THOMAS KUHN ON REVOLUTION AND PAUL FEYERABEND ON ANARCHY

The classical Pragmatists recognized a philosophical significance of the phenomenon of belief. But belief has taken on a much greater importance in contemporary Pragmatism, where a descriptive discourse believed to be true (what Quine calls the "web of belief") constitutes a context that controls the semantics of the descriptive terms in the discourse. This is the contextual or artifactual thesis of the semantics of language. 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 J. 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 James 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 continuous.

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 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 can exist 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 role 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 itself 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 positive 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 the semantics led him to his "skeptical approach", just as it 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 formative intellectual experience, which has calls his “Aristotle experience” and which he says is responsible for much that is distinctive and original in his thinking.

Kuhn’s “Aristotle Experience”

The twentieth-century philosophers of science who have made influential contributions were inspired by their reflections on the spectacular developments in twentieth-century physics, notably relativity theory and quantum theory. Kuhn reports that his intellectually formative experience, however, was inspired by his reading Aristotle's *Physics*, and he calls this 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 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 rather than Aristotle's, because Aristotle's words had not meant to Aristotle and his contemporaries what they mean today to Kuhn and his 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 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 inverts the historical order; his account of scientific revolution describes what Aristotelian natural philosophers required to reach Newtonian ideas instead of what he, a Newtonian reading Aristotle's text, required to reach those of the Aristotelian natural philosophers. Thus he maintains that experiences like his Aristotle experience, in which the pieces suddenly sort themselves out and coming 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" may be characterized as 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 *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 the Copernican revolution, and seldom come from Aristotle's texts.

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, 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 referred to as planets and the earth is referred to as a planet like Mars and Jupiter, thereby making the two theories not just incompatible, but what he calls “incommensurable”. Nonetheless, as he develops his semantical views over the years, he maintains that astronomers holding either theory can 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 belief". That problem is 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 belief" 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 role 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, which 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 that are 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 a *succes de scandale* in the academic philosophy community. 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” chicken tracks that retarded the examination of the same modern sciences by the Logical Positivists and their like-minded pedantics. 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. Although many tenants of his 1962 book were previously published by Kuhn in his "The Essential Tension" in 1959, later reprinted in a book of the same name in 1977, the 1962 book was probably the most popular book pertaining to philosophy and history of science published in the 1960's and 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 circle of professional 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 an 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 explanation 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 belief", the practice of ignoring anomalies.

The thesis of the book offers a coherent description of the historical development in 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 called the "problem of scientific belief", reappears as "paradigm consensus" in his 1962 book, where it assumes a positive function without the ambivalence that it formerly had in Kuhn's 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 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 a "puzzle-solving" type of research 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 belief" is no longer problematic; the belief status of the paradigm is explained by its institutional status. This status effectively makes it what Conant 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. It is therefore 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 them. 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 no competing schools and 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, which the paradigm seems 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 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 occur as the extension of the paradigm proceeds over a period of time. In fact the paradigm is the source of the concepts needed for recognizing the new fact and for giving it its 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. Scientists are not easily

distracted by anomalies 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 new 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 an existing 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, that between competing paradigms 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 communication or reconciliation.

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 condensed vapor track in the cloud chamber and the *gestalt* which is 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 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 deciding 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 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 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* 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 and said 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 Shapere complained, Kuhn made no analysis of meaning to explain meaning change. Then in his third phase Kuhn 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 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 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 theory of scientific criticism and 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 have had a 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. On the other hand Kuhn's philosophy was principally influenced by earlier episodes in his "Aristotle experience" and in 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 test, the 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, that 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, and that both reject the Positivist idea 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, and 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's 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 goes on 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 were 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 a 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 stereotype theory that says "all swans are 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 do scientists actually make the choice between competing theories, and how scientific progress is to be understood. He says that the type of answer to these questions must in the final analysis by 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"

Popper criticizes the aim of normal science as viewed by Kuhn, and he rejects the historical relativism that 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 giving in 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.

But Popper is principally concerned with 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 it is a mistaken one. He says that Kuhn's view 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 break out of 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 discussion 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 a historical, sociological, or psychological argument, but is a logical one, and it is a mistaken one. Popper says 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 increased the 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 he 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 the most 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, and that 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 this 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 or not it is Kuhn's intention to provide a 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 he 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 explanations 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. 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?

Thirdly Feyerabend switches from a purely methodological perspective to a historical one, and replies to his own rhetorical question about theory proliferation vs. normal science consensus. Using his 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 extra-ordinary 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 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 Positivist 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, they 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. Firstly 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. Secondly different paradigms make researchers see things differently, such that researchers in different paradigms not only have different concepts, but also have different perceptions. Thirdly paradigms have different methods including intellectual as well as physical instruments for practicing research and evaluation results. He 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.

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 he 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 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 his view succeeds in providing any extratheoretical basis for comparing and for judging theories and paradigms. The result he says is a 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. They must using probabilistic theory decide upon some probability threshold below which statistical evidence will be held to be inconsistent with theory. 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 says that he agrees with Popper in rejecting any role 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 their limited success, and which suggests means for their improvement in both scope and precision.

(4) Finally the improvement in predictive technique must be a challenging task demanding high talent and dedication.

Fourthly 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.

Fifthly 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. Firstly he notes that 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 to enable such a comparison. He states that Popper's basic statements function as if they have this neutral character. 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 or conditions of applicability in subtle ways. But Kuhn states that to him "incommensurable" does not mean "incomparable", and in this respect he departs from Feyerabend's incommensurability thesis. In his 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 that translation is difficult is given in terms of his concept of paradigm. The paradigm is pre-articulate knowledge that functions as an example, which 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 that were 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. And it may be noted that Feyerabend does not consider the transition to the Copernican celestial theory to be a case of semantic incommensurability.

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. Kuhn 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 that he can escape his critics' claims that his views of theory choice are irrational and that he is a 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 a debate over theory choice neither party has access to an argument, which 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 based. While Kuhn faced a veritable fusillade from philosophers of science, he was received with unrestrained enthusiasm 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. And American academic sociologists have longed to demonstrate the manifest scientific progress that the more mature scientific professions have often exhibited in their histories. Consequently like Monsieur Jordain, the American sociologists were delightedly surprised when Kuhn 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 he 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" 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 backward American academic sociologists. And remarkably 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 irony: notwithstanding the mediocrity of their own science's accomplishments, sociologists deluded themselves into believing that they are the world's experts in the philosophy and practices of basic scientific research.

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. 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. He 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. Kuhn and Hagstrom are a mutual admiration society unto themselves. Hagstrom acknowledges Kuhn's influence in his preface, and he references and quotes 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. A short time after Kuhn's 1962 book there appeared a new sociological journal, *Sociological Methods and Research.* In a statement of policy reprinted in every issue for many years the editors state that the journal is devoted to sociology as a "cumulative" empirical science, and they describe 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. And in "Editorial Policies and Practices among Leading Journals in Four Scientific Fields" in the *Sociological Quarterly* (1978) Janice M. Beyer reports 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 either 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 options 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 uniformity that is indifferent to the distinction between contrary evidence and the contrary opinions of author and referees.

Ironically the outcome of the self-conscious attempt to make sociology a "mature" science practicing normal science with a 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. What actually happens in sociology, however, is that the 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. The philosophy of science that the sociologists enforce upon their membership is the Romanticist 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 peculiar variation on functionalism. Even though his functionalist sociology is defunct, Parson's Romantic philosophy of science 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, when they made Romanticism their consensus-paradigm philosophy of science. 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 ontology described by its predecessor, and attempted to cope with the anything-but-intuitive or commonsense semantical interpretation and consequent ontology of the radically new theory. Furthermore in the twentieth century this practice had become sufficiently routine that they were able to recognize and articulate their reactions.

It took the philosophers of science, however, nearly fifty years to capture the practice by developing the new systematic philosophy of language, which defines the contemporary Pragmatist philosophy. The contemporary Pragmatist philosophy of science differs from both Positivism and Romanticism in a very fundamental way, because both of these latter include ontological considerations in their criteria for scientific criticism. They differ between one another only about which types of ontology they will accept: the Positivists reject all "mentalism" in social and behavioral science, while the Romantics require it. The contemporary Pragmatists on the other hand subordinate all ontological questions and 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 ontologies to the outcomes of empirical criticisms is that ontologies change as science develops. Ironically the philosophy of science that the contemporary sociologists impose upon their membership is not only anachronistic but also quite at variance with the philosophy which 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 explanation, whereby empathetic plausibility 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 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, 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 sense" according to the intuition of the particular critic. This 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 quantum theory, 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, because the American sociological profession accepts and enforces consensus about Weber's strong version of the Romantic philosophy of science, and consequently they 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 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 that there existed 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 thesis of the 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 Parsonian 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 Parsonian functionalist sociology. Contemporary academic sociologists not only believe that social conformity to a consensus paradigm in the scientific community functions to produce social order, with 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 an ostensibly progress-producing consensus paradigm. The outcome of this combination of Parsonian 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 deserving the epitaph "American Gothic" sociology.

As it happens, American Gothic sociology seems to have become the appalling specter 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. Therefore sociology's decline is not merely a demographic phenomenon circumstantial to the history of the profession; it is the consequence of a pathological condition intrinsic to the American sociological profession's institutional values, normative standards, and research practices.

Kuhn's Linguistic Analysis of Incommensurability

Philosophers of science such as Feyerabend typically start with linguistic analysis. But Kuhn firstly wrote his interpretative description in history of science, and only after many years did he attempt any language analysis to explain and defend his thesis of semantic incommensurability. In the years following *Structure of Scientific Revolutions* this thesis evolved considerably, but he never repudiated it, because it is the corner stone for his philosophy of science, without which his metatheory collapses. Or better, it might be called the keystone of his architectonic, because it 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-science dichotomy would differ only in degree, thus causing his distinctive thesis of scientific revolution crumble.

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" (1993) he states that his efforts to understand and refine his incommensurability thesis has 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' language analyses, and the knowledgeable reader of *Road Since Structure* will find himself struggling through Kuhn's lengthy, laborious, and loquacious re-inventions of his incommensurability thesis, much as Kuhn 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 a later 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 the magnitudes have no common integer divisor except the number one. In mathematics "incommensurability" means there is no common measure, and for 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" (1983) he noted that his view of revolutionary change has increasingly moderated. 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 maintains that incommensurability is *all* language [italics in editors' the 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 very 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" (1989) 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 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" (1979) 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 ask some else who can 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 the 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. Many philosophers maintain that reference is not possible without characterizing concepts.

Later he further elaborates his theory of referential determination in his "Commensurability, Comparability, and Communicability" (1983) 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 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" (1991) 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. This neutral language is not the same as the Positivist observation language, and Kuhn 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 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 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 Kuhn's terminology means "learned." The interpreter need know only one language and he confronts another language as unintelligible noises and inscriptions. Quine's radical translator 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 are 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 intensions and rejects nominalism and its purely referential theory of meaning. Existence is neither the same as nor a condition for meaningfulness, and Kuhn says that he joins Hesse in maintaining that an 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. 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 use 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, thereby restructuring the 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" (1991) 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, and 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 what 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. For example "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

Of the four basic questions in philosophy of science (the aim of science, scientific discovery, scientific criticism, and scientific explanation) the most radical aspect of Kuhn's philosophy is his views on the aim of science and specifically his thesis of the uncritical character of "normal" science and its views on scientific criticism. Though a historian of science, Kuhn had written his *Structure of Scientific Revolutions* for philosophers of science, and he was disappointed to find that they did not receive it sympathetically. In response to criticism by philosophers he modified and evolved his philosophy several times over succeeding decades.

His thesis is twofold: Firstly in the normal science phase the consensus paradigm or theory assumes institutional status, and that therefore scientists' conformity to the consensus view becomes the criterion for scientific criticism. The conventionally recognized criteria for empirical criticism are subordinate to this institutionalized criterion of conformity to the prevailing paradigm, and "scientific progress" is understood in these terms.

Secondly in the revolutionary science phase, which is incidental to the conscious aim of science, semantic incommensurability between old and new successive theories makes the revolutionary transition such that the conventional criteria for theory choice cannot apply. In response to critics' questions about the possibility 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.

Then in response to philosophers' demand that he supply a linguistic analysis explaining his incommensurability thesis, he evolved his position substantially over the thirty years following *Structure of Scientific Revolutions*. The result was his 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 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 Logical Positivist philosophy and specifically of the 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 in chapter eleven of his *Structure of Science* (1961). Hempel and Oppenheim emphasize the logical-deductive nature of scientific explanation, 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 view of semantical change is not the same as Feyerabend's.

Initially the Logical Positivist interest in reduction was part of the 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. 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. 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 by 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. 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 references Bridgman's idea of "operationalist definitions", 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 equivocations, 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* 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, thus 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 room for meaning variance in the theoretical meanings, and yet there is also room for 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, 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 concerning 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 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 are 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 deductively. 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, the condition of 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 and their associated "coordinating definitions" in the primary science. 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, 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 meanings. 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. His pragmatic theory of observation gives Feyerabend an alternative to any reductionist thesis such as Nagel's. He 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 something 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 is 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, that separates observability and meaning, is what Feyerabend calls the "contextual theory of meaning". This theory of meaning description implies a wholistic approach in his view, 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 reasons. 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 Aristotle's theory of motion to Newton's theory. In this case Newton's theory offers the same quantitative measurements as Aristotle's, and 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 Newton's approach the procedures and assumptions of the theory 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 Newton's theory, because the usage involves laws that are inconsistent with Newtonian physics. Thus contrary to Nagel, the concept of impetus is not 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 the claim that the terms in the statements that have been derived from the kinetic theory with the help of correlating hypotheses will have 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 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'** will be 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 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" (1965), 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 elaborates 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 titled *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 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, and it is governed by what Whorf calls "an invisible central exchange". This central exchange of linkage bonds is what gives rise to the covert categories, or "cryptotypes" as he also calls them, that are submerged, subtle and elusive meanings corresponding to no actual word, yet which have a functionally important role 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 expression is the thesis of linguistic relativity, the thesis that language structure controls thought. He 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 was more prescient than he probably knew. 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 resolve the issue of whether the electron's wave and particle aspects are instantiated as two aspects of one and the same entity, as the Copenhagen advocates maintain, 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. Thus Hanson's observation that the mathematical expressions of the wave mechanics and the matrix mechanics can be transformed into one another does not support his thesis that such transformability implies the correctness of the Copenhagen interpretation. And Bohm is correct in maintaining that the wave-particle issue occurs in what he calls the “informal” language and not in the formalism. It is ironic that Feyerabend did not exploit Whorf’s insights during the years that Feyerabend was supporting Bohm’s hidden-variables interpretation in opposition to Hanson’ defense of the Copenhagen duality thesis.

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 the 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 their own rationality, and must allow "reason" which 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 involves 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. He maintains 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. And 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 some subjective basis, such as the scientist's metaphysical prejudices, his religious convictions, or his personal judgments of taste.

Feyerabend on Scientific Anarchy

In *Science and a Free Society* Feyerabend says in a section containing some autobiographical notes, that von Weizsacker has "prime responsibility" for Feyerabend's change to his anarchistic view. They met in Hamburg in 1965 and discussed the foundations of quantum theory. Feyerabend complained that alternatives to quantum theory had been omitted, 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 science. The only rule that he admits is that "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 the theory to make it fit 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 by Philipp Frank. 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 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 that 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 a "detection device" in Feyerabend's words. This procedure consists firstly of affirming the truth of the 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 they fail to notice motion that common to such objects as falling stones and the earth, and he also used the *ad hoc* hypothesis 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 fact is that resulting from his reinterpretation of observed experience, such as the falling stone. Galileo’s principle of the relativity of motion changed the conceptual component in observed fact. Another such 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 Logical 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 world view. 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 any 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 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 Copenhagen interpretation of the quantum theory. Initially he was critical of the Copenhagen interpretation, and particularly 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. Instead Feyerabend defended the possibility of an altogether new microphysical theory. In the 1960's Feyerabend became involved in a long debate with Russell Hanson. As a result he reconsidered the merits of the current quantum theory, and the 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-colloquial 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 things appearing on the other hand. In Feyerabend's view this distinction is fundamental to realism. On Thesis I the things appearing are those that are referred to 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.

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 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 this concept of realism from Popper. In "Complementarity" (1958) he references Popper's "The Aim of Science" published in *Ratio* (1957), and says it as 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's and Galileo's laws. But Popper rejects a reductionist relation between Newton's 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 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's 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 exact 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 philosophers 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, and Feyerabend therefore describes Bohr as a 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, even though no such derivation has actually been accomplished to date.

But this first approach does not seem to be 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 the observed fact of 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* (ed. Colodny) 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 "creed", and that the objections of a few opponents such as Einstein and Schroedinger 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. The 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 smaller and presently experimentally inaccessible (therefore hidden) order of magnitude than the quantum domain that is described by modern quantum theory.

In "Professor Bohm's Philosophy of Nature", a review of Bohm's 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 role for speculation in modern empirical physics. In a discussion of the role 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 role 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, a professional 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 (ed. Cohen and Wartofsky, 1967) 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 role of semantics of observation and of theory language in the discovery process. The evolution 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 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 experimental 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* (ed. Edwards, 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 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 classical 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 states 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 he 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 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 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 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 idea 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" (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 of 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.

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. He 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.

Feyerabend then states that the choice of metaphysical traditions is a choice of "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 role 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. What has failed is not realism, but rationalism with its universalist criterion for realism. Feyerabend 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".

Feyerabend's 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 says 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 a 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 Role 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 a 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 by 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 Role 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 implications is that scientific statements describing the subject matter are supposed to be expression 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. Scientific realism accepts 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. Feyerabend 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 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 a 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. Presumably this would include the criticisms by Shapere.

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 rejects 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 criticism of Popper's propensity interpretation is similar. 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 that 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 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, 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 "all ravens are 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 has discussed at length in *Against Method* and earlier papers. Incommensurability is not only the principal basis for his historical relativism, which Popper opposes, but it 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 theories are surrounded by an ocean of anomalies, 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 basic topics that may be considered in philosophy of science (aim of science, scientific explanation, scientific criticism, and scientific discovery) the place to begin an overview of Feyerabend's philosophy of science is with the topic of scientific criticism.

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 progress in science. While Kuhn and Feyerabend on the other hand choose to examine the practices of criticism and the conditions for progress, where test design statements are being revised, such that tests are invalidated. 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 role of his own prejudices in his preference for a 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 belief", 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 sociological controls, 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 or his tenaciously held view.

The process by which the individual scientist's tenacity is strategically functional is counterinduction. Its strategic 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 occurs 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 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 is, 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 role 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 inter-revolutionary 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, but instead 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 reference Marx's call for "revolution in permanence", but their meanings are diametrically opposed. Popper means perpetual conjectures and refutations occurring within an enduring institutionalized logical framework for conclusive refutation, while Feyerabend means perpetual institutional change with no controlling tradition-independent framework.

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 of 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 sufficiently naive 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 reconsidered 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 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 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, 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 the attempt to argue from premises in the historical level to conclusions in the analytical level. It is clear, however, that given his distinction between the theoretical and historical traditions and the way he relates them, Feyerabend would not admit to Hanson's "genetic fallacy" thesis.

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 new 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 admonition 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 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. Mach offered an analysis of the 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. Therefore, 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

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 criticism and development.

There certainly exists what may be called a conceptual scheme, but it is beliefs that bond and structure. And what they bond and structure are the components of complex meanings for association with the sign vehicle or individual term. 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 more informal experience occasions a reconsideration of one or several beliefs, the falsifying test outcome or experience can always be expressed with the existing vocabulary with its conventional semantics by articulating the contradiction to the theory's prediction. The empirically based contradiction makes the bonds and structures disintegrate, but formation of a new semantical re-integration due to a revision of beliefs by formation of new hypotheses is constrained psychologically only by the mundane fact of language habit. This is not to trivialize scientific discovery; 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 more extensive rejection of current beliefs, the more demanding the task.

Two reasons for incommensurability can be distinguished in Kuhn's literary corpus. The first is due to semantics that is unavailable in the language of an earlier theory that is not available 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 descriptive terminology 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, or other observational instruments not available at the time the earlier theory was formulated, but which are recognized and expressed in the language of a later theory.

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

Kuhn's second reason for incommensurability, lexicon restructuring, does not occasion incommensurability in the sense of inexpressibility; 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 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 have no semantic incommensurability.

In his "Commensurability, Comparability and Communicability", Kuhn says that if scientists moving forward in time experience revolutions, their shifts in gestalt switch will ordinarily be smaller than the historian's, because what the later experience as a single revolutionary change will usually have been spread over a number of such changes during the development of the science. And he 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 these 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, 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 disintegrated concepts 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 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 viewing a new microbe under a microscope and for which he yet has no classification. Then later the product of “phenomenal-seeing” description may be associated with a new “kind word”, which functions as a label or classification for the new phenomenon, and the new kind term may then later acquire still more semantics by incorporation into a theory. 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 terms based on them) introduced into the current language may be accommodated by the relevant currently accepted theory by the extension of that theory, or their introduction may subsequently occasion a modification of the current theory by elaborating it into a new and slightly different theory. And new semantic values may eventually lead to revolutionary revisions of current theory. But they do not constitute them.

Turn next to the philosophy of Feyerabend, which is more elaborate than Kuhn’s. Feyerabend’s began with an agenda for modern microphysics: an attempt 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 use of "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 logical quantification of general statements. Bohr's insistence upon the use of classical concepts for observational description in quantum theory experiments makes quantum theory inconsistent with both these meanings of "universality". Thus Feyerabend's later 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.

Quite apart from his acceptance of Bohr's use of classical concepts, 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. 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 that of the preceding theory, Aristotle's doctrine of the four causes which explained qualitative change as well as mechanical motion.

But Feyerabend also 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 classical concepts, 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 the Logical 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. What is needed in order to see how universal logical quantification is consistent with duality without complementarity, is a semantical metatheory of meaning description which enables an analysis of the semantical composition of the meanings of the descriptive terms. Consider such an analysis, which may serve as 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 may be viewed as both analytic and synthetic, or as what Quine calls an “analytical hypothesis”. The theories 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 statements have explicit quantifiers.

Imagine a list of statements that are universally quantified affirmations having the same subject term and believed to be true. The concepts associated with the descriptive terms predicated of the common subject by the several statements 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 parts consisting of the predicating concepts, and the meaning need not be viewed wholistically. Consider in turn the relations that may obtain among the concepts that are universally predicated in the 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 subject term common to 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 may be related to one another by universally quantified affirmations, and the predicate concepts are different 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 vague, but vagueness may be reduced in discrete increments by adding or excluding semantic values. Adding universal statements to the list 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 are the added semantic values. Adding universal negations relating concepts predicated of the common subject clarifies the meaning of the subject term by showing equivocation and excluding semantic values. And adding 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 makes a deductive system.

Now consider science: In all scientific experiments including microphysical experiments, the relevant descriptive language is dichotomously divided into a set of universal statements that are presumed for testing and another set of universal statements that are proposed for testing. The former is called test design statements and the latter are called theory statements. The distinction between them is pragmatic, because it depends upon the functions of the statements in testing. A given descriptive term occurring in a theory may be viewed as a subject term occurring in a list of universal affirmations with the list dichotomously divided into test design and theory statements. The subject term is thus common to the test design statements and to the theory statements. The dual analytic-synthetic nature of the statements makes the common subject term have part of its semantics supplied by the descriptive terms predicated of it by the test design statements, which are presumed true for the test for all who accept the test design. And this part of the term's semantics remains unchanged through the test, so long as the distinction between theory and test design statements remains unchanged.

For each descriptive term common to the test design and the theory, the part of the term's semantics supplied by the theory statements changes; it is different before and after the test. Before the execution of a test of a theory, all interested scientists who agree to the test design must also agree that the universal statements describing the test design are true independently of the theory. Thus if the test outcome is an inconsistency between the characterization supplied by the test design statements and the characterization made by theory's prediction statements, then it is the theory that is to be viewed as falsified. This independence of test design 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 that addresses the same problem. Therefore for the cognizant scientific profession the semantical parts defined by the test design statements before test execution make the terms vague with respect to the theory's claims. This amounts to saying that the theory does not define any part of the semantics of its constituent terms. However it may happen that the originating proposer and the supporting advocates of the theory may have such high confidence in their theory, that for them the theory may have come to supply part of the semantics for its constituent terms even before the test.

After the test is executed in accordance with its test design, the test design statements and the theory statements are either consistent or inconsistent with one another (after discounting for measurement error not attributable to failure to execute the test in accordance with the agreed test design). Therefore they either characterize the same observed instances or they do not. If the test outcome is an inconsistency between the test design statements and the proposed theory, then the theory is falsified. And since the theory is therefore no longer believed to be true, it cannot contribute to the semantics 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 theory and test design statements, then for each common term the semantics contributed by the two sets of statements are parts of one meaning complex of the univocal descriptive term, and they identify the same instances.

Furthermore, the additional characterization supplied by the semantics of the tested and nonfalsified theory statements resolves the vagueness that the meaning of the descriptive term had before the test for those who did not share the confidence had by the theory's proposers and advocates. However, the original proposers and the supporting advocates of the theory have options if the test outcome was a falsification. They may choose to reverse the status of the test design statements and theory statements, such that the theory assumes the role of defining the subject of the test, and the test design is rejected as an adequate or appropriate description of the phenomenon under investigation. This prejudice or tenacity amounts to what Feyerabend calls counterinduction.

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 are not necessarily resolved in relation to one another merely by the nonfalsifying test outcome. Any resolution of the vagueness in these predicate concepts requires that additional universal statements furthermore relate them to one another. Such would be the case were the statements formerly used as independent test design statements were revised, such that they could be incorporated into a deductive system and derived from the nonfalsified theory after the test. The resulting deductive system may make test design statements logical consequences of the theory, but with the theory tested and not falsified, this loss of independence of the test design statements is no longer important. 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 and described by the test design statements. Such a revision of test design language is possible in the case of relativity theory, but is not possible in the case of quantum theory.

In cases where description of the apparatus and physical procedures in terms of the laws derived from the theory is possible after the nonfalsifying test outcome, the original pretest description by the independent test design must result in what retrospectively may be called "errors". Furthermore for the test to have been valid, these errors must be very small relative to the physical effect that the apparatus is used to produce or detect in the nonfalsifying test of the theory. And the concepts associated with the descriptive terms in the original test design statement must be viewed as vague relative to the terms in the theory with respect to the more precise meanings they may later receive from the definitive role of the nonfalsified theory, after the test design statements are made derivable from the theory. This vagueness means that before the test the concepts associated with the vocabulary used in the test design statements assumed the semantical status that Heisenberg called the physicist's "everyday" concepts.

Feyerabend's Thesis I requires that the test design statements, 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. And since this derivative macrophysical description has never been achieved for the quantum theory, he later accepted Bohr's complementary thesis, which is the description of the microphysical phenomena with classical macrophysical concepts. As it happens, contrary to Bohr's instrumentalist thesis but consistent with Heisenberg's semantical views, the microphysical phenomena can be described with the variables in the mathematical expressions of the quantum theory and without classical concepts. But there is no quantum description of the functioning of the macrophysical apparatus by means of laws logically derived from the quantum theory. Thus at the conclusion of the first section of "Trivializing Knowledge" in *Farewell to Reason* Feyerabend says, that though Popper rejects reductionism, the variety of entities Popper admits to be real can be admitted as parts of the same world only if the theories that constitute them can be united in a way precluded by the incommensurability that Feyerabend finds in the relative knowledge in Bohr's complementarity thesis of quantum theory. He then concludes that science is not a theoretical tradition expressed as deductive systems, as he says Popper assumes, but rather is a historical tradition.

But contrary to Feyerabend, relativism is not the exclusive alternative to deductivism. The choice between classical and 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 semantics supplied by these test design statements may remain vague about this subject for an indefinite time after the test, just as they had to before the test was performed and while its outcome was not yet known. After the test the semantics supplied by the tested and nonfalsified quantum theory provides further resolution of the concepts associated with these terms common to both test design and theory statements. But the semantics supplied by the macrophysical descriptive terms in the test design statements may retain their vagueness indefinitely until a reductionist macrophysical quantum theory is developed, since the concepts associated with these terms are not unanalyzable wholes, but rather are complexes of semantic values.

If Feyerabend's Thesis I were modified such that after the nonfalsifying test outcome the theory as a set of universal analytic-synthetic statements defines only part of the semantics of its constituent descriptive terms, then a modified Thesis I becomes applicable to quantum theory. The application of the modified semantical principle implies that the test design-defined part of the meaning complex associated with the theory's descriptive term is not properly called "classical", because it makes no microphysical claims. Before the test it is vague with respect to any microphysics, and Heisenberg's term "everyday" is appropriate to describe the concepts associated with these terms. But after the test outcome is known 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 the 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, and those quantum concepts were resolved by the context supplied by his matrix mechanics.

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 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 in quantum theory had he ignored the reductionist program of Ludwig, developed a metatheory of semantical description, and appropriately modified Thesis I. With appropriate modification as described above, the application of Feyerabend's Thesis I to the quantum theory need not imply historical relativism, the rejection of the validity 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 his 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 pluralistic thesis.

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. Most often Feyerabend treats incommensurability as a relation between successively different 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 therefore are 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 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 initial proposal of these new theories.

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 kind of difficulty 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. 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, they both refrain from developing a metatheory of semantical description that would enable their readers to individuate theories and thereby to characterize 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 an obscurantist evasion in the absence of such a semantical metatheory.

The semantics of the Newtonian and relativity theories that Feyerabend says are incommensurable may be examined by considering their synthetic statements analytically. 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. They 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 displayed images of the stars unaffected by the gravitational effects of the sun. A later set displayed 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. But correction furthermore had to be made for distorting refraction due to atmospheric turbulence and heat gradients. The 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 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 – both Einsteinian relativistic and Newtonian classical physics – shares descriptive variables with the same set of test design statements. If the test design statements are viewed as analytic statements, then any descriptive variable occurring both in a test design statement and in either theory has a univocal semantics due to the semantic values contributed by the test design statements. This semantics is shared by both theories, and it makes the theories semantically commensurable.

Feyerabend maintained 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 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 transformation giving it the semantically defining status of an analytic statement, and the statements of the theory supply part of the semantics for each descriptive term common to the theory and the test design statements. This semantical contribution by the 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, and thus the terms may be said to have Einsteinian semantics. But the semantics supplied to these terms by their test design statements is still vague, just as before the test. However, if the test design statements are subsequently derived logically from the relativistic theory, then these common terms receive still more Einsteinian semantic values and additional structure from the accepted relativity theory. In this case everyday concepts may still describe the phenomenon, but the Einsteinian concepts are resolutions of the vagueness in the everyday concepts in the descriptive terms in the test design statements. In either case, 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 for the convinced advocates of the Newtonian theory before 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 role of the test design statements, or reconsider and reject the test design itself.

Nonetheless some physicists inaccurately refer to the concepts in the test design statements of relativity theory as Newtonian concepts. This is 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 the photographs, one used before the eclipse and another used during the eclipse. Since the resulting two sets of photographs were compared, a correction had to be made for differences in magnification. But no correction was even considered for the different deflections of starlight inside the telescopes due to the different gravitational effects of their different masses even by those who believed in the relativity theory, because such differential relativistic effects were not empirically detectable. But the nonmeasurability or undetectability does not imply that the test design statements affirm 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. Consequently the concepts in the test design statements describing the phenomena were vague about any relativistic effect introduced by the different masses of the two telescopes, and the concepts in the test design statements are too vague to be described as Newtonian or Einsteinian. This vagueness in the concepts in test design statements is indicated by a possible variation retrospectively called a measurement error, that is not due to failure to execute the test in conformity with the test design, and that is recognized only after the test outcome is accepted. There was such error in the Eddington experiment, but it was very small relative to the measured deflection of starlight by the sun's gravitational force through interstellar distances. This inaccuracy due to vagueness is relative to the other concepts in the test design statements, and it must be distinguished from the vagueness relative to the concepts in the theory. The meaning parts or semantic values defined by the test design statements are vague with respect to those defined by the theory statements, but this vagueness does not affect the measurement accuracy, since the condition of independence precludes the theory statements being used for measurement.

In addition to Bohr's complementarity thesis and his own incommensurability thesis, Feyerabend is led to his radical historicism by the view 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 it says that particular historical circumstances do not function to supply initial conditions for universal theories describing recurrent aspects of human social behavior, but rather preclude the validity of universals altogether. The persuasive objection to this historicism is that concepts are inherently universal (or as Popper says, all terms are disposition terms). The metatheory, which proposes using synthetic universal statements analytically for semantical description, which also enables exhibiting semantical continuity through scientific change through history, is a variation on this old but valid objection to this old philosophy of historicism. However, Feyerabend's historicism enjoys a novel plausibility that could not be admitted by philosophies from Platonism to Positivism, which advance a naturalistic philosophy of the semantics of terms. Platonic Ideas, Aristotelian forms and simple apprehensions, Romantic intuitions, and Positivistic phenomena, sensations, sense data, and operationalist definitions are all variations on the myth of "the given".

The scientific revolutions of the twentieth century have forced philosophers, and specifically Pragmatists, to affirm that meaning and belief are mutually conditioning, and in this sense are relativized to one another. But universal statements used to describe the real world condition this relativism. The real world is what imposes constraints on this mutual conditioning in language, that makes falsification possible, and that reveals the real world to us. Given any selected set of concepts, only some statements can be maintained; and conversely given any selected set of stated beliefs, only some concepts may be defined. The selection of truths is negotiable among interested scientists. But outside the narrow limits of measurement error and associated conceptual vagueness, truth conditioning expressed in universal statements linking initial conditions and test outcomes is not negotiable once test design statements are formulated and accepted. New experiences anomalous to our universal beliefs force revisions of those universal beliefs and therefore of their semantics. In empirical science the locus of the semantical revision is a proposed universal hypothesis – the theory – conditioned upon chosen universal test design statements. The empirical test is the window to new vision of reality.

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 radical relativism. Popper had 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", which is to say that the scientist should not redefine his problem by reconsidering any experiment's test design, especially 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. They are objectionable because they are *ad hoc* content-decreasing stratagems, merely evasions.

But the prejudiced or tenacious response of a scientist to an apparently falsifying test outcome does 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 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 in the Wilson cloud chamber experiment with the semantics of his indeterminacy relations 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 tracks are very large in comparison to the electron, and the produced effect admits easily to either interpretation. But the counterinduction strategy could also have been used by tenacious Newtonians who chose to reject the findings from Eddington's eclipse experiment. 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 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.

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

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 contemporary Pragmatism.

