion is less universal than Aristotle's doctrine of the four types of cause, which explained qualitative change as well as mechanical motion.

With respect to Popper's second sense of universalism Feyerabend believes that his Thesis I with its dependence on universal logical quantification cannot be applied to quantum theory due to Bohr's semantical thesis of complementarity, which is duality expressed with inconsistent classical concepts. Feyerabend thus finds incommensurability within quantum theory, and he therefore rejects universalism in the sense of universal logical quantification. This rejection involves a semantical error that is made by many philosophers including both the positivists and the Copenhagen physicists. That semantical error consists of implicitly regarding the meanings of descriptive terms or variables, or even larger units of language, as unanalyzable wholes. A semantical metatheory of meaning description that enables analysis of semantical composition of the meanings of the descriptive terms is needed to see how universal logical quantification is consistent with duality without Bohr's complementarity.

Componential semantics

Below are some preliminary considerations for such a semantical analysis, which might serve for a modification of Feyerabend's Thesis I. Since Quine's rejection of the analytic truth, and notwithstanding the fact that he rejected analyticity altogether, the analytic-synthetic distinction may still be viewed as a pragmatic one instead of a semantic one, such that any descriptive universally quantified statement believed to be true, may be viewed as both analytic and synthetic instead of dichotomously, *i.e.*, it may be viewed as what Quine calls an "analytical hypothesis". The laws found in physics and in many other sciences use mathematical syntax, where universal quantification is expressed implicitly by letting the numeric variables have no measurement values; the variables await assignment of their measurement values by execution of a measurement procedure or by evaluation in an equation from other variables having measurement values already assigned. Furthermore the universality in mathematical language is claimed only for measurement instances; it makes no ontological reference to entities. The following analysis applies to mathematically expressed language, but for the sake of simplicity the analysis is here given in terms of categorical statements, because such statements have explicit syncategorematic quantifiers.

Consider next a list of universally quantified affirmations having the same subject term, and which are believed to be true. The concepts associated with the descriptive terms predicated of the common subject by the several categorical affirmations in the list exhibit a composition or complexity in the meaning of the subject term. The meaning of the subject term may therefore be said to have component parts consisting of the predicating concepts, and its meaning thus need not be viewed wholistically.

Consider in turn the relations that may obtain among the concepts that are universally predicated in the believed universal affirmations having the common subject term. These predicate terms may or may not be related to each other by other universal statements. If any of the predicate concepts are related to one another by universally quantified negative statements, then the common subject term in the statements in the list is equivocal, and the predicate concepts related to one another by universal negations are parts of different meanings of the equivocal subject term. Otherwise the subject term common to the statements in the list is univocal, whether or not the predicate concepts are related to one another by universally quantified affirmations, and the predicate concepts are different component parts of the one meaning of the univocal subject term.

Terms are either univocal or equivocal; concepts are relatively clear or vague. **All concepts are always more or less vague**, but vagueness may be reduced by adding or excluding semantic values. Adding universal affirmations to the list of universally quantified affirmations having the same subject term believed to be true reduces the vagueness in their common subject term by clarifying the meaning of the shared subject term with respect to the added predicate concepts that contain the added semantic values. Asserting universal negations relating concepts predicated of the common subject also clarifies the meaning of the subject term by showing equivocation and thus excluding semantic values. And asserting universal affirmations relating the concepts predicated of the common subject, clarifies the meaning of the subject term by revealing additional structure in the meaning of the common univocal subject term, and making a deductive system.

Semantics of Experiments

Now consider science: In all scientific experiments the relevant set of universal statements is dichotomously divided into a subset of universal statements that is presumed for testing and the remainder subset of universal statements that is explicitly proposed for testing. The division is pragmatic. The former subset is called "test-design statements" and the latter subset is called "theory statements". The test-design statements identify the subject of the test and the test procedures, and are presumed true for the test.

Consider a descriptive term that is a subject term in any one of the universal statements in the above-mentioned set, and that is common to both the test-design statements and the theory statements in the divided set. The dual analytic-synthetic nature of all of the universal statements makes the common subject term have part of its semantics supplied by the concepts that are predicated of it in the test-design subset of statements. This part of the common subject term's semantics remains unchanged through the test, so long as the division between theory and test-design statements remains unchanged. The proponents and advocates of the theory remainder-set of statements presumably believe that the theory statements are true with enough conviction to warrant empirical testing. But their belief does not carry the same high degree of conviction that they have invested in the test-design statements.

Before the execution of a test of the theory, all scientists interested in the test outcome agree that the universally quantified test-design statements and also the particularly quantified language that describes the test's initial conditions and its outcome with semantics defined in the universally quantified test-design statements, are believed true independently of the theory. Thus if the test outcome shows an inconsistency between the characterization supplied by the test-outcome statements and the characterization made by theory's prediction statements, the interested scientists agree than it is the theory that is to be viewed as falsified and not the universally quantified test-design statements. This independence of test-design and test-outcome statements is required for the test to be contingent, and it precludes the test-design statements from either implying or denying the theory to be tested or any alternative theory that addresses the same problem. Therefore for the cognizant scientific profession the semantical parts defined by the test-design statements before test execution leave the test-design's constituent terms effectively vague, because test-design statements are silent with respect to any theory's claims.

Notwithstanding that the originating proposer and supporting advocates of the theory may have such high confidence in their theory, that for them the theory may supply part of the semantics for its constituent terms even before testing, they have nonetheless agreed that in the event of a falsifying test outcome the test-design language trumps the theory. This amounts to saying that functionally the theory does not define any part of the semantics of its constituent terms that are common to the test design. Or in other words the test-design statements assumed the vague semantical status that Heisenberg called the physicist's "everyday" concepts.

After the test is executed in accordance with its test design, the particularly quantified test-outcome statements and the theory's particularly quantified prediction statements are either consistent or inconsistent with one another (after discounting empirical underdetermination not attributable to failure to execute the test in accordance with the agreed test design). In other words they either characterize the same observed or measurement instances or they do not. If the test outcome is an inconsistency between the test-outcome description and the theory's prediction, then the theory is falsified. And since the theory is therefore no longer believed to be true, it cannot contribute to the semantics of any of its constituent descriptive terms even for the proposer and advocates of the theory.

But if the test outcome is not a falsifying inconsistency between the theory's prediction and the test-outcome description, then for each term common to the theory and test design the semantics contributed by the universally quantified test-design and theory statements are component parts of the univocal meaning complex of each shared descriptive term, and they identify the same instances. The additional characterization supplied by the semantics of the tested and nonfalsified theory statements thereby resolves the vagueness that the meaning of the common descriptive terms had before the test, especially for those who did not share the conviction had by the theory's proposers and advocates.

Nonfalsified theory redefines the test design

In some sciences such as physics a theory's domain may include the test-design domain for the theory. As stated above, *before* the test execution of such a theory and *before* the test outcome is known, the test-design language must be vague about the tested theory's domain, in order for the test to be independent of the theory's description. But if *after* the test the outcome is known to be nonfalsification of the tested theory, then the nonfalsified theory has become a law, and the domain of the test-design language at least in principle may be describable with the language of the nonfalsified theory now a law. This application of the tested and nonfalsified theory to its test domain changes the semantics of the test-design statements by still further resolving the vagueness in the test-design language.

While the vagueness in the concept associated with the common subject term is reduced by a nonfalsifying test outcome, the vagueness in the concepts predicated of the subject term by the two sets of statements is not necessarily resolved by the relation of the predicate concepts to one another merely by the nonfalsifying test outcome. Resolution of the vagueness in these predicate concepts requires additional universal statements relating the predicates in the tested and nonfalsified theory and test-design statements. Such would be the case were the statements formerly used as independent test-design statements revised, such that they could be incorporated into a deductive system and thus derived from the nonfalsified theory after the test. The resulting deductive system makes the universally quantified test-design statements logical consequences of the new laws due to the theory having been tested and not falsified. But this loss of independence of the test-design statements is no longer important for the test, since the nonfalsifying test outcome is known. This amounts to deriving from the theory a new set of laws applicable to the functioning of the apparatus and physical procedures of an experiment described by the test-design statements.

In 1925 when rejecting positivism Einstein told Heisenberg that the physicist must assume that this can be done. Einstein argued that it is in principle impossible to base any theory on observable magnitudes alone, because in fact the very opposite occurs: **it is the theory that decides what the physicist can observe**. Einstein argued that when the physicist claims to have observed something new, he is actually saying that while he is about to formulate a new theory that does not agree with the old one, he nevertheless must assume that the new theory covers the path from the phenomenon to his consciousness and functions in a sufficiently adequate way, that he can rely upon it and can speak of observations. The claim to have introduced nothing but observable magnitudes is actually to have made an assumption about a property of the theory that the physicist is trying to formulate. But Einstein required only an assumption, not an actual deductive derivation.

Feyerabend's universality criterion

Feyerabend's first criterion of universality set forth in his Thesis I *requires* that the test-design laws, which describe the macrophysical experimental set up, must be incorporated into a deductive system consisting of the microphysical quantum theory in a manner analogous to the incorporation of Kepler's empirical laws into Newton's theory enabled by the approximate nature of Kepler's laws.

As it happens, contrary to Bohr's instrumentalist thesis and to Heisenberg's closed-off-theories doctrine but consistent with Heisenberg's pragmatic semantical views, the microphysical phenomena can be described with the semantics of the quantum theory and without classical concepts. This is what Heisenberg did when he construed the observed tracks in the Wilson cloud chamber using his quantum theory. But he offered no quantum description of the functioning of the macrophysical apparatus, *i.e.*, the Wilson cloud chamber by means of laws logically derived from the quantum theory.

Since the 1990's there has been a successful replacement of the traditional language with its classical concepts by a new language, which is better adapted to the mathematics of quantum theory. In his *Understanding Quantum Mechanics* (1999) Princeton University physicist Roland Omnès reports that recent conceptual developments using the Hilbertian framework have enabled all the features of classical physics to be derived directly from Copenhagen quantum physics. And he says that this mathematics of quantum mechanics is a "universal language of interpretation" for both microphysical and macrophysical description. This new language accomplishes what Bohr's "complementarity" use of classical concepts cannot. Furthermore the deductive relationship has not only resolved the vagueness in the semantics of Heisenberg's "everyday" language, but because it is deductive, it has even ***further*** resolved the vagueness in the semantics of the vocabulary in both macrophysics and microphysics.

Alternative to relativism and deductivism

Contrary to Feyerabend, relativism is not the exclusive alternative to deductivism. The choice between classical and derived quantum macrophysical descriptions is a false dichotomy. The universal test-design statements, such as those describing the experimental set up, need not say anything about the fundamental constitution of matter; that is what the microphysical theory describes. The pretest independent test-design statements are vague with respect to any microphysics, and Heisenberg's term "everyday" is appropriate to describe the vague concepts associated with these terms. After a nonfalsifying test the semantics supplied by the quantum theory provides further resolution of the concepts associated with the terms common to both test-design and theory statements. The vagueness in the "everyday" concepts is never resolved into classical concepts. The whole meaning complex constituting each concept is more properly called a "quantum" concept, given that the quantum theory is not falsified, because the quantum theory resolves vagueness by the addition of the quantum-theory-defined meaning parts to each whole meaning complex. And it is for this reason Heisenberg was able to use quantum concepts when he described the *observed* free electron in the Wilson cloud chamber, since those concepts were resolved by the quantum context supplied by his matrix mechanics and later his by indeterminacy relations.

Summary

In summary, semantical analysis reveals that duality need not be expressed in classical terms by Bohr's complementarity principle, because the semantics of the descriptive terms used for observation are not simple, wholistic, or unanalyzable, and because prior to testing the semantics of these terms cannot imply an alternative description to that set forth by the quantum theory, in order for testing to have the contingency that gives it its function as an empirical decision procedure in the practice of basic science. Feyerabend was closer to the mark with the first of his two approaches to realism in microphysics set forth in his "Complementarity" (1958), and he might have retained universalism – universal quantification – in quantum theory had he ignored the reductionist program, and had he developed a metatheory of semantical description, and then appropriately modified his Thesis I. With appropriate modification as described above, the application of Feyerabend's Thesis I to the quantum theory need not imply deductivism, and he need not have opted for historical relativism and rejected universalism in the sense of universal quantification.

The quantum theory with its quantum postulate, its duality thesis, and its indeterminacy relations has no need for Newtonian semantics, either before, during, or after any empirical test. It is a universal theory with a univocal descriptive vocabulary, and it is not semantically unique in empirical science due to any internal incommensurability. Had Feyerabend considered Heisenberg's realistic philosophy of the quantum theory, he would probably not have been driven to advocate his incommensurability and historical relativist theses, in order to implement a realistic agenda for microphysics. Then instead of speaking of the Galileo-Einstein tradition, he could have referenced the Galileo-Einstein-Heisenberg tradition including Heisenberg's pluralism.

Incommensurability between theories

Consider further Feyerabend's incommensurability thesis, which is central to his historical relativism. Rejecting the naturalistic theory of the semantics of language including the language of observational description enables dispensing altogether with classical concepts in quantum theory, and thereby with incommensurability within the quantum theory. But Feyerabend sees incommensurability in Bohr's complementarity thesis only as a special case, a case that is intrinsic to a single theory due to the use of classical concepts.

Feyerabend also treats incommensurability as a relation between successive theories, and he maintained the existence of incommensurability even before he adopted Bohr's interpretation of quantum theory. In his earlier statements of the thesis he says that two theories are incommensurable, if they can have no common meaning, because there exists no general concept having an extension including instances described by both theories. The two theories therefore cannot describe the same subject matter, and are therefore incommensurable.

In *Against Method* he also referenced Whorf's thesis of linguistic relativity to explain incommensurability in terms of covert resistances in the grammar of language. There he maintains that these covert resistances in the grammar of an accepted theory not only lead scientists to oppose the truth of a new theory, but also lead the scientists to oppose the presumption that the new theory is an alternative to the older one. He considers both the quantum theory and the relativity theory to be incommensurable in relation to their predecessor, Newtonian mechanics. However, he offers no evidence for his highly implausible historical thesis that the advocates of Newtonian physics had failed to recognize that either quantum theory or relativity theory is an alternative to Newtonian physics at the time of the proposal of these new theories.

Incommensurability as inexpressibility

Feyerabend furthermore maintains that since incommensurability is due to covert classifications and involves major conceptual changes, it is hardly ever possible to give an explicit definition of it. He says that the phenomenon must be shown, and that one must be led up to it by being confronted with a variety of instances, so that one can judge for oneself. Feyerabend's concept of incommensurability suffers from the same obscurantism as Kuhn's concept of paradigm. Readers of Feyerabend must rely on his identification of which transitional episodes in the history of science are to be taken as involving incommensurability and which ones do not, just as Kuhn's readers must rely on the latter's identification of which transitional episodes are transitions to a new and incommensurable paradigm and which ones are merely further articulations of the same paradigm, as Shapere had complained. Although the two philosophers do not hold exactly the same views on the nature of incommensurability, and while they disagree about Kuhn's thesis of normal science, neither developed a metatheory of semantical description that would enable clear and unambiguous individuation theories and thus characterization of semantical continuity and discontinuity through scientific change. Feyerabend's recourse to the Wittgensteinian-like view that incommensurability cannot be defined but can only be shown, may reasonably be regarded as evasive in the absence of such a semantical metatheory.

Semantics of the eclipse experiment

The semantics of the Newtonian and relativity theories that Feyerabend says are incommensurable may be examined by considering their synthetic statements analytically for semantical analysis. By way of example consider one of the more famous empirical tests of Einstein's general theory of relativity, the test that had a formative influence on Popper. Two British astronomers undertook this test known as the "eclipse experiment", Sir Arthur Eddington of Cambridge University and Sir Frank Doyle of the Royal Greenwich Observatory. The test consisted of measuring the gravitationally produced bending of starlight visible during an eclipse of the sun that occurred on May 29, 1919, and then comparing measurements of the visible stars' positions with the different predictions made by Einstein's general theory of relativity and by Newton's celestial mechanics. The test design included the use of telescopes and photographic equipment for recording the telescopic images of the stars. Firstly reference photographs were made during ordinary night darkness of the stars that would be visible in the proximity of the eclipsed sun. These photographs were used for comparison with photographs of the same stars made during the eclipse. The reference photographs were made with the telescope at Oxford University several months prior to the eclipse, when these stars would be visible at night in England.

Then the astronomers journeyed to the island of Principe off the coast of West Africa, in order to be in the path of the total solar eclipse. During the darkness produced by the eclipse they photographed the stars that were visible in the proximity of the sun's disk. They then had two sets of photographs: An earlier set displaying images of the stars unaffected by the gravitational effects of the sun, and a later set displaying images of the stars near the edge of the disk of the eclipsed sun and therefore produced by light rays affected by the sun's gravitational influence. The stars in both sets of photographs that are farthest from the sun in the eclipse photographs are deflected only negligibly in the eclipse photograph. And since different telescopes were used for making the two sets of photographs, reference to these effectively undeflected star images was used to determine an overall magnification correction for the different telescopes. And correction furthermore had to be made for distorting refraction due to atmospheric turbulence and heat gradients, because the atmospheric distortions are large enough to be comparable to the effect being measured. But they are also random from photograph to photograph, and the correction can be made by averaging over the many photographs. Such are the essentials of the design of the Eddington eclipse experiment.

The test outcome is as follows: The amount of deflection calculated with the general theory of relativity is 1.75 arc seconds. Eddington's findings showed a deflection of 1.60 ± 0.31 arc seconds. The error in these measurements is small enough to conclude that Einstein's general theory is valid, and that the Newtonian celestial mechanics can no longer be considered valid. Later more accurate experiments have reduced the error of measurement, thereby further validating the relativity hypothesis. In this experiment the test-design statements include description of the optical and photographic equipment and of their functioning, of the conditions in which they were used, and of the photographs of the measured phenomenon made with these measurement instruments. These statements have universal import, since they describe the repeatable experiment, and are presumed to be true characterizations of the experimental set up. The theory statements are also universal, and each theory – Einsteinian relativistic physics and Newtonian classical physics – shares descriptive variables with the same set of test-design statements. Since the test-design statements may be viewed as analytic statements, any descriptive variable occurring both in a test design statement and in both theories has univocal semantics with respect to the semantic values contributed by the test-design statements. **This test-design semantics is shared by both theories, and it makes the theories semantically commensurable.**

Feyerabend maintains that theories are incommensurable, because there is no concept that is general enough to include both the Euclidian concept of space occurring in Newton's theory and the Reimannian concept occurring in Einstein's theory. In fact the common part of the meanings in the semantics of the descriptive terms common to the two theories and to the test-design statements, are not common meanings due to a more general geometrical concept. There is a common meaning because the test-design statements are silent about the claims made by either theory, even as both the theories claim to reference the same instances that the test-design statements definitively describe. ***Before*** the test this silence is due to the vagueness in the common part of the meaning of the terms shared by the theory statements and defined by the test-design statements. In the case of the test design for Eddington's eclipse experiment, it may be said that before the test the meanings contributed by the test-design statements are not properly called either Newtonian or Einsteinian. For purposes of describing the experimental set up, their semantics have the status of Heisenberg's "everyday" concepts that are silent about the relation between parallel lines at distances very much greater than those in the apparatus.

After the test is executed, the nonfalsification of the relativistic theory and the falsification of the Newtonian theory are known outcomes of the test. This acceptance of the relativity theory is a pragmatic determination giving it the semantically defining status of analytic statements, and the statements of the theory – now a law – supply part of the semantics for each descriptive term common to the theory and the test-design statements. This semantical contribution by the former theory to each of these common descriptive variables may be said to resolve some of the vagueness in the whole meaning complex associated with each of these common terms. Thus the common terms no longer have Heisenberg's "everyday" status, but have Einsteinian semantics. No Newtonian semantics is involved.

The semantics supplied to these terms by their test-design statements is still vague, because all meanings are always vague, although less so than before the test outcome is known. However, were the test-design statements subsequently derived logically from the nonfalsified relativity theory, then these common terms would receive still more Einsteinian semantic values and additional structure from the accepted relativity theory. The laws constituting the universal test-design statements would have been made a logically derived extension of the nonfalsified relativity theory. In this case formerly "everyday" concepts receive further resolution of their vagueness as descriptive terms in the test-design statements. Thus regardless of whether or not the test-design statements describing the experimental set up can be logically derived from the relativity theory, no resolution of the "everyday" concepts to Newtonian concepts is involved either before, during, or after the test, except perhaps for the convinced advocates of the Newtonian theory before their accepting the latter theory's falsification. But after the test outcome falsifying the Newtonian theory, even the most convinced advocates of the Newtonian theory must accept the semantically controlling rôle of the test-design statements, or simply reject the test design.

Newtonian confusion

Nonetheless some physicists incorrectly refer to the concepts in the test-design statements for testing relativity theory as Newtonian concepts even after the nonfalsifying test outcome. This error occurs because any relativistic effects in the test equipment are too small to be detected or measured, and therefore do not jeopardize the conclusiveness of the test. For example two different telescopes were used in the Eddington eclipse experiment to produce sets of photographs, one used before the eclipse and another during the eclipse. Since the resulting photographs had to be compared, a correction had to be made for differences in magnification. But no correction was attempted for the different deflections of starlight inside the telescopes due to the different gravitational effects of the different masses of the different telescopes even by those who believed in the relativity theory, because such differential relativistic effects are not empirically detectable. But this empirical underdetermination does not imply that the test-design statements ever affirmed the Newtonian theory. For the test to have any contingency the test-design statements must be silent about the tested theory and any alternative to it.

Cultural relativism

In addition to Bohr's complementarity thesis and his own incommensurability thesis, Feyerabend is also led to his radical historicism by his thesis that whether in philosophy of science or in any social science, cultural views and values including the criteria and research practices of empirical science are inseparable from historical conditions. In its radical variant historicism precludes the validity of universals altogether saying that particular historical circumstances cannot supply identical initial conditions for universally quantified theories describing recurrent aspects of human social behavior. The objection to historicism is firstly that concepts are inherently universal (or as Popper says, all terms are disposition terms). And secondly that the hypothetical character of universally quantified empirical statements does not as such invalidate them.

Truth is relative to what is said, because it is a property of statements; statements about reality are more or less true and false, while reality just exists. The scientific revolutions of the twentieth century led philosophers and specifically pragmatists to affirm relativized semantics, and therefore to affirm that meaning and belief are mutually conditioning. Reality imposes a constraint – the empirical constraint – on this mutual conditioning in language that enables falsification. In empirical science the locus of the falsification is by prior decision assigned to a proposed universally quantified hypothesis, *i.e.*, a theory, because it is conditioned upon previously selected universal test-design statements. Outside the limits of empirical underdetermination – measurement error and conceptual vagueness – truth conditioning imposed on universal statements linking initial conditions and test outcomes is not negotiable once test-design statements are formulated and accepted. But falsifying experiences anomalous to our universal beliefs may force revisions of those universal empirical beliefs and therefore of their semantics.

Critique of Popper's falsificationism

The evolution of thinking from Conant's recognition of prejudice in science to Feyerabend's **counterinduction** thesis has brought to light an important limitation in Popper's falsificationist thesis of scientific criticism. In this respect Feyerabend's philosophy of science represents a development beyond Popper, even after discounting Feyerabend's historicism. Popper had correctly rejected the positivists' naturalistic philosophy of the semantics of language, and maintained that every statement in science can be revised. But the paradigmatic status he accorded to Eddington's 1919-eclipse experiment as a crucial experiment had deflected Popper from exploring the implications of the artifactual semantics thesis, because he identified all semantical analysis with essentialism. He saw that the decidability of a crucial experiment depends on the scientist "sticking to his problem". But he further maintained that the scientist should never redefine his problem by reconsidering any experiment's test design after the test outcome has been a falsification of the proposed theory. Such revisions in Popper's view have no contributing function in the development of science, and are objectionable as *ad hoc* content-decreasing stratagems, *i.e.*, merely evasions.

But the prejudiced or tenacious response of a scientist to an apparently falsifying test outcome may have a contributing function in the development of science, as Feyerabend illustrates in his examination of Galileo's arguments for the Copernican cosmology. Use of the apparently falsified theory as a "detecting device" by letting his prejudicial belief in the heliocentric theory control the semantics of observational description, enabled Galileo to reconceptualize the sense stimuli and thus to reinterpret observations previously described with the equally prejudiced alternative semantics built into the Aristotelian cosmology. This was also the strategy used by Heisenberg, when he reinterpreted the observational description of the electron tracks in the Wilson cloud chamber experiment with the semantics of his quantum theory pursuant to Einstein's anticipation of Feyerabend's Thesis I, *i.e.*, that theory decides what the scientist can observe.

As it happens, the cloud chamber experiment was not designed to decide between Newtonian and quantum mechanics. The water droplets suggesting discontinuity in the condensation tracks are very large in comparison to the electron, and the produced effect admits easily to either interpretation. But Heisenberg's reconceptualization of the sense stimuli led him to develop his indeterminacy relations. In the eclipse experiment in 1919 the counterinduction strategy could also have been used by tenacious Newtonians who chose to reject the Eddington's findings. Conceivably the artifactual status of the semantics of language permits the dissenting scientists to view the falsifying test outcome as a refutation of one or several of Eddington's test-design statements rather than as a refutation of the Newtonian theory. Or more precisely, what some scientists view as definitive test-design statements, others may decide to view as falsified theory.

Another historic example of counterinduction, of using an apparently falsified theory as a detecting device, is the discovery of the planet Neptune. In 1821, when Uranus happened to pass Neptune in its orbit – an alignment that had not occurred since 1649 and was not to occur again until 1993 – Alexis Bouvard developed calculations predicting future positions of the planet Uranus using Newton's celestial mechanics. But observations of Uranus showed significant deviations from the predicted positions.

A first possible response would have been to dismiss the deviations as measurement errors and preserve belief in Newton's celestial mechanics. But astronomical measurements are repeatable, and the deviations were large enough that they were not dismissed as observational errors. They were recognized to be a new problem.

A second possible response would have been to give Newton's celestial mechanics the hypothetical status of a theory, to view Newton's law of gravitation as falsified by the anomalous observations of Uranus, and then attempt to revise Newtonian celestial mechanics. But by then confidence in Newtonian celestial mechanics was very high, and no alternative to Newton's physics had been proposed. Therefore there was great reluctance to reject Newtonian physics.

A third possible response, which was historically taken, was to preserve belief in the Newtonian celestial mechanics, propose a new auxiliary hypothesis of a gravitationally disturbing phenomenon, and then reinterpret the observations by supplementing the description of the deviations using the auxiliary hypothesis of the disturbing phenomenon. Disturbing phenomena can "contaminate" even supposedly controlled laboratory experiments. The auxiliary hypothesis changed the semantics of the test-design description with respect to what was observed. In 1845 both John Couch Adams in England and Urbain Le Verrier in France independently using apparently falsified Newtonian physics as a **detecting device** made calculations of the positions of a disturbing postulated planet to guide future observations in order **to detect** the postulated disturbing body. In September 1846 using Le Verrier's calculations Johann Galle observed the postulated planet with the telescope at the Berlin Observatory.

Theory is language proposed for testing, and test design is language presumed for testing. But here the status of the discourses was reversed. In this third response the Newtonian gravitation law was not deemed a tested and falsified theory, but rather was presumed to be true and used for a new test design. The new test-design language was actually given the relatively more hypothetical status of theory by supplementing it with the auxiliary hypothesis of the postulated planet characterizing the observed deviations in the positions of Uranus. The nonfalsifying test outcome of this new hypothesis was Galle's observational **detection** of the postulated planet, which Le Verrier named Neptune.

But counterinduction is after all just a discovery **strategy**, and Le Verrier's counterinduction effort failed to explain a deviant motion of the planet Mercury when its orbit comes closest to the sun, a deviation known as its perihelion precession. He presumed to postulate a gravitationally disturbing planet that he named Vulcan and predicted its orbital positions in 1843. But unlike Le Verrier and most physicists at the time, Einstein had given Newton's celestial mechanics the hypothetical status of theory language, and he viewed Newton's law of gravitation as falsified by the anomalous perihelion precession. He had initially attempted a revision of Newtonian celestial mechanics by generalizing on his special theory of relativity. This first attempt is known as his *Entwurf* version, which he developed in 1913 in collaboration with his mathematician friend Marcel Grossman. But working in collaboration with his friend Michele Besso he found that the *Entwurf* version had clearly failed to account accurately for Mercury's orbital deviations; it showed only 18 seconds of arc each century instead of the actual 43 seconds.

In 1915 he finally abandoned the *Entwurf* version with its intuitive physical ideas carried over from Newton's theory, and under prodding from the mathematician David Hilbert turned to mathematics exclusively to produce his general theory of relativity. He then developed his general theory, and in November 1915 he correctly predicted the deviations in Mercury's orbit. He received a congratulating letter from Hilbert on "conquering" the perihelion motion of Mercury. After years of delay due to World War I his general theory was vindicated by Arthur Eddington's famous eclipse test of 1919. Some astronomers reported that they observed a transit of a planet across the sun's disk, but these claims were found to be spurious when larger telescopes were used, and Le Verrier's postulated planet Vulcan has never been observed.

Le Verrier's response to Uranus' deviant orbital observations was the opposite to Einstein's response to the deviant orbital observations of Mercury. Le Verrier reversed the roles of theory and test-design language by preserving his belief in Newton's physics and using it to revise the test-design language with his postulate of a disturbing planet. Einstein viewed Newton's celestial mechanics to be hypothetical, because he believed that the theory statements were more likely to be productively revised than the test-design statements, and he took the deviant orbital observations of Mercury to be falsifying, thus indicating that revision was needed. Empirical tests are conclusive decision procedures only for scientists who agree on which language is proposed theory and which is presumed test design, and who furthermore accept both the test design and the test-execution outcomes produced with the accepted test design.

Semantical consequences

Feyerabend recognizes that there are semantical consequences to counterinduction. In "Trivializing Knowledge", a paper critical of Popper, Feyerabend states that the "contents" of theories and experiments are constituted by the refutation performed and accepted by the scientific community, rather than functioning as the basis on which falsifiability can be decided, as Popper maintains. He considers the stock theory like "Every swan is white", and states that while a black swan falsifies the theory, the refutation depends on the reasons for the anomalous swan's black color. Earlier in his "Popper's Objective Knowledge" he gives the same example, and says that the decision about the significance of the anomalously black swan depends on having a theory of color production in animals.

But his discussion by means of this stock theory pertains more to the factors that motivate a scientific community to decide between test-design and theory statements, than to a description of the semantics resulting from that decision once made. Feyerabend has no metatheory of semantical description for characterizing the "contents" of theories and experiments. In this respect Feyerabend's philosophy suffers the same deficiency as Popper's.

Achievements

KUHN AND FEYERABEND

The conflicts between Popper and Feyerabend were struggles between giants in the philosophy of science profession. Having started in the theatre before turning to philosophy, Feyerabend chose a theatrical writing style that offended the droll scholars of the profession who tended to treat him dismissively. Judging by the typical fare to be found even today in the philosophy journals with their lingering residual positivism, he stands above the academic crowd by an order of magnitude. Feyerabend was an outstanding twentieth-century philosopher of science, who advanced the frontier of the discipline, as it was turning from an encrusted positivism to the new contemporary pragmatism.

