INTRODUCTION
TO
PHILOSOPHY OF SCIENCE

The aim of philosophy of science is to understand what scientists did and how they did it, where history of science shows that they performed basic research very well. Therefore to achieve this aim, philosophers look back to the great achievements in the evolution of modern science that started with the Copernicus with greater emphasis given to more recent accomplishments.

The earliest philosophy of science in the last two hundred years is Romanticism, which started as a humanities discipline and was later adapted to science as a humanities specialty. The Romantics view the aim of science as interpretative understanding, which is a mentalistic ontology acquired by introspection. They call language containing this ontology "theory". The most successful science sharing in the humanities aim is economics, but since the development of econometrics that enables forecasting and policy, the humanities aim is mixed with the natural science aim of prediction and control. Often, however, econometricians have found that successful forecasting by econometric models must be purchased at the price of rejecting equation specifications based on the interpretative understanding supplied by neoclassical macroeconomic and microeconomic theory. In this context the term "economic theory" means precisely such neoclassical equation specifications. Aside from economics Romanticism has little relevance to the great accomplishments in the history of science, because its concept of the aim of science has severed it from the benefits of the examination of the history of science. The Romantic philosophy of social science is still resolutely practiced in immature sciences such as sociology, where mentalistic description prevails, where quantification and prediction are seldom attempted, and where implementation in social policy is seldom effective and often counterproductive.

Positivism followed Romanticism. Many Positivists were physicists, who took physics as the paradigm of the empirical sciences, and several wrote histories of physics. Positivism is practiced in behaviorist

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psychology, but has negligible representation in any of the social sciences. The term "theory" in the Positivist philosophy of science means language referring to entities or phenomena that are not directly observable. On this meaning the term includes the Romantic concept of "theory", which refers to the covert and introspectively acquired mental experience rejected by behaviorists. Theory is also defined in opposition to observation language, which serves as the logical reduction basis that enables theory language to be both empirically acceptable and semantically meaningful. Positivism originated as a reaction against Romanticism, and purported to be more adequate to the history of science, even if its reductionism agenda made it remote from the practice of basic research.

Pragmatism followed Positivism. The contemporary Pragmatism's ascendancy over Positivism was occasioned by philosophers' reflection on the modern quantum theory in microphysics. There have been numerous revolutionary developments in science, but none since Newton's mechanics has had an impact on philosophy of science comparable to the development of quantum theory. Its impact on philosophy has been even greater than Einstein's relativity theory, which occasioned Popper's effective critique of Positivism. Initially several of the essential insights of contemporary Pragmatism were articulated by one of the originators of the quantum theory, Heisenberg, who reinterpreted the observed tracks of the electron in the Wilson cloud chamber, and who also practiced scientific realism.

Many years later Heisenberg's ideas were taken up and further developed by academic philosophers in several leading American universities, and it is now the ascendant philosophy of science in the United States. Contemporary Pragmatism contains several new ideas. Firstly by introducing reciprocity between truth and meaning the Pragmatists philosophers, following the physicists Einstein and Heisenberg, dispensed with the naturalistic observation-theory semantics, thereby undercutting the observation-language reduction base essential to Positivism. Pragmatists substituted a relativistic semantics for the Positivists' naturalistic primitive observation semantics, thereby revising the meanings of "theory" and "observation", to recognize their functions in basic research science. Secondly by relativizing semantics, they also relativized ontology thereby removing it from the criteria for scientific criticism. The intended outcome of this development was recognition of the absolute priority of empirical criteria in scientific criticism, in order to account for physicists' acceptance of quantum theory with its distinctively counterintuitive ontology of duality. A related outcome was a new philosophy of science with which to reexamine retrospectively the previous great achievements in the history of

science. Feyerabend for example found that Galileo had revised his observation language when defending the Copernican heliocentric theory, something unthinkable to the Positivists.

The implications of ontological relativity are fundamentally devastating for both Romanticism and Positivism, both of which are defined in terms of prior ontological commitments. For the Pragmatist no ontology may function as a criterion for scientific criticism, because ontological commitment is consequent upon empirical testing, and is produced by a nonfalsifying test outcome that warrants belief in the tested theory. Neither "theory", "law" nor "explanation" are defined in terms of any prior ontology, semantics, or subject matter, but rather are defined in terms of their functioning in basic research: "theory" is any universally quantified statement proposed for empirical testing; "scientific law" is any empirically tested and currently nonfalsified theory; "explanation" is a deduction concluding to either a description of particular events or to another universal law statement. Thus the Pragmatist can accept but does not require the Romantic's mentalistic description, and he can accept but does not require the Positivist's nonmentalist description.

As the contemporary Pragmatism has been achieving its ascendancy, a new approach – computational philosophy of science – has emerged as a specialty in a new school of psychology called "cognitive psychology." Computational philosophy of science is less a new philosophy and more a new analytical technique enabled by the computer, and its appearance was not occasioned by a new revolutionary development in science; quantum theory is still the touchstone for contemporary philosophy of science. Cognitive psychology considers its subject to be conceptual representations, and there emerged a psychologistic turn, which was occasioned in part by rejection of the nominalist philosophy of language that some philosophers such as Quine have carried forward from Positivism into Pragmatism. But nominalism is not integral to Pragmatism; conceptualism is perfectly consistent with the contemporary Pragmatism. The computational approach is a new analytical technique occasioned by the emergence of computer technology compatible with the contemporary Pragmatism, much as the symbolic logic was once a new analytical technique compatible with Positivism and produced Logical Positivism. The computational analytical technique has already yielded many interesting re-examinations of past revolutionary episodes in the history of science. Its promise for the future – already realized in a few cases – is fruitful contributions to the advancement of contemporary science. A computational Pragmatist philosophy of science clearly seems destined to be the agenda for the twenty-first century.

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Organizational Overview

There are four basic topics in modern philosophy of science:

1 The institutionalized value system of modern science, also called the aim of science.
2 Scientific discovery, also known as new theory development.
3 Scientific criticism, especially the criteria used for the acceptance or rejection of theories.
4 Scientific explanation, the end product of basic science.

Theories, laws and explanations are linguistic artifacts. Therefore philosophy of language is integral to philosophy of science. There have been several philosophical approaches to language and to science in the twentieth century: Romanticism, Positivism, contemporary Pragmatism, and psychologistic computational philosophy of science. The last is more a technique than a philosophy.

The following discussion therefore begins with a brief overview of each of the philosophical approaches, and then proceeds to the examination of the elements of philosophy of language. Finally with this background the four topics are examined in the order listed above.

Romanticism

The earliest of these philosophies is Romanticism, which is still widely represented today in the social sciences including neoclassical economics and sociology. This philosophy had its origins in the German Idealist philosophies of Kant and Hegel, although the Idealist philosophies are of purely antiquarian interest to philosophers of science today. But contemporary Romantics carry forward the Idealist thesis that there is a fundamental distinction between sciences of nature and sciences of culture. According to the Romantics any valid and "causal" explanation of human behavior must describe the mental experiences – the views, values and motivations – of the human agents studied by social science. Access to these mental experiences requires introspection by the social science researcher, who if he does not share in the same culture as his subjects, at least shares in their humanity. The resulting interpretative understanding yields the "theoretical explanation" of observed behavior. Thus in the Romantic philosophy the semantics of the terms "theory" and "explanation" represent culture understood as shared mental experience, and these terms

mean something quite different from their meanings both in the natural sciences and in other philosophies of science.

The Romantics' philosophy of scientific discovery is based on introspection. Furthermore some Romantics advocate Max Weber's *verstehen* thesis of criticism, and require that explanations be validated by empathetic plausibility, so that they "make sense" in the scientist's vicarious imagination. When Romantics apply empirical criteria, it is often for survey research, where the survey responses are articulate expressions of the subject's mental state, often including his erroneous beliefs. The verbal survey responses are subject to the researcher's interpretative understanding. There may occur a conflict between the *verstehen* judgment and the empirical survey findings, and different Romantics will decide differently as to which to choose with some rejecting the empirical data out of hand. And when the empirical data are not survey data describing mental states, but instead are measurements of nonverbal behavior or demographics, then the absence of mentalistic descriptions supplying interpretative understanding will occasion the Romantics' rejection of valid empirical findings. Romanticism has its distinctive philosophical theses in philosophy of language and therefore in the four basic topics in philosophy of science.

Positivism

Positivism originated in the British Empiricist philosophers including notably David Hume, although these Empiricist philosophies are of largely antiquarian interest to philosophers of science today. The French philosopher Auguste Comte founded Positivism in the late nineteenth century. Apart from Behaviorist psychology there is only a residual representation of Positivism today in either science or philosophy of science. Positivists believe that all sciences share the same methodological concepts and philosophy of science, and their ideas are based on examination of the natural sciences. This view evolved into the Logical Positivist Unity of Science agenda. The Positivists are therefore very critical of the Romantics' introspective mentalistic view of theory and explanation in social science.

Positivism enjoyed its widest acceptance in physics during the apogee of Newtonian physics. Yet the Positivists were critical of Newton's theory, and their aim was to develop permanent foundations for Newtonian physics in observation by eliminating all of its theoretical components. Positivism later saw a revival after the First World War as Logical Positivism, which was advocated by a group of physicists and philosophers known as the

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"Vienna Circle." The Logical Positivists wished to imitate the physicists' use of mathematics in philosophy, and attempted to apply the Russellian symbolic logic to this end. They were also influenced by the success of Einstein's relativity theory in physics, which convinced them that physics is becoming more theoretical instead of less theoretical. Therefore they revised the original Positivist agenda from eliminating all theory to justifying theory accepted by contemporary physics. The justification was to be accomplished by using the Russellian symbolic logic to relate theoretical terms to observation language, an agenda known as logical reductionism.

Contemporary Pragmatism

In the middle of the twentieth century there emerged a new philosophy in the United States that was a reaction against Positivism. Called contemporary Pragmatism, it is currently the ascendant philosophy of science in academic philosophy in the United States as well as in many other countries. Pragmatism had an earlier representation in the classical Pragmatists - Pierce, James and Dewey - in the United States, but while some aspects of the classical Pragmatism have been carried forward into the new, the new contemporary Pragmatism is largely the product of philosophical examination of the quantum theory in microphysics developed in Europe the 1920's rather than a gloss on the classical Pragmatists. Physicists have offered several ontological interpretations of the modern quantum theory. Many have accepted one called the "Copenhagen interpretation." There are two versions of the Copenhagen interpretation, both of which assert the thesis of "duality", which says that the wave and particle properties of the electron are two aspects of the same entity, rather then separate entities that are always found together. One version called "complementarity" advanced by Bohr, says that the mathematical expressions of the theory must be viewed instrumentally instead of realistically, that only the ordinary language used for macrophysics can be used to express duality, and that the terms "wave" and "particle" are complementary because the semantics of the two terms make them mutually exclusive. The other version advanced by Heisenberg also contains the idea of duality, but says that the mathematical expression is realistic and descriptive, and does not need Bohr's complementarity. Basically the two versions differ in their philosophy of language. Heisenberg's philosophy of language was due to the influence of Einstein, and it has been incorporated

into the contemporary Pragmatist philosophy of language pioneered independently by Quine.

The Romantic and Positivist philosophies of science have been historically opposed to one another, but in comparison to the contemporary Pragmatist philosophy they are much more similar to one another than to the contemporary Pragmatism. The contemporary Pragmatist philosophy of science is distinguished by a new philosophy of language, which replaced the traditional naturalistic view of the semantics of descriptive terms with an artifactual view. The outcome of this new linguistic philosophy is that ontology, semantics, and truth are mutually determining unlike the simpler unidirectional relation found in earlier philosophies including classical Pragmatism. It thus revolutionized philosophy of science by relativizing the semantics and ontology of language and their relation truth.

While the contemporary Pragmatism emerged as a critique of Positivism, the Logical Positivists' emphasis on analysis of language and their nominalist referential theory of meaning have been carried forward into the contemporary Pragmatism, which continues in the Analytic tradition. The Analytic philosophers took the "linguistic turn" in philosophy, in search of the objectivity they believed lacking in both earlier Positivism and especially Romanticism. In their linguistic philosophy they adopted nominalism and rejected concepts, ideas, and all other mentalistic views of knowledge. Their adoption of nominalism was also motivated by their acceptance of the Russellian symbolic logic, in which ontological claims are indicated by the logical quantifier in the predicate calculus. The ontology expressed by the Russellian predicate calculus does not admit attributes or properties except by placing predicates in the range of logical quantifiers, thereby making them reference subsisting entities. Thus all predicates are either uninterpreted symbols or logically quantified terms referencing either mental or Platonic abstract "entities." Hence the Logical Positivists regard all philosophers as either Nominalists or Platonists. Some Pragmatist philosophers of science today continue to accept the Positivists' referential theory of the semantics of language, but this nominalism it is not essential to the contemporary Pragmatism.

Computational Philosophy of Science

Philosophers and scientists have long desired to have a "method" of routinizing scientific research, so that progress no longer depends on mysterious intuition or inexplicable genius. Francis Bacon (1561-1626)

thought he had such a method, an inductive method, which he set forth in his *Novum Organon*. John Stuart Mill (1801-1873) thought he also had such a method that he had set forth as his canons of induction in his *A System of Logic*. Neither was successful, but techniques have evolved considerably since their times. Recently and largely independently of academic philosophy of science, there has emerged a new approach in philosophy of science, which consists of developing computer systems for the creation of new scientific theories. These computer systems also apply criteria for selecting a subset of their developed theories for output as acceptable theories. This is a new technical approach that has replaced both the symbolic logic and the Logical Positivists' agenda. However, this technical approach has become a specialty in a new area of psychology known as "cognitive psychology", also known as "artificial intelligence." The originator of this approach is Herbert Simon, a Nobel laureate economist and a founder of artificial intelligence. A more recent name of the specialty is "computational philosophy of science" originated by Paul Thagard in his *Computational Philosophy of Science* (1988), which he defines as normative cognitive psychology.

This new technical agenda has ended up as a specialty in psychology, because the computational philosophers of science reject the residual Positivist nominalism in contemporary Pragmatism. The cognitive psychologists regard the subject of their investigations to be mental representations. Nominalism is not essential to the contemporary Pragmatism. But in other respects this cognitive-psychology approach may be viewed more as a technique than a philosophy. Before discussing the four topics in philosophy of science mentioned above, consider firstly the elements of philosophy language.

Synchronic Metalinguistic Analysis

Firstly some preliminaries: Philosophers of science divide language into two types: *object language* and *metalanguage*. Metalanguage is the discourse used to describe an object language, which in turn is the language used to describe some domain of the real world. The language of science is typically expressed in an object language, while the discourse of philosophy of science is typically in an appropriate metalanguage. Furthermore language may be viewed either *synchronically* or *diachronically*. The synchronic view is static, i.e. limited to a point in time like a photograph. The diachronic view exhibits change in a discourse or language over time.

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If the transitional process of change through time is described, then the diachronic view is also *dynamic*. Otherwise it is a *comparative static* view containing only "before" and "after" snapshots. Linguistic analysis offers four successive perspectives on language, which are increasingly inclusive: (1) *syntax*, (2) *semantics*, (3) *ontology*, and (4) *pragmatics*.

Syntax

Syntax is the minimally inclusive perspective, and its object is the most obvious part of language. *Syntax is the system of symbols in linguistic expressions considered in abstraction from the meanings associated with the symbols.* It is what remains after the removal of pragmatics, ontology, and semantics, and it consists of the forms of expression, so its perspective is said to be "formal." Since meanings are excluded from the syntactical perspective, the expressions are also said to be *semantically uninterpreted*. Syntax includes the physical sound symbols, but in science most of the language used is written, and written syntax consists of the visible ink marks on paper. Examples are the sentences of colloquial discourse, the formulas of pure or formal mathematics, the expressions of symbolic logic, and the instruction code in computer languages such as **FORTRAN**, **BASIC**, **C**, or **LISP**.

Syntactical Rules

Syntax is not quite as stark as some ancient inscriptions that are completely undecipherable to a field archeologist, because in addition to the uninterpreted inscriptions, there are rules that pertain to them. These are syntactical rules, and they are of two types: *formation rules and transformation rules*. Typically in the written languages of science the elementary symbols in the syntactical structure of an expression are organized serially and horizontally, and are often called "concatenated strings." However vertical or multidimensional positioning may also be significant in syntactical constructions, as in schematic diagrams or numbers arranged in matrices. Syntactical construction is governed by "*formation rules*", which are expressed in a metalanguage, since they are rules about language.

Formation rules enable construction of grammatical sentences or well-formed formulas from more elementary syntactical symbols. The native

speaker of a colloquial language can routinely produce grammatical sentences, but the linguist's task of formulating explicit formation rules for a natural language is more difficult. Linguists apply syntactical formation rules to small elements of language such as sound phonemes and the written alphabet. But for the analysis of scientific texts philosophers are content with such elements as words and terms. Artificial languages such as those of mathematics and computer systems are typically more regular, and their rules are less complex than those of colloquial discourse. Grammatically correct expressions in these artificial languages are conventionally called "well formed formulas." When there exists a comprehensive set of formation rules for a language, it becomes possible to develop a type of computer program called a "generative grammar", which can generate grammatically correct expressions or well formed formulas for a language. These computer programs input, process, and output object language, while the coded instructions constituting the computer program are statements in a metalanguage. When a computerized generative grammar is used to produce new scientific theories in an object language for an empirical science, the computer system is called a "*discovery system.*"

Transformation rules change well-formed formulas or grammatical sentences into other such formulas or sentences. For example there are transformation rules for colloquial language that change a declarative sentence into an interrogative sentence. But the discourse of science is expository, and philosophy of science therefore principally considers the declarative sentence in descriptive discourse. Furthermore transformation rules are of greater interest to logicians than to philosophers of science, who are more interested in formation rules for generative grammar discovery systems. Logical inferences are said to be made by transformation rules, but logic rules are intended not only to produce new grammatical sentences but also to guarantee truth transferability from one sentence to another.

Semantics

Semantics is consideration of the meanings associated with syntactical structures, and therefore includes the syntactical perspective. Language viewed in the semantical perspective is said to be a "*semantically interpreted.*" In comparison to syntax the topic of semantics has been more philosophically controversial, and it is in the area of semantics that philosophy of language and philosophy of science have exhibited the greatest amount of change in recent decades. There is now a post-Positivist

view, which has been developed most extensively to date in the contemporary Pragmatist philosophy. And it is also a post-Romanticist view. But for purposes of contrast consider firstly a stereotypically generic version of the traditional Positivist view of semantics.

Traditional Positivist Semantics

On the traditional Positivist view descriptive terms receive their semantics *ostensively* unless they are given their meanings *contextually* by explicit definitions. In the simple case of primitive terms such as "black" the child's ostensive acquisition of meaning was thought to consist of his pointing his finger at an instance of perceived blackness in some black thing such as a raven bird, and then hearing the word "black." A French or German word would presumably have served equally well. There have been various theories about what cognitive processes are involved in this supposedly primitive perception, but the outcome of the process was thought to be the acquisition of primitive sensations or sense data. Most notably the sensation thus acquired is thought to be identical for all persons. And the concept serves as an elementary and atomistic building block for the construction of larger units of language such as sentences. Then from the early experiences that "this raven is black" or "some ravens are black", the learner may acquire more extensive experience with ravens that may occasion the generalized belief that "all ravens are black."

What is fundamental to this traditional view is the naturalistic philosophy of the semantics of language, the thesis that the semantics of descriptive terms is determined by the nature of human perception or other cognitive processes and/or by the nature of the real world itself. Different languages are conventional in their vocabulary symbols and in their syntactical structures and rules, but on the naturalistic thesis nature determines that the semantics is the same for all persons who have had the same kinds of experiences that occasioned their having acquired their semantics by simple ostension. Furthermore the naturalistic semantics of a descriptive term is invariable through time and in different contexts. This *meaning invariance* is a property of terms thought to have only an ostensively acquired semantics.

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The Positivist Analytic–Synthetic Semantical Dichotomy

In addition to the descriptive terms that have primitive and simple semantics, the traditional view also recognized the existence of terms that have complex semantics. A type of sentence called a "definition" reveals the composition in a complex meaning. The defined term or *definiendum* has a compositional semantics that is exhibited by the defining terms or *definiens*. Terms having complex semantics also occur in sentences called "analytical" or just "analytic", while the terms having simple and primitive semantics occur in sentences called "synthetic", thus giving rise to the *analytic-synthetic* distinction. But this difference is not merely a distinction; it also alleges a dichotomous separation between the simple and complex types of descriptive terms. An example of an analytical sentence is "all bachelors are unmarried." The semantics of the term "bachelor" is compositional, because the idea of being unmarried is included as a part of the complex meaning of the idea of bachelorhood due to the definition of "bachelor", thus making the phrase "unmarried bachelor" redundant. A closely related claim traditionally made of the analytic sentence is that it is an *a priori* or self-evident truth, a truth known by reflection on the inclusive relation of the meanings of its constituent terms. Contemporary Pragmatists reject the thesis of *a priori* truth.

The Positivist Theory-Observation Semantical Dichotomy

Another example of compositional semantics is the Positivists' thesis of "*theoretical terms.*" Stock examples of theoretical terms found in the natural sciences are terms such as "neutrino" and "prion." The Positivists considered theoretical entities such as neutrinos and prions to be postulated entities as opposed to observed entities. They called terms that reference observed entities and that receive their semantics ostensively "*observation terms*", and they called the sentences containing only such terms "*observation sentences.*" They called terms that reference postulated entities and that therefore cannot receive their semantics ostensively "*theoretical terms.*" And they called sentences containing any such terms "*theory sentences*" or just "*theories.*" They also believe that theoretical terms are meaningless unless these terms receive their semantics from observation terms, because on the nominalists' referential philosophy of meaning, terms purporting nonexistent entities are meaningless. Therefore the Logical Positivists proposed a type of sentence which they called the "*reduction*

sentence", also called "correspondence rule" or "bridge principle", which purportedly enables theoretical terms to derive their semantics deductively from observation terms by the symbolic logic. Both the reduction sentence and the definition exhibit composition in the semantics of their descriptive terms. But while the definition determines the whole meaning of the defined term, the reduction sentence determines only part of the meaning of the theoretical term, because the theoretical term will receive additional meaning as the scientific theory containing it is further developed. The problem of reduction, however, is a problem that the Logical Positivists themselves finally agreed they could never solve, because they could not exclude meaningless theories from those accepted by scientists.

Contemporary Pragmatist Semantics

The development of the contemporary Pragmatist philosophy was occasioned by the development of the modern quantum theory in physics, and it contains a new philosophy of language with a new metatheory for semantics. The fundamental postulate in the contemporary Pragmatist philosophy of language is the rejection of the naturalistic thesis of the semantics of language and the development of an artifactual thesis that relativizes semantics. The rejection of the naturalistic thesis in philosophy of language is not new to linguistics, but it is as fundamentally opposed to the Positivist philosophy as the rejection of the parallel postulate is to Euclidian geometry. The artifactual thesis of the semantics of language is that semantics of any term is determined in its context of statements believed to be true for any reason. *Three notable consequences of the artifactual thesis are (1) the rejection of the Positivist observation-theory dichotomy, (2) the rejection of the Positivist thesis of meaning invariance for descriptive terms, and (3) the rejection of the Positivist analytic-synthetic dichotomy.*

Rejection of the Positivist Observation-Theory Dichotomy

More than thirty years after Heisenberg, one of the developers of the modern quantum theory, had said that he could "see" the electron in the Wilson cloud chamber, philosophers began to reconsider the concept of observation, an idea that had previously seemed obvious. Today on the Pragmatist view there are no observation terms that receive their meanings by simple ostension. Rather every descriptive term is embedded in a

connecting "web of beliefs", to use a phrase of Quine, which constitutes the context determining the term's meaning. A unilingual dictionary is a listing of a subset of these beliefs for each univocal lexical entry. It is necessary to know much about what the speaker believes about ravens even just to recognize it as a raven, much less perhaps also to view it as some kind of omen. Contrary to the Positivists, observation terms are not uncontaminated by theory context. Furthermore ostension cannot fully determine the semantics of the word "raven" even in its belief context. All descriptive terms have a residual vagueness that can never be completely eliminated, but can be reduced by the addition of clarifying context. The vagueness is a manifestation of the empirical underdetermination of language. *All descriptive language is empirically underdetermined by reality.*

Rejection of Positivist Meaning Invariance Thesis

One of the motivations for the Positivists' maintaining the observation-theory dichotomy is the belief that science offers a kind of knowledge that is permanently valid and true. In the Positivist philosophy it is observation that was presumed to deliver this certitude, while theory is subject to revision sometimes revolutionary in scope. When the observation-theory dichotomy is rejected, the foundation for this permanence crumbles, and the Positivists' observation language becomes subject to semantical change or *meaning variance*. A revolutionary change in theory, such as the replacement of Newton's theory of gravitation with Einstein's, has the effect of changing the semantics of all the language common to both the old and new theories including what the Positivists called observation language.

Rejection of the Positivist Analytic-Synthetic Dichotomy

On the traditional view analytic sentences are those the truth of which could be known *a priori*, i.e. by reflection on the meanings of the constituent descriptive terms, while synthetic sentences require empirical determination of their truth status, and can only be known *a posteriori*. Thus to know the truth status of the analytic sentence "All unmarried men are bachelors" it is unnecessary to take a survey of unmarried men to determine how many men are bachelors, because the meaning of bachelor is determined by the context constituting the definition of bachelor as an

unmarried man. But on the artifactual thesis of the semantics of language all descriptive terms are contextually determined, such that all declarative and universally quantified sentences may be called analytic. Yet their truth status is not thereby known *a priori*, because they are also synthetic. Therefore when any universally quantified declarative sentence is accepted as true, it can be used analytically for a partial analysis of its constituent descriptive subject term. Thus "All ravens are black" is as analytic as "All bachelors are unmarried men", so long as one believes that all ravens are black, because the meaning of "raven" include the idea of blackness, just as the meaning of "bachelor" includes the unmarried state. Normally in science the reason for belief is the empirical adequacy demonstrated by an empirical test such as an experiment. *All universally quantified statements believe to be true are both analytic and synthetic, and can be called "analytical hypotheses."*

Traditional Romanticist Semantics

On the Romanticist view the Positivist semantics is acceptable for the natural sciences, but it is deemed inadequate for research in the cultural sciences of human action. Human action has meaning for the human actors; it is purposeful and motivated for them. Therefore the semantics for the cultural sciences explaining human action is the subjective meaning that the action has for the actor. The researcher's access to and sharing of this meaning requires the aid of introspection, even if its acquisition also involves the actor's overt linguistically expressed reporting. The resulting meaning is called interpretative understanding. In the cultural sciences both the actor's utterances and all his other voluntary actions require interpretative understanding. When applied to linguistic tests, the acquisition of such human understanding is called hermaneutics. The validity of the sharing is based in their shared humanity, and where the researcher lives in the same society or group, it is also based in their shared culture.

Some Romantics deny that interpretative understanding can change. Von Mises, the Austrian economist, maintains that economics is a permanent, a priori, and purely deductive science, which he calls praexology, and which he says is developed entirely from introspectively and intuitively self-evident propositions. But this is a minority view. Many more cultural science researchers admit to cultural change and its constituent meaning change on the part of the actors. And since this meaning change

can happen in the actors, it can happen in the researchers also, since their practice of cultural science research is also human action. However, the cultural science researchers' examination of cultural change is simply comparative in the sense that it is not a componential semantical analysis.

Semantical Rules

Just as there are syntactical rules, so too there are semantical rules. In the contemporary Pragmatist philosophy of science the semantical rules describe the meaning of a descriptive term by exploiting the analytic-synthetic character of universally quantified statements believed to be true. If it is believed that all ravens are in fact black, then the statement "All ravens are black" is a semantical rule describing part of the meaning of the term "raven." The idea of blackness is a component part of the complex idea of raven, as is revealed by the redundancy in the phrase "black raven."

Semantical rules are statements in a metalanguage, since they are about language. The semantical rules can be expressed in the style of a Tarskian sentence using single quotation marks for object language and double quotation marks for metalanguage. Consider the traditional Tarskian formulation: "'All ravens are black', if and only if all ravens are black." This conditional sentence only expresses the truth condition for the universal affirmation. On the other hand a semantical rule in the Tarskian style would read: "The concept black is a component part of the concept raven, if and only if 'all ravens are black' is believed to be true." Like the universal affirmation, this statement analyzes the composition of the meaning of "raven."

Univocal and Equivocal Terms

The definitions in a unilingual dictionary are semantical rules. Usually each lexical entry in the unilingual dictionary offers several meanings for a descriptive term, because terms are routinely equivocal with several alternative meanings. Even the English language, which has a very large vocabulary, economizes on words by giving each word several different meanings, which are distinguished in context. There is always at least one semantical rule for each univocal use of a descriptive term. The descriptive term is univocal if none of the predicates in the several statements functioning as semantical rules can be related to one another by a

universally quantified negative statement. Thus if two semantical rules are "Every X is A" and "Every X is B", and if it is also believed that "No A is B", then the terms A and B are parts of different meanings for the term "X", and "X" is equivocal. Otherwise A and B would be different parts of the one meaning complex associated with the univocal term "X." Furthermore some of the structure of the meaning complex associated with the univocal term is revealed if the predicates in the statements can be related to one another in universally quantified affirmations, such that some of the statements in the list form a deductive system. Thus if the predicate terms "A" and "B" in "Every X is A" and "Every X is B" were related in the statement "Every A is B", then one of the statements in the list could be logically derived from another. Awareness of the deductive relationship and the consequent display of structure of the meaning complex associated with the term "X" makes the meaning of "X" more coherent. The dictionary meanings are only minimal descriptions of the meanings of univocal descriptive terms. Such terms may have many semantical rules, when many characteristics apply universally to a given subject term. Thus there are multiple predicates that universally characterize ravens, characteristics which are known to the ornithologist, and which may fill a page of his reference book about birds.

Relativized Semantics

As said above, all the statements believed to be true and predicating characteristics universally of ravens are semantical rules describing the complex meaning of "raven." But if a bird watcher captures a bird specimen that looks like a red raven, he must make a decision. He must decide whether he will continue to believe "All ravens are black" and that he holds in his birdcage a red nonraven bird, or he must decide not to continue to believe "All ravens are black" and that he holds a nonblack raven bird. In either case a semantical change must occur. Because semantics is relativized to a system of beliefs, it has an artifactual nature, which means that a decision is involved. Color could be made a criterion for species identification instead of the ability to interbreed, although many other beliefs would also then be affected in violation of Quine's principle of minimum mutilation of the web of beliefs.

The decision is also ontological. If the decision to reject the belief "All ravens are black" becomes conventional, then the phrase "red raven" becomes a literal description for a type of existing birds. Red ravens

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suddenly populate many trees in the world, however long ago nature had evolved red ravens. But if the decision is to continue to believe "All ravens are black", then there are no red ravens in existence. In that case the phrase "red raven" is a metaphor like "vulpine man", and the reader or listener is left to surmise from context and supply from imagination what the poet might have had in mind by his phrase "red raven." But if the reader-supplied metaphorical meaning later becomes conventional, much less trite, then the metaphor has become a dead metaphor, and "red" becomes at least in part equivocal with a new literal meaning, as with the two literal meanings for "running" in "running title" and "running turtle."

The bird watcher's scientific discovery requires that all the ornithological reference books be updated either to include a new species of red-colored bird or to exclude the characterization that all ravens are black. The availability of the choice is due to the artifactuality of the semantics of language and to the ontology the relativized semantics describes. As it happens, since color is not conventionally definitive of animal species, especially if the birds of different color can interbreed, the books will probably not announce a new species, but instead will note that red ravens have been observed. These semantical and ontological details may seem rather pedantic, if not quite bird-brained, but semantics and ontology have been controversial in science and philosophy. For example in 1905 Einstein's relativity theory changed the semantics of the familiar term "simultaneity" in a way that many of his cohorts in physics had found difficult to accept. And today economists still argue whether or not consumer credit card borrowing limits are money, a decision that is hugely consequential for a banker's legally required minimum reserve requirements. Our linguistic decisions alone neither create nor annihilate reality. But they do change our characterization of it into kinds according to the degree that the current state of our semantics discriminates the sometimes profuse and sometimes paltry manifold of attributes, whereby physical things manifest themselves to us.

Clear and Vague Meaning

Terms are univocal or equivocal; meanings are clear or vague. Clarity is increased for a descriptive term by the addition of universal statements to the list of statements believed to be true and containing it as a common subject term, and also by the addition of universal statements believed true and relating the predicates in the list. The universal statements may be

either affirmative or negative. Affirmative statements offer clarity by adding information and in some cases by exhibiting semantic structure. Negative statements offer clarity by contrast and by exhibiting equivocation. Vagueness remains to the extent that such clarification is lacking. Vagueness can never be eliminated completely, since it is the absence of information, but it is reduced by the addition of universal statements accepted as true. Inevitable vagueness is a manifestation of the empirical underdetermination of language.

Analysis of Semantical Change vs "Holism"

Semantical change was vexing to the contemporary Pragmatists, when they first accepted the artifactual thesis of the semantics of language. When they threw out *a priori* analytic truth they mistakenly also rejected analyticity. And when they accepted the contextual determination of meaning, they mistakenly took an indefinitely large context as the smallest unit of language that can be examined. This context was typically construed either as consisting of a whole explicit theory with no criteria for individuating theories, or even more vaguely as a "paradigm" consisting of a whole theory together with many associated pre-articulate beliefs and tacit skills. This is a wholistic (or "holistic") semantical thesis. On the wholistic view a new theory that succeeds an old theory that has been falsified by empirical testing must completely replace the old theory together with all its observational semantics and ontology. This view is typically associated with the historian of science Thomas Kuhn, who wrote a popular monograph titled *Structure of Scientific Revolutions* in 1962, and also with the philosopher of science, Paul Feyerabend. This wholism creates a problem for the decidability of empirical testing in science, because complete replacement deprives the two theories of any semantical continuity, such that they cannot describe the same phenomena or address the same problem. If a new theory must completely replace an old one, such that there can be no semantical continuity, how can the new theory be said to be an alternative to the old one, much less be a better one?

However, it is not necessary to accept the wholistic view of semantics, because rejection of the analytic-synthetic dichotomy and its *a priori* truth claim do not imply the rejection of analyticity. The contextual determination of meaning implies only that the dichotomy need be rejected, not analyticity as such. As discussed above, universally quantified empirical (i.e. synthetic) statements believed true for any reason are also analytic

statements used as semantical rules for semantical analysis. And the analysis consists of exhibiting the composition and structures of meanings by revealing their component parts. Therefore when a semantical change occurs due to a change in some of the beliefs in the context of a system of beliefs, some parts remain common to both the old and new meanings, while the semantical change consists in dropping some parts and in adding some new ones. The meaning parts that endure through the change from one theory to a later one are those occurring in the statements of empirical test design, which do not change. Furthermore since every predicate term has a semantical rule describing its complexity, the web of beliefs contains elementary components that may be called "semantic values." These semantic values are the smallest distinguished features of the real world that are recognized by the language at the current time. The introduction of new semantic values produces partial semantic incommensurability between old and new descriptive discourse, such that discourse after the introduction of the new semantic values cannot be fully commensurated with the old discourse about the same subject.

Semantical State Descriptions

A state description is a synchronic display consisting of a list of universally quantified statements containing both the currently nonfalsified theories addressing one problem and the test design statements that define the problem. The theories may be nonfalsified because they have not been tested. And the state description may be augmented with falsified theories for new theory development, so that it is a cumulative state description; old theories have scrap value consisting of language that may be recycled. The state description is a semantical description, because the universally quantified statements believed to be true at the given point in time, function as semantical rules exhibiting the component parts of the composite meanings associated with their common univocal descriptive subject terms. Furthermore a state description is for a scientific "profession", which consists of the persons who are attempting to solve the scientific problem. On this definition a profession is a much smaller group than the academicians in the field of the problem, while at the same time it is not restricted to academicians. A diachronic display consists of two state descriptions representing two chronologically successive states sharing a set of common descriptive terms. Both synchronic and diachronic displays are static analyses; the diachronic display enables a comparative static analysis.

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State descriptions are the beginning and ending points for a dynamic analysis, which describes the transition from one state to the next.

Scientific Realism

Academic philosophy has often been a comfortable and remunerative haven from reality. Even more than insane schizophrenics, inane academics need reality checks. In particular pedantic philosophers need be told that there is a real world existing independently of human cognition, and that it is the first object of human cognition. Realism is not a conclusion that can be proved logically either by science or in any other way. But all persons are experientially aware of reality from the awakening of consciousness. That awareness is a primordial *prejudice*. One is reminded of Bertrand Russell's "proof" for realism: after announcing his intent he simply raised his hands. Nothing spoken, but enough said. This awareness grows in sophistication with the acquisition of language including in due course the acquisition of the language of science. The advancement of science is the increasing adequacy of human knowledge of the real world. For the empirical scientist the consciousness of reality becomes astute when theory reveals reality, and acute when reality refutes theory. A falsifying test outcome is no time for Cartesian doubt that the first object of human knowledge is the recalcitrant real world. Such is the basis for scientific realism. *Scientific realism is the thesis that the most critically empirically tested and currently nonfalsified theory, i.e. a scientific law, in science is the most adequate available description of reality.*

Relativized Ontology

Ontology is the third of the metalinguistic perspectives after syntax and semantics. *Ontology pertains to the real world as linguistically characterized. In the context of science the characterizing language has meanings associated with the descriptive terms in empirically tested and nonfalsified universal statements believed true.* When scientific realism is joined with semantics relativized to universally quantified statements believed to be true, the result is the thesis that Quine calls "ontological relativity". Scientific realism pertains indiscriminately to all empirically warranted statements, but ontology is the distinctive characterization of reality claimed by the semantics of an individual statement. It may be added

that no realistic claim is made by what a particular scientific discourse does not describe. Silence is vagueness. As mentioned above, if one maintains the empirically warranted belief expressed in substantive language that all ravens are black, then both raven entities with their black attribute are real, and red ravens are not real. Historically philosophers and scientists believed that they knew very well just what is real however much they disagreed among themselves, and they brought their preconceptions to the criticism of scientific theories. This presumption led them to reject out of hand many new and empirically acceptable theories that did not conform to their ontological preconceptions. Eventually philosophers of science recognized that often the prevailing ontological preconceptions used by scientists to criticize new theories have been nothing more than ontologies described by previously accepted theories. Scientific realism lets the scientists do the ontologizing instead of the philosopher.

Relativized ontology is the thesis that each empirically tested and nonfalsified set of universally quantified statements believed to be true defines its own ontology. It may be added that this applies to the universally quantified language presumed true in order to conduct the empirical tests, because it is empirical language having definitional force. Ontological issues depend on prior decisions about semantical rules, which in turn enable characterization of evidence operative in empirical testing. Subordinating ontological claims to such universally quantified statements believed true due to their empirical warrant is an outcome of the relativistic semantics, because the relativized semantics produces relativized ontology. Quine called this "ontological relativity", although Quine imposed a nominalist ontology due to his acceptance of the Russellian predicate calculus notational conventions.

Relativized ontology effectively makes all referential terms theoretical terms, because it makes all entities posited entities. The referencing of an entity is by means of the descriptive semantics that is described by the universally quantified statements characterizing it and believed true. Thus the relativized semantics makes ontological commitment no less relative whether the postulated entity is an elephant, an electron, or an elf. Beliefs that enable us to make successful predictions routinely are deemed more empirically warranted than those not so warranted, and the entities, properties or any other manifestations of reality postulated in those successfully predicting beliefs are invested with greater ontological commitment than alternatives. It is to those manifestations that are most empirically consequential and about which we have the most characterizing information, to which we make our strongest ontological commitments. If

the postulate of elves enabled us to predict economic fluctuations more accurately and reliably than humans, then we would accept busy elves as real entities, and would busy ourselves about them, as we have done with elephants and electrons for other types of predictable consequences. And when we find our belief in elves to be empirically inconsequential, we reject the reality of elves, as we reject the reality of possessing demons once thought responsible for sickness.

As it happens, "demon" is not part of contemporary ontology, but it could have been otherwise. Just as the meaning of "atom" has evolved since the time of Democritus, the meaning of "demon" might too have evolved to become as beneficial as the modern meaning of "bacterium" – had empirical testing regulated its evolving semantics. Then today scientists might materialize (i.e. visualize) demons with microscopes, and physicians might write incantations (i.e. prescriptions), so pharmacists might dispense antidemonics (i.e. antibiotics) to exorcise them. But terms such as "materialize", "incantation" and "antidemonics" would have acquired a new semantics in more empirical contexts. As Quine observed in his "Two Dogmas" in 1952, we can preserve our belief in any statement positing anything, if we are willing to make sufficiently drastic redistribution of truth values elsewhere in our web of beliefs – the set of related beliefs that we use as semantical rules to describe our semantics and associated ontologies. And ontologies based on scientific realism are those for which beliefs are regulated by empirical science.

Causality

The ideas of cause and effect are ontological categories, because they are about the real world that exists independently of human cognition, which is not to say independent of human actions in the real world such as measuring. The causal relationship is expressed in the nontruth-functional conditional statement that makes a universal claim that is believed to be true. The causal dependency asserted to exist between what is described by the antecedent and consequent clauses is never proved or permanently established, but its tested and nonfalsified status warrants the belief in the assertion and thus in an ontological commitment. When in the progress of science the theory is falsified, it is made clear thereby that the universality of the claim is not valid, and that a more adequate characterization of the specific causal relation is needed, if it is retained at all.

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Pragmatics and Theory Language

Pragmatics is the fourth and the most inclusive of the metalinguistic perspectives. Pragmatics pertains to the language user's use of his language understood as semantically interpreted syntax and associated ontology. The controlling pragmatics of basic science is described in the statement of the aim of science: *to create explanations by the development and empirical testing of theories that are laws because they are not falsified when tested.* Explanations and laws are accomplished science; theories are work in process at the frontier of development.

Scientific theories are universally quantified semantically interpreted syntactical structures proposed for testing. This is the definition of theory language in the contemporary Pragmatist philosophy of science. It contains the traditional idea that theories are hypotheses, but the reason for their hypothetical status is not due to the Positivist observation-theory dichotomy. The Positivist observation-theory dichotomy is based on the semantical thesis that observation sentences have a naturalistic semantics acquired by observation, and that theory language has no semantics unless and until it is logically related to observation statements with reduction sentences. But when the observation-theory dichotomy falls, so too must the semantical basis for identifying theory language.

Today the contemporary Pragmatists have replaced the semantical basis for identifying theory language with a pragmatic one: theories are hypothetical because they are untested and are proposed for testing. Actually all universally quantified statements are hypothetical in the sense that they cannot be incorrigibly true and beyond revision. But theories are those statements that are selected as relatively more hypothetical and more likely to be revised when testing shows revision is needed. Empirical testing is the pragmatics of theory language in science. After its test outcome is known, the theory is no longer a theory. The test outcome transforms the theory into either a law or a falsified discourse. Furthermore at some later time a law may revert to a theory to be tested again. For about three hundred years Newtonian mechanics had been received as paradigmatic of scientific law in physics. But Newton's theory of gravitation was tested again in the famous Eddington eclipse experiment of 1919, after Einstein had proposed his alternative general relativity theory. For a brief time early in the twentieth century Newton's "theory" was actually a theory again.

The term "theory" is thus ambiguous in contemporary usage. Both the traditional and the pragmatic meanings continue to be used. In the traditional sense we still speak of Newton's "theory" of gravitation. In the

pragmatic sense it is now falsified physics in basic science, although it is still used by engineers whose applied-science purposes can accept its known error. But this knowledge of the error means that Newtonian mechanics is no longer either a hypothesis for testing or our law-based explanation of the physical universe. Hanson recognized this difference between the pragmatic and traditional meanings of "theory" in his distinction between "research science" and "almanac science."

Pragmatic Definition of the Language of Test Design and Observation

Accepting or rejecting the hypothesis that there are red ravens presumes a prior agreement about the semantics needed to identify a bird's species. Similarly the empirical test of a scientific theory presumes a prior agreement about the semantics needed to identify the test subject, to set up the test apparatus, to perform the test operations, and to characterize the test's initial conditions and outcome. This is done with the test design language. *Pragmatically theory is universally quantified language that is _proposed_ for testing, and test-design language is universally quantified language that is _presumed_ for testing*. Both types of language are believed to be true, but for different reasons. Test-design statements are presumed true with definitional force for executing the test, while the advocates of the theory propose the theory statements as true with sufficient plausibility for testing with an expected nonfalsifying outcome. The descriptive terms common to both the test-design statements and the theory statements thus have their semantics determined jointly by both sets of universally quantified statements.

Observation sentences are test-design sentences and test-outcome sentences with their logical quantification changed from universal to particular quantification for executing the test and for reporting its observed outcome. To describe an individual test execution, the test-design statements have their quantification changed from universal to particular, and are then called observation statements for describing the concrete test. This is a pragmatic sense of observation language, because it depends on the use of the language and not on the semantics. Unlike the Positivists the Pragmatists recognize no inherently observational semantics. The statement predicting the test outcome is a statement of the tested theory with its quantification made particular for the individual test. After the test is performed, the statement reporting the test outcome also has particular quantification for the individual test and is observation language. Whether

or not the actual test outcome agrees with the theory's prediction, both the prediction statement and test-outcome statement have the same vocabulary, and their semantics are the same in so far as their descriptive semantics is definable by reference to the universally quantified test-design statements. Herein lies independence of the test from the theory. Herein also lies the semantical continuity throughout the test for each of the terms common to the test design and the theory regardless of the test outcome, because the parts of the complex semantics defined by the test-design statements are unchanged throughout the test. The statement reporting the test outcome is an observation statement describing what was observed in the test execution. But the prediction statement is not as such an observation statement; it is only incidentally an observation statement when the test outcome is nonfalsifying, such that the prediction is the same as the test-outcome statement. All scientists define the semantics of their observation language when they formulate and accept test designs. Feyerabend had hit upon an important historical insight when he said that in defending the Copernican heliocentric theory Galileo had created his own observation language.

Semantic Individuation of Theories

Theory language is defined pragmatically, but theories are individuated semantically. Theories may be individuated in either of two ways. *Firstly different theory expressions are different theories because they address different subjects.* Theory expressions may be different theories, because they are unrelated; their subjects individuate them. Different theory expressions having different test designs are different theories, because the test-design language identifies the subject of the test. *Secondly different theory expressions are different theories because each makes contrary claims about the same subject*, where different claims usually means different predictions. They have different semantics. Occasionally there is more than one theory proposed for empirical testing with the same set of test-design statements. Since the proposals are all universally quantified and are proposed for testing, they are all instances of theory language. While they have the same test-design statements and therefore all address the same subject, they are not the same theory, because they make contrary claims about the same subject.

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Diachronic Comparative Static Semantical Analysis

There has been much confusion due to philosophers' failure to recognize principles for the individuation of theories. Many philosophers state that theories are not falsified by empirical tests, because all theory choice is comparative, and because scientists retain a falsified theory until a better theory is developed and tested with a nonfalsifying outcome. But when it is said that scientists retain a falsified theory, the response of the scientists is not adequately described. What should be said is that when the scientist tries to save the theory by making adjustments to it, he has made a new theory. When the adjustments are not merely *ad hoc*, but are attempts to modify the universal claims of the theory even in relatively minor ways, in order to enable it to survive a previously falsifying test design, then the original theory has been discarded and a new theory developed.

Theories modified to produce improved predictions while retaining the same test design are different theories. If a change of the test-design has the effect of reducing semantical vagueness or measurement error, the outcome of the empirical test with the modified design may or may not be a falsification of the previously tested and nonfalsified theory. But modified test designs that produce improved predictions produce different theories, which in turn results in a new state description. When the universal statements or equations in either the new theory or test design are used as semantical rules for semantical analysis, the change in meaning of the descriptive terms common to both state descriptions are exhibited by comparison between the two successive state descriptions. Universal statements that are the same in both state descriptions exhibit semantical continuity, while those that have changed or replaced exhibit semantical change. As noted above, such comparison is not possible with a wholistic (or "holistic") view of semantics.

Mathematical Language in Science

The stereotypic "All ravens are black" categorical type of statement is not typically the form used explicitly in the object languages of science. The object language of science is more often expressed either in colloquial language or in mathematical language. Colloquial discourse is often implicitly universal with universality intended. In such cases the grammatical form may lack definite articles or quantifiers, and may be without a copula explicitly containing a form of the verb "to be." Colloquial

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language is often called the "informal" language of science. The informal colloquial expressions can be transformed into the categorical form although usually at the expense of awkward style.

   A mathematical language for science is an object language for which the syntax is supplied by mathematics. The syntax includes the notational symbols and the formation and transformation rules. Whenever possible the object language of science is mathematical rather than colloquial. This preference is not due to an aesthetic appreciation for deductive elegance. Mathematical syntax is preferred, because measurement quantification of the subject of discourse enables the scientist to quantify the error in his theories, after estimates are made for the measurement errors by repetition of the measurements.

Universal Quantification in Mathematical Language in Science

   Mathematical language in science is universally quantified when descriptive variables have semantics but no associated numerical values. It is particularly quantified when numeric values are associated with the descriptive variables either by measurement or by calculation from measurement values. Like the categorical statements, the mathematically well formed formulas, usually equations, are explicitly quantified logically as either universal or particular, even though the explicit indication is not with such logical quantifiers as "every", "all", or "no." Universal quantification is changed to particular quantification in mathematical language, when measurements are made for an ongoing empirical test situation and are associated with the descriptive variables in the equation. When an equation is particularly quantified logically by association with measurement values, it may be said to describe a *numerical measurement instance*. In the case of quantum theory the situation is distinctive by the fact of duality, which means that not all the variables such as those representing momentum and position can have specific values simultaneously. But realizing a value for any one of them makes the logical quantification particular. Quantification is also changed similarly, when numeric values are associated with descriptive variables by computation with the equation and measurement values. When an equation is particularly quantified logically by association with such computed values, it may be said to describe a *numerical empirical instance*, since the referenced instance has not been measured. This occurs when an equation is used to make a quantitative prediction, and the numerical empirical instance is the

predicted value intended to be compared with a measurement value for the same phenomenon in an empirical test.

Semantics of Mathematical Language in Science

The semantics for a descriptive variable is determined by the context consisting of statements and/or equations believed to be true. The semantics-determining statements include measurement language describing the subject measured and the measurement procedures and any employed apparatus. Like the Positivist "operationalist definitions" the statements setting forth the measurement procedures and apparatus contribute meaning to the descriptive term. But unlike the operationalist definitions, each statement does not constitute a separate definition for the measured subject, thereby making the term equivocal. Instead different measurement procedures contribute different parts to the one univocal meaning of the descriptive term, unless and until the different procedures are found to produce different measurement values, where the differences are greater than estimated measurement error. Semantics for the descriptive variables in the theory is also supplied by the equations of the theory itself, such that the structure of their meaning complexes is in part mathematical.

Ontology of Mathematical Language in Science

In the categorical proposition the quantified subject term references individual instances and also describes the attributes that enable identifying the instances, while the predicate term only describes attributes. In an older vocabulary the same idea is expressed by saying that the subject term has personal supposition, while the predicate has only simple supposition. Both categorical statements and colloquial discourse have been called the "thing language", because the instances referenced are "things" or "instantiated entities." Attributes manifest the things of which they are aspects, and enable classification of the manifested things into kinds. The things thus classified and the attributes thus manifested the ontology of the categorical proposition believed to be true. The ontological claim is made explicit by the term "is" in the copula.

However, the ontological claim made by the mathematical equation is not about instances that are things or entities. *The individual instances referenced by the mathematical equation are numerical measurement*

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instances. The measurement instances are related to thing instances and their attributes by the colloquial statements describing the measured subject, the metric, and the measurement procedures including any apparatus, which typically occur in the test design language.

Aside on the Ontological Issue in Quantum Theory

An ontological issue in modern quantum theory in microphysics is about whether or not microphysical waves and particles are two aspects of the same entity. The affirmative view is called the "duality" thesis. Its advocates cite the de Broglie equation relating both wave and particle properties, and also note that the mathematical expression for the wave function can be transformed into the mathematical expression for the matrix mechanics. One version of the negative view is called the "pilot wave" thesis, which affirms the separate reality of wave and particle, and says that they always found together as exhibited in the Young two-slit experiment. Other versions deny the reality of either the wave or the particle. This ontological issue cannot be resolved by appeal to the mathematically expressed theory, because the mathematics says nothing about entities. It only references numerical measurement instances. Bohm was correct in maintaining that the interpretation issue of the quantum theory is in the informal language of physics, and not in the theory's mathematics. The issue about entities is supplementary to the mathematically expressed and empirically tested quantum theory. This ontological issue has therefore continued for many decades, as each side advocates its preferred informal language and associated ontology to address the question of individual entities. The issue is a variation on the ontological problem of the red raven.

Dynamic Diachronic Metalinguistic Analysis

Turn next to the dynamic diachronic metalinguistic analysis, the examination of the processes of how the language of science changes through time from one language state to a later one. *Language changes in science result from the two basic types of research functions: theory development and theory testing.* The linguistic changes are not merely incidental to the performance of basic research, since the product of basic science is new language consisting of theories hopefully yielding laws and explanations. A change of state description is produced whenever a new

theory is proposed, and whenever a proposed theory is tested by the most critically empirical test that can be applied at the current time. If the test outcome is a falsification, the proposed theory is eliminated from the current state description. When the test outcome is not a falsification a theory has become a new law in the state description.

The Institutionalized Aim of Science

The preceding sections have discussed the archetypal twentieth-century philosophies of science: Romanticism, Positivism, Pragmatism, and Psychologism. And they have also discussed the basic perspectives of language: syntax, semantics, ontology, and pragmatics. Finally consider next the four topics in philosophy of science in the light of these previous discussions beginning with the institutional aim of science.

Issues about the aim of science are the most fundamental, because they profoundly affect all the other topics. And as it happens the literature of philosophy of science offers a variety of proposals for the aim of science. The Positivists had proposed that science should achieve firm foundations either by relying on observation language exclusively or by limiting theoretical terms to those that are related by logical reduction to an observation language serving as a reduction base. Neurath, a proponent of the unity of science agenda, proposed that all sciences including the social sciences aim at logical reduction to physics, which in turn is to be reduced to observation. On the other hand Romantics in the social sciences maintain that the sciences of nature differ fundamentally from the sciences of culture, which are the social sciences. They propose that science aims at vicarious imputation of subjectively based interpretative "understanding", so that an explanation "makes sense" to the social scientist due to his personal experiences as a participant in shared human nature and, when possible, participation in the same culture as the social agents he is studying. Some of them advocate the philosophy of Weber, in which this understanding called "*verstehen*" is not only a source for the requisite mentalistic ontology, but is also a basis for validation. Most fundamentally Romantics who do not altogether reject the aim of prediction and control in cultural sciences, subordinate it to interpretative understanding.

Most of the more recent proposals in academic philosophy of science arise from reflection on episodes in the history of the natural sciences. Popper, reflecting on the development of relativity theory by Einstein, proposed that the aim of science is to produce tested and nonfalsified

theories having greater information content than their predecessors. Kuhn, reflecting on the development of the much earlier Copernican heliocentric theory, proposed that small incremental changes extending the prevailing theory defines the institutionalized aim of science, which he called "normal" science, and that scientists do not consciously aim to produce revolutionary new theories. Feyerabend, reflecting on the development of the quantum theory, proposed that each scientist has his own aim, and that anything institutional is an impediment to science. His philosophy of science is an early variation upon the then-emerging Pragmatist ideas, but it is also a quite idiosyncratic version. Thagard, reflecting on the wave theory of sound and on other more recent developments in natural science, proposed that scientists choose theories that maximize what he calls "explanatory coherence", which he defined in terms of empirical adequacy, breadth of explanation, simplicity of explanation, and analogy with established explanations. He developed his computerized cognitive system **ECHO,** to simulate the realization of this aim in various episodes of theory choice in the history of science.

The contemporary Pragmatist philosophy is now the ascendant view in academic philosophy. It evolved from an examination of the development of quantum theory in physics in the 1920's and from a consequent critique of Positivism.   However, the mature articulation of the contemporary Pragmatism did not come to fruition until the early 1970's.   Today Pragmatists view modern empirical science as a cultural institution having its distinctive system of views and values. The institutionally regulated activities of research scientists may be described succinctly in a statement of the aim of science, which the contemporary research scientist seeking to maximize his success may employ as what some social scientists call a *rationality postulate*. The Pragmatist rationality postulate for the practice of research in the empirical sciences is the following statement of the aim of science:

> **Scientists aim to construct explanations by developing theories that satisfy the most critically empirical tests that can be applied at the current time. Such satisfactory theories may be called scientific laws.**

This statement is explained by examining the second, third, and fourth topics in philosophy of science as three sequential steps. It can be rephrased to describe the successful achievements in the history of science, so as not to impute motives to scientists whose personal objectives and psychological

experiences often cannot correctly be described in a statement of the conscious aim of science. The statement rephrased in terms of successful outcomes instead of a conscious aim reads as follows:

> **Science achieves explanations by developing theories that satisfy the most critically empirical tests that can be applied at the current time. Such satisfactory theories may be called scientific laws.**

Institutional Change

Change of institutions is different from change within institutions. In the history of science successful researchers in basic science have routinely failed to understand the reasons for their success, and have often formulated or accepted erroneous philosophies of science to explain their successes. One of the most historically notorious such misunderstandings is Newton's "*Hypotheses non fingo*", his denial that his monumental theory of gravitation is a hypothesis. In due course such false practices and beliefs become suspect, as successful developments are achieved in spite of the erroneous proscriptions and prescriptions. As Feyerabend noted in his *Against Method*, successful scientists have often broken prevailing methodological rules. The successful and institutionalized practices of scientific research had firstly to evolve through trial and error before they could be examined, analyzed, and formulated into new philosophies of science. The rationality postulate is therefore a postulate in the sense of a hypothesis, and what is rational today will likely be seen tomorrow as superstition, as both science and philosophy of science continue to evolve. Not surprisingly there exists what may be called a cultural lag between the evolution of science and the development of philosophy of science, since the latter depends on the former. For example over thirty years passed between the development of the modern quantum theory and the consequent emergence of the contemporary Pragmatist philosophy of science. The evolution in science that involves a revision of the rationality postulate amounts to an *institutional change*. Such changes do not occur rapidly or easily, and are usually intergenerational due to the magnitude of the adjustment.

Not only is there a cultural lag between science and philosophy, there are also cultural lags among the several sciences. Philosophers of science have preferred to examine physics and astronomy, because these have been

the most advanced of the sciences since the historic Scientific Revolution, which started with Copernicus. Many other sciences have tended to lag behind physics and astronomy with the social and behavioral sciences farther behind than the natural sciences other than physics and astronomy. The result has been the survival of philosophical superstitions in the lagging sciences, especially to the extent that they have looked to their own less successful histories to formulate their own philosophies of science. For example sociologists and many neoclassical economists continue to use a Romanticist philosophy of science, and believe that cultural sciences or sciences of "human action" are fundamentally different from the natural sciences. In addition, the behaviorist school of psychology continues to use the Positivist philosophy of science. In the contemporary perspective these sciences are institutionally retarded, because they impose prior ontological commitments – either mentalistic or nonmentalistic - as criteria for scientific criticism.

Institutional change in science must be distinguished from change within the prevailing institutional matrix of the aim of science and the criteria for scientific criticism. Philosophy of science is principally concerned with the latter. It has less to say about the former except retrospectively, because institutional change is unique and distinctively historical. Its occurrence can be recognized retrospectively, because it is seen to involve not only a change in formerly accepted explanations in science, but also a change in the prevailing concept of the nature of science itself. Contrary to philosophers such as Kuhn, the existing institutional matrix is not identified with the prevailing scientific explanations. There have been revolutionary developments in science such as Darwin's theory of evolution that had no effect on the institution of basic science, however great the impact Darwin's theory had on the science of biology and on the macrosociety. In fact it is the enduring stability of the institution of science through even dramatic revolutionary changes that makes philosophy of science possible and useful to the practitioner of basic research science.

Scientific Discovery

Recall the distinctively Pragmatist meaning of the term "theory" as universally quantified statements proposed for testing. The topic of scientific discovery is the problem of creating new theories that will pass empirical testing with nonfalsifying outcomes. There have been other ideas about discovery depending on the meaning of "theory." Positivist

philosophers' discussion of this topic consisted of induction, which yields empirical generalizations, and of the human creative processes, which yield theories. But they could offer no explanation as to how scientists create theories. For Positivists the term "theory" refers to sentences containing "theoretical terms", which describe unobserved entities. Such entities can be microphysical particles such as electrons or mental states such as ideas. For Romantic social scientists and philosophers the creative process consists of the imputation of vicariously based ideas and motives that "make sense" to the social scientist because he can recognize them in his personal experience. Thus the social sciences are cultural sciences in which the term "theory" refers to language describing the mental states experienced by the subjects of their social theories. On the contemporary Pragmatist view there is no separate class of vocabulary called "theoretical terms", as the Positivists thought, nor do mental experiences warrant uniquely labeling discourse about it "theory", as the Romantics thought. For the contemporary Pragmatist philosophers "theory" is defined pragmatically instead of semantically; it is any universally quantified discourse proposed for empirical testing. Thus the problem of scientific discovery is essentially that of analyzing and proceduralizing the creation of such statements that are empirically testable and hopefully when tested are not falsified.

As mentioned above both *theory development* and *theory testing* change the state description of the language in the science, and thus offer a dynamic diachronic view. Theory creation introduces new language into the current state description, while falsification eliminates language from the current state description. The most significant work addressing the problem of scientific discovery has been the relatively recent development of computerized discovery systems. These systems, also called "artificial-intelligence" systems, describe the transition from an inputted state description to an outputted one generated by the computer system and representing a later language state. To be useful *every discovery system must contain procedures both for theory creation and for theory selection.* Different computer systems created by different developers implement different strategies in their system designs for the discovering. If the discovery system is a generative grammar, then only the descriptive vocabulary from the initial state description is inputted to the system. But whatever the system design, the input information is from an initial state description, and the output information is the terminal state description. There are issues in the philosophy of science literature as to just what the state descriptions are describing. On the cognitive psychology agenda, the state descriptions represent in individual's psychological state consisting of

mental representations. On the linguistic analysis agenda, the state descriptions represent the shared semantics of a language-using community constituting a scientific profession. On either interpretation, however, the input state description represents the knowledge available for future discovery, and the output state description is the one or several new theories that constitute the discovered knowledge.

The sources of language for the input state description is crucial for a discovery system. In his *Introduction to Metascience* (1976) Hickey distinguished three types of theory development that are relevant to input language: (1) theory extension, (2) theory elaboration, and (3) theory revision. Firstly theory extension is based on a currently nonfalsified theory that is used to address the scientific problem under investigation. The extension could be a simple addition of statements to make a general theory more specific for a new problem. This process involves minimal change.

Secondly theory elaboration is the correction of a currently falsified theory by the addition of new factors or variables in a manner that changes the theory's predictions while preserving the theory's universal quantification so it is not merely *ad hoc*. The input language consists of factors or variables that represent anything that seems plausible for solving the problem, and the amount of vocabulary inputted to a mechanized discovery system could be large. This theory-development strategy amounts to a fishing expedition in search for a correcting factor or variable.

Thirdly theory revision is essentially a reorganization of the constituent information in existing theories. The source of input for theory revision consists of the descriptive vocabulary from all the currently nonfalsified theories addressing the problem at hand. The nonfalsified theories need not have been tested empirically. Since the problem is unsolved, it does not have any theory that is tested and not falsified. The descriptive vocabulary from recently falsified theories may also be included as inputs to make an accumulative state description. Rejected theories have scrap value. The size of the input state description is relatively small. Yet it must be large enough to supply sufficient information for the development of a new theory. The new theory is typically very different from previous theories. This output is most likely to be called "revolutionary." Hickey's **METAMODEL** system has been used for both theory elaboration and theory revision, often combined in the same input.

The revision can also be the patterning of a proposed solution to the new problem by analogy with an existing explanation. Thagard's reconstruction of the development of the theory of sound waves on analogy with water waves by means of his **PI** system might be taken as an example

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of mechanized theory revision. This source of input for analogy, however, is potentially very large, and this strategy has not been used in any mechanized system for developing a contribution to the current state of any science, although there are many examples of the use of analogy in the history of science.

To date discovery systems that have actually produced new theories for a scientific profession have had certain characteristics. Firstly researchers working in their own specialized scientific field of application have developed the effective discovery systems, while neither academic philosophers nor cognitive psychologists have such a track record. Cognitive psychologists have been content to apply their discovery systems to the replication of past episodes in the history of science, rather than apply their systems to the current state of a science and actually produce new theories. Their efforts to date have been like a stage play in perpetual rehearsal with no performance. Secondly the discovery procedures used in the systems are typically described as merely the mechanized automation of theory-developmental practices already used in the scientific field of application. Thirdly the input descriptions contain numerical data, and the mechanized discovery procedures applied to the input data incorporate statistical-analysis procedures. Fourthly and finally the scientific fields of application have been the social sciences. Statistical inference procedures are commonly used in the social sciences to discover relations among data, thus making these sciences obvious opportunities for the first useful discovery systems.

Scientific Criticism

The philosophical discourse on scientific criticism has little to say about the specifics of experimental design. Instead it pertains to the criteria for the acceptance or rejection of theories. The only criterion acknowledged by the contemporary Pragmatists is the empirical test. Whenever in the history of science there has been a conflict between the empirical criterion and any nonempirical criteria for the assessment of new theories, eventually it was always the empirical criterion that governed theory selection. Contemporary Pragmatists accept scientific realism and ontological relativity, and therefore reject all prior ontological criteria and subordinate ontological commitment to empirical criticism.

INTRODUCTION

The Logic of Testing

The universally quantified statements of the theory in an empirical test can be cast into a conditional proposition in the form "If A, then C." The antecedent clause "A" represents the set of universally quantified statements describing the antecedent conditions, those of the test-design for the test. When the test is executed the logical quantification of "A" is changed to particular quantification to describe the individual test situation, and it is regarded as true, if the test is executed in compliance with its test design. *The empirical test is conclusive only if it is executed in accordance with its test design.*

The consequent clause "C" represents the set of universally quantified statements describing the predicted outcome of the execution of a test. Its logical quantification is changed to particular quantification to describe the predicted outcome of the individual test. Another statement, "O", which also has particular quantification, describes the observed outcome from execution of the test in the same vocabulary that is used in the prediction statement "C." The logic of the test is the nontruth-functional *modus tollens* argument form, according to which the conditional hypothetical statement expressing the theory is falsified if the statements "C" and "O" are not accepted as saying the same thing, i.e. if the prediction is wrong.

The *nontruth-functional* conditional logic implements Popper's falsificationist philosophy of scientific criticism. The conditional statement expressing the tested theory asserts not merely a conjunction, but a *dependency* between the phenomena described by the antecedent and consequent components. This claimed dependency cannot be conclusively established or verified on the basis of the truth-values of the component statements except in the case of falsification. The truth table for the truth-functional Russellian logic therefore is not the logic of empirical testing in science. When the antecedent clause is false, the test is invalid due to a failure to comply with its test design. For purposes of comparison truth-functional and nontruth-functional truth tables appear as follows:

Truth-Functional Truth Table			Nontruth-Functional Truth Table		
A	**B**	**A ⊃ B**	**A**	**B**	**If A, then B.**
T	T	T	T	T	Not Falsified
T	F	F	T	F	Falsified
F	T	T	F	T	Invalid Test
F	F	T	F	F	Invalid Test

INTRODUCTION

Empirical Decidability and Semantics

The decidability of empirical testing is not absolute. Popper had recognized that the statement reporting the observed test outcome, which he called a "basic statement", requires prior agreement among the cognizant scientists, and that it is not incorrigibly true. Normally the semantics is such that if a test has a nonfalsifying outcome, the semantics is unchanged with the universally quantified statements of both the theory and the test design contributing to the meanings of the terms common to both kinds of statements. But when the outcome is a falsification, there is a semantical change produced for those who accept the outcome as a falsification of the theory. The test-design statements continue to control the semantics of the terms common to the theory and test design by contributing their parts of the meaning complex of each of the common terms. But the parts of the meaning complex contributed by the theory statements are excluded from the semantics of those common terms, at least for those who previously believed in the tested theory but no longer do as a result of the test.

In the event of falsification, there is also a different semantical change produced for those who do not accept the outcome as a falsification of the theory. Such a dissenting scientist has reconsidered either the test-design statements or the report of the test outcome. If he challenges the test outcome, then he has merely questioned whether or not the test was executed in compliance with its agreed test design, and the test may be repeated to answer his challenge to validity.

But if he challenges the test design itself, then he has changed his mind about the test design, and has thereby changed the semantics involved in the test in a fundamental way. The semantical change produced for such a recalcitrant believer in the theory consists in the theory statements controlling the meanings of the terms common to the theory and test-design statements. In that case the parts of the meaning complex contributed by the test-design statements are the parts excluded from the semantics of at least one and probably several of the terms common to the theory and test-design statements. This amounts to attacking the test design as if it were falsified, and letting the theory define the subject of the test – a role reversal in the pragmatics of the test design and theory language, that redefines the problem under investigation. Popper rejects such a response to a test, calling it a content-decreasing stratagem, and he admonishes the scientist to stick to his problem and refrain from criticizing everything. But the dissenting scientists may decide that the design of the falsifying test is a misconception of the problem that the tested theory is intended to solve, and may take exception

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to a measurement procedure or other aspects of the test design. *Empirical tests are conclusive decision procedures only for the scientists who agree on which language is proposed theory and which language is presumed test-design.*

Empirical Underdetermination

Another factor affecting decidability of empirical testing is the empirical underdetermination of language, with the result that empirical criteria cannot always result in unambiguous theory choice. Mathematically expressed theories use measurement data containing some measurement error, which is a manifestation of empirical underdetermination. Scientists like measurement and mathematically expressed theories, because they can measure the amount of error in the theory. But separating the measurement error from the prediction error made by the equation constituting the theory can be problematic. Repetition of the measurement procedure enables estimation of the degree or range of measurement error. If the prediction made by the equation exhibits an error that is large relative to the estimated measurement error, then the theory is deemed conclusively falsified. Otherwise the theory is either untestable or the test design is inadequate for the theory. If there are several theories yielding prediction errors that are different but small relative to one another, and are also small enough to be within the estimated range of measurement error, then the inescapable empirical underdetermination inherent in language has imposed undecidability in the choice of alternative theories for the given test design. The problem of empirical underdetermination also occurs in the testing of qualitative theories. In such cases the empirical underdetermination is manifested as conceptual vagueness. All concepts have vagueness, which can be reduced but can never be eliminated. *Empirical tests are conclusive to the extent that measurement error and vagueness are small relative to the effect produced in the empirical test.*

Given the dilemma of having several alternative theories that are not falsified by empirical test due to empirical underdetermination, philosophers have proposed nonempirical criteria that may be operative in theory choice. But no such nonempirical criteria enable scientists to predict which alternative nonfalsified theory will make more reliable predictions, when the degree of empirical underdetermination is reduced by improved observation practices or test designs. And when scientists are confronted by such dilemmas, better observation practices with test designs having added

clarifying information or more accurate measurements are in order. The existence of several alternative theories that have survived an empirical test without having been falsified is thus endemic to science. In the social sciences that use statistical techniques for testing this is a common outcome, but it also happens in natural sciences. Einstein had described this situation in physics as an "embarrassment of riches." The resulting multiple explanations are equally scientific. Different scientists may have distinctive reasons, such as aesthetic, circumstantial, or strategic reasons, for preferring one alternative explanation to another. Thagard has noted three such criteria implemented in his **ECHO** system, his artificial-intelligence system specifically for theory selection. He finds that the most important criterion is breadth of explanation, followed by simplicity of explanation, and finally analogy with previously accepted theories. Where the empirical criteria are not decisive, theory selection becomes more of a professional career decision for the scientist rather than a purely scientific one. For example knowing what a profession currently likes to see in new theories helps getting a paper published in the refereed literature. Thagard considers these selection criteria to operate as inference to the "best" explanation. But contemporary Pragmatists are inclined to exclude all such nonempirical criteria from the aim of science, because while relevant to persuasion, they are irrelevant to evidence. They are like the psychological criteria that trial lawyers use to select and address juries in order to win lawsuits, but have nothing to do with courtroom evidence rules.

Scientific Pluralism

Language is always empirically underdetermined by the real world, such that there is always the possibility of the development of a new theory that is empirically equal to or superior to currently accepted explanations of the same subject. This empirical underdetermination may be due to errors of measurement or to the residual vagueness always present in descriptive variables and terms, and it is often reduced by development of more adequate observation practices and technologies for test designs. The undecidability of the resulting empirically adequate multiplicity of scientific explanations is recognized by the Pragmatist thesis of "scientific pluralism." *Scientific pluralism is the undecidability among alternative laws and consequently explanations due to the empirical underdetermination of language.*

INTRODUCTION

Nonempirical Linguistic Constraints

The constraint imposed by empirical test outcomes, the empirical constraint, is an institutionalized cultural value that is not viewed as an obstacle to be overcome, but rather is a condition to be respected for the advancement of science. There are other constraints that are viewed as impediments that must be overcome for the advancement of science. Some of these impediments are purely circumstantial. They may be sociological, dogmatic, financial, political or academic. These impediments are external to science. There are two other nonempirical constraints that are internal to science in the sense that they are inherent in the nature of language. They are *the cognition constraint and the communication constraint.*

The cognition constraint inhibits a theorist's ability to construct new theories, and it is manifested as what is often mundanely referred to as lack of imagination. Semantical rules are not just rules; they are also linguistic habits that enable fluency in both thought and speech. The rules are such that the meaning of a descriptive subject term is determined by the set of universally quantified statements believed to be true. Thus given the belief in certain universally quantified statements, the meanings of their constituent descriptive terms are determined. Conversely given the established meaning of a descriptive term, certain conventions and beliefs are sustained, with the result that change of belief is made difficult by the need to change linguistic habits. Accordingly the more revolutionary the revision of beliefs, the greater the impeding force of the cognition constraint imposed by psychological habit. And if a new syntax is required such as an unfamiliar mathematics, then the semantical restructuring of the affected meaning complexes is even greater. This follows from the relativistic semantics, which is opposed to the thesis that language is neutral in the sense of being merely a passive instrument for thought. It is noteworthy that the use of discovery systems circumvents this problem, because the machines have no linguistic habits; they mechanically apply a generative grammar to inputted linguistic symbols.

The communication constraint is similar to the cognition constrain; it is merely the impediment to understanding a new theory relative to those currently known due to prevailing linguistic habits. The scientist must learn the new theory well enough to restructure the composite meaning complexes associated with the descriptive terms common to both the old theories he already knows and new theory to which he had recently been exposed. And it may be noted that the scientist viewing the computerized discovery system

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output experiences the same communication impediment with the machine that he would have were the outputted theories developed by a fellow human scientist.

If the differences between the old and new theories are very great, some members of the affected scientific profession are unwilling or unable to accomplish the learning adjustment required, and they become the rear guard defending the older conventional wisdom. In the meanwhile the developers and advocates of the new theory, who have mastered the new theory's semantics, assume the role of the *avant garde* until the new theory's acceptance has become sufficiently widespread that it has become the new conventional wisdom appearing in the textbooks. This is the conversion process described by Kuhn in revolutionary transitional episodes. However, contrary to Kuhn the transition does not involve a complete semantical discontinuity. Rather involves an extensive restructuring of the new theory's semantical description of the domain common to both old and new theories as described by the semantics in their shared test design statements.

Scientific Explanation

Whether viewed heuristically or historically the ultimate aim of basic science is the production of explanations. One of the most obvious characteristics of an explanation is that it consists of language. Thus it may be said that basic science produces a linguistic artifact. This is what distinguishes basic or pure science from applied science and technology. Applied science and technology produce nonlinguistic real products, such as engineered buildings, medical clinical therapies, and social policies affecting human activities. So long as a tested theory has not been falsified, it is accepted as a scientific law, which may occur in an explanation. Thus in the contemporary Pragmatist philosophy of science "explanation" is defined as follows: *An explanation is a deduction containing one or several scientific law statements concluding to statements describing particular events or to universal statements.* Laws and theories are distinguished pragmatically. *A law statement is a former theory that has been tested by the most critical test design currently possible and is not yet falsified by the executed empirical tests.*

The statements or equations of an explanation, like those of a theory, are law statements that are universally quantified logically. And the litmus test of the law's universal claim is the prediction of future events or of currently unavailable evidence for past events in an explanation. Prediction

is the guarantee that the law is not *ad hoc* to its development sample. Furthermore a motivating and social justification, which is external to basic science, is the control that is often yielded by basic science's power of prediction. Such control enables applied science and technology, which fundamentally distinguishes applied science from basic research science.

Summary

This introduction started with discussion of four types of philosophy of science – Romanticism, Positivism, contemporary Pragmatism, and psychologistic computational philosophy of science. It then took up philosophy of language – syntax, semantics, ontology, and pragmatics. And it finally considered the four topical areas of philosophy of science – the aim of science, discovery, criticism, and explanation. To facilitate an integrated understanding of these three discussions, the following recapitulation picks up the stick from the other end, as it were, and structures the whole discussion around the four topical areas.

Aim of Science:

On the Romantic philosophy the natural and cultural sciences have different aims. Romanticists do not object to the Positivist view of the aim of the natural sciences. In fact it supplies Romantics with a stereotypic misunderstanding of natural science. But Romantics maintain that the aim of the cultural sciences of human action consists of interpretative understanding in terms of the conscious views, values, norms and motives of human subjects. Thus they require a mentalistic ontology for the cultural sciences. And they also deny that explanation in cultural science aims at prediction and control.

On the Positivist philosophy the natural and social/behavioral sciences have the same aim. That aim is to enable prediction and ideally control of phenomena by means of language either expressing or founded upon direct observation. Positivists reject a nonobservable mentalistic ontology for social sciences.

On the contemporary Pragmatist philosophy the aim of science is explanation that enables prediction and ideally control of the real world. Like the Positivists they maintain the aim of science is the same for all sciences, but unlike the Positivists and the Romantics they reject commitment to any ontology prior to empirical testing, whether mentalistic

or nonmentalistic. Pragmatists permit but do not require mentalistic or nonmentalistic ontologies.

On the cognitive psychology view the aim of science is whatever they find in history; they do not characterize it. Philosophically they are eclectic. They reject Behaviorism, which is Positivism in psychology, yet they distinguish observation terms, as those that are inputted to the system, from theoretical terms, as those that are generated and outputted by the system. Like the Romantics they view the subject of their psychological investigations as mental representations, but contrary to the Romantics they equate human concepts to the data structures in their computer systems. And unlike Positivists they are not nominalist. Within these parameters they select criteria for scientific criticism according to what is needed for their systems to replicate the particular historical episodes that they simulate mechanically.

Discovery:

The Romantics' concept of scientific discovery for cultural science is based on their concept of scientific theory, which they define in terms of the mental states experienced by the social actors whose actions they investigate. Acquiring this kind of knowledge is believed to require introspection by the researcher. The Romantics therefore deny that social theory can be developed exclusively by analysis of empirically acquired social statistics, and they have a Luddite attitude toward computational theory development with mechanized discovery systems.

The Positivist' concept of scientific theory is also distinctive. They dichotomize observation language and theory language. The latter contains descriptive terms referencing entities that have never been observed, and thus given their nominalism they presume that theory language is not meaningful until reductively related to the observation language. Discoveries expressible exclusively with observation terms are called empirical generalizations. Generalizations are the product of induction resulting from recognition of similarities in repeated direct observations. The Positivists offer no explanation for the discovery of theories except to note that theories are free creations of the imagination and are not generalizations based in observation.

The contemporary Pragmatists' concept of theory differs from both the Romantics' and the Positivists' concept, because the Pragmatists reject associating theory with any prior ontology. They define theory functionally as any universally quantified discourse proposed for testing. This concept

lends itself to computer processing, since any output from the discovery system is considered to be theory proposed for further empirical testing.

Finally the cognitive psychologists' principal concern is with the development of computerized systems, with the objective of characterizing, proceduralizing and mechanizing the psychology of the discovery process.

Criticism:

The Romantics require as a criterion for scientific criticism in cultural sciences, that the language describe a mentalistic ontology. Language that does not conform to this prior ontological criterion is rejected out of hand as "atheoretical" and as unsuitable for cultural science notwithstanding valid empirical findings. Some Romantics furthermore require Weber's *verstehen* criterion that the theory be empathetically or vicariously plausible in the personal experience of the researcher. Such theories are said to "make sense."

The Positivists maintain that empirical generalizations are always provisional, and must be tested empirically. The early Positivists such as Mach viewed theories as temporary expedients relegated to less than scientific status, to be replaced later by empirical generalizations based on direct observations as science progressed. The later Positivists such as Carnap were more accepting of theories, but conditioned the acceptance of theories not only on the confirming outcome of scientists' empirical test, but also conditioned the theory's meaningfulness on the philosophers' logical reduction to an observation language.

The contemporary Pragmatists give absolute authority to the outcome of empirical testing as the criterion for theory acceptance and selection, so long as the observed effect is large enough to be distinguishable from error due to the empirical underdetermination of language. They view falsification as conclusive. They deny that an empirical test outcome can establish a theory, but they accept nonfalsification as warranting belief in the theory's ontological claims. The empirical underdetermination manifested by measurement error or conceptual vagueness results in undecidability, such that more than one theory may be empirically nonfalsified. This scientific pluralism permits the scientists to choose among the alternative tested and nonfalsified theories on the basis of other criteria, such as simplicity or familiarity.

Cognitive psychology will consider any criteria for scientific criticism that their cognitive systems can successfully use to simulate historical episodes in the progress of science. These have included empirical

adequacy, breadth of explanation, simplicity of explanation, and analogy with established explanations.

Explanation:

The above mentioned considerations flow through to the topic of scientific explanation. For the Romantics explanation in cultural science is interpretative understanding. Knowing the social actors' misunderstanding is deemed more important than connecting their intentional action to its unintended consequences. Romantic explanation is discourse having the required mentalistic ontology.

The Positivists on the other hand are committed to an observational ontology traditionally called phenomena, sense data, or sensations. They typically reject the mentalistic ontology of the Romantics. On their philosophy scientific laws are either empirical generalizations containing only observation terms, or they are theories confirmed by empirical testing and found meaningful, because their theoretical terms have been logically related to a suitable observation-language reduction basis.

The contemporary Pragmatists define scientific law as language that was formerly theory, because it has been empirically tested and has not yet been falsified. And since nonfalsification warrants belief, the law and the explanations in which it is used describes its own ontology.

The cognitive psychologists view an explanation as either the output of a cognitive discovery system or a primitive term in a theory-selection system, which is applied to a problem in basic research science. Cognitive psychologists construe an explanation as a conceptual representation.

ERNST MACH AND PIERRE DUHEM ON PHYSICAL THEORY

Ernst Mach (1838-1916) is a representative figure of the early Positivist philosophy of science in physics at the turn of the twentieth century. He earned a doctorate in physics from the University of Vienna in 1860, taught experimental physics for most of his career at the University of Prague (1867-1895), and then held the chair of Inductive Philosophy at the University of Vienna (1895-1901). He set himself the philosophical task of implementing the phenomenalist philosophy of David Hume in physics while Newtonian mechanics still prevailed in physics, and he made contributions to physics, psychology, and education, as well as to philosophy.

Pierre Duhem (1861-1916), another important early Positivist, studied physics at the Ecole Normale in Paris, where he received a doctorate in physics, and was a professor of physics at the University of Bordeaux for most of his career. His principal interest was physical chemistry, where he aspired to recast the theoretical foundations of chemical processes on the basis of a generalized thermodynamics. Unlike Mach, Duhem accepted the Aristotelian metaphysics, which he viewed as separate from Positivist physics, and believed that progress in physical theory asymptotically approaches a "natural classification", which he viewed as analogous to the cosmology of Aristotle. Duhem's philosophy differed from Mach's philosophy by the former's acceptance of physical theory as integral to physics, and his development of a semantical metatheory to locate theory in Positivist physics. This semantical metatheory served as the basis for a general philosophy of language in the contemporary Pragmatism, and retrospection reveals that it has been his more lasting philosophical contribution.

MACH AND DUHEM

Mach's Phenomenalism

Prior to the contemporary Pragmatism philosophers based their philosophies of science on one or another metaphysical viewpoint. Though Positivists philosophers including Mach were explicitly "antimetaphysical" (Mach even denied that he was a philosopher), they were actually advocating their own metaphysics while labeling the views they opposed as "metaphysical", and using the term as a pejorative. Positivism is a philosophy that evolved in reaction against the various Romantic philosophies, and what the Positivists meant by "metaphysics" was the metaphysics of the Romantics. Just as the views of the Romantics evolved from the philosophical tradition of the Rationalists, similarly those of the Positivists evolved from the tradition of the Empiricists. Thus Mach's epistemology is very similar to the views of the Empiricists Berkeley and Hume, and he explicitly expressed indebtedness to them in his works.

Mach's principal work setting forth his phenomenalist philosophy is his *Analysis of Sensations* (1885), which went through five editions in both German and English, although Mach also discussed his epistemological views in many of his other works. His epistemology postulates "elements" such as individual sounds, temperatures, pressures, spaces, times, and colors. When these elements are considered in relation to one another, they are studied by the physical sciences, and when they are considered in relation to the human mind or rather the nervous system of the human body, they are called "sensations" and are studied by psychology. One of the central theses of Mach's *Analysis of Sensations* is that the only difference between elements and sensations is the aspect under which they are viewed, and that physics and psychology therefore have the same subject matter. The distinction between the physical and the psychical is entirely a matter of convenience or practicality, because everything is merely a function of these elements. Everything is a mental construct consisting of complexes of sensations. All material things including our own bodies and even the ego are nothing but complexes of elements that have been constructed by the human mind having some fixedness or constancy in sense experience. A fundamental thesis of Mach's philosophy is that material bodies do not produce sensations, but rather complexes of sensations are associated together by the human mind to produce material bodies. Ultimately all that is valuable in science is the discovery of functional relations of dependency of sensations upon one another. The constancies that enable our mental construction of physical bodies have no privileged reality status. This is even more so with such mental constructs as the physicists' molecules and

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atoms, which are mental constructs that unlike those of physical bodies are not found in experience. The Positivist phenomenalist philosophy is a nonrealist metaphysics, and if it is generously said to have an ontology, the ontology consists merely of the phenomenal elements or sensations.

Mach's Philosophy of Science

Aim of Science

Mach's philosophy of science is rich enough that it addresses all the four basic topics conventionally considered in a philosophy of science: the aim of science, explanation, criticism, and discovery. He offers several statements of the aim of science. One sets forth the "biological task of science", which is to give the fully developed human individual with as perfect a means of orienting himself as possible. In a second statement he says that the aim of all science is the representation of facts in thought either for practical purposes or for removing intellectual discomfort, since every practical and intellectual need is satisfied when our thoughts can represent the facts of the senses completely. He adds that our knowledge of a phenomenon of nature is as complete as possible, when thoughts are set before the mind's eye such that all the relevant sensible facts can be regarded as a substitute for the phenomenon itself. Then the facts appear to be familiar and are not able to occasion any surprise. In a third statement he says that the goal of science is the simplest and most economical abstract expression of facts. The noted economy of science involves uncompleted facts, judgments or laws. The last two statements of the aim of science are essentially the same as Mach's theory of scientific explanation, and do not represent different aims for science. And unlike the first, the second and third statements give an aim that is intrinsic to science.

Scientific Explanation

Mach set forth his theory of scientific explanation in many places including his *Analysis of Sensations*, his "The Economical Nature of Physical Inquiry" (1882) and "On the Principle of Comparison in Physics" (1894) reprinted in his *Popular Scientific Lectures* (1898). He says that explanation is the economical description of experience in terms of elements. When we examine facts for the first time they appear confusing. In time we discover simple stable elements out of which we can mentally construct the entire factual domain, and when we have reached the point

where everywhere we can discuss the same facts with other persons, then we no longer feel lost and the phenomenon is explained. The explanation offers a survey of a given domain of facts with the least expenditure of thought. The representation of all the facts of a domain by some one single mental process is economical. He adds that the greatest perfection in mental economy occurs when science uses mathematics.

Not all descriptions are explanations; only direct descriptions can be explanations, while theories on the other hand are indirect descriptions and are not explanations. Direct descriptions may be either complete or incomplete. Description of what is presently observed is a complete description. Incomplete description refers to what is presently unobserved but observable and what is associated by a law, as for example the movement of a comet that is presently unobserved or the body of a man who disappears behind a pillar. The incomplete description can be completed by the human mind by means of the associations made by a scientific law. A direct description is one in which a single feature of resemblance among facts is called from memory, while a theory such as the description of light as a wave motion is an appeal to another description that had previously been made elsewhere. A theoretical idea offers more than what we actually observe in a new fact. It can be used to extend a fact and enrich it with features, which we are firstly induced to seek from its suggestions and, which are often actually found. A theory may lead to discoveries, but the adoption of a theory always carries a danger: even the most fruitful theory may be an obstacle to inquiry. By way of example Mach says the theory that light is an undifferentiated straight line of particles impeded the discovery of the periodicity of light. The ideal of a given domain of facts is direct description; such description accomplishes all that the scientific investigator could wish

Scientific Criticism

In the *Analysis of Sensations* Mach states that he has taken Hume as his starting point, and this starting point is reflected in his views on scientific criticism. The scientist like everyone else knows the elements with complete certainty as sensations. But scientists and other persons also make judgments that are laws or generalizations. Since the aim of science is the adaptation of thoughts to facts, a new fact may require a new adaptation, which finds its expression in the operation of judgment. A judgment is the supplementing of a sensational presentation, in order to represent more completely a sensational fact. In the adaptation of thoughts to facts the

adaptation can be made only to what is constant in the facts. Only the mental construction of constant elements can yield economy. But our confidence in the constancy in our judgments or generalizations rests entirely on the supposition, which in a given case has been substantiated by numerous trials, that our mental adaptation is sufficient. And we must be prepared to find this supposition contradicted at any moment.

Therefore empirical laws as well as theories are provisional in Mach's view, but for different reasons. The empirical generalizations are provisional, because they impute constancies to an infinite number of individual occurrences of sensations while only a limited number have actually been experienced. On the other hand theories postulate things that have never been experienced; no one for example has ever (in Mach's time) actually seen atoms or molecules nor has anyone ever experienced Newtonian absolute space or absolute time. Mach did not seem to find the provisional status of empirical laws to be very disturbing and in fact he considered them to be necessary for science to have its economy. But he considered the provisional status of theories to be an unsatisfactory expediency for science. His theory of scientific criticism includes a phenomenalist criterion that rejects theories. Initially the Logical Positivists who followed Mach were reluctant to accept Hume's skeptical views on scientific criticism, and instead accepted the idea of "verification", the view the scientific laws or empirical generalizations can be established in some permanent sense, an idea that historically had been definitive of truly scientific knowledge. But Carnap and the Logical Positivists moved toward Mach's acceptance of scientific laws as provisionally true instead of permanently true, even as they moved away from his phenomenalism.

Scientific Discovery

Unlike most other philosophers, Mach's concept of scientific discovery does not involve the idea of theory development. In his "The Part Played by Accident in Invention and Discovery" (1895) in his *Popular Scientific Lectures* Mach notes the importance of accident in invention and discovery, but maintains that the inventor is not passive. In fact Mach compares the discoverer to the artist. He says that no man should consider attempting to solve a great problem unless he has thoroughly saturated his mind with the subject and everything else recedes into relative insignificance. Then the discoverer can detect the uncommon features in an accidental occurrence and their determining conditions. Mach believed that it is the idea that dominates the thinking of the inquirer and not vice versa.

The movement of thought obeys the laws of association, and in a mind rich with experience every sensation is connected with so many others that the course of thought is easily influenced by apparently insignificant circumstances, the accidental occurrence of which turn out to be decisive.

Therefore there is a *process* of discovery, and Mach considered how this process could be guided. He explicitly rejected any combinatorial approach as too laborious and extensive. The man of genius in Mach's view consciously or unconsciously pursues systematic methods, and in his deliberate presentiment he omits many alternatives and abandons others after hasty trial, alternatives on which less endowed minds would squander their energies. From the abundance of fancies that a free and active imagination produces, there emerges one particular configuration which fits perfectly with a basic design or idea. Mach does not elaborate further upon this process; and while he believes that it may be guided, he does not propose any consciously repeatable procedure. Perhaps he could go no further in this investigation, because he also believed in *gestalt* qualities and accepted a wholistic view of complexes of sense impressions. In any event his belief that the process can be guided leads him to conclude that genius may be regarded as only a small deviation from the average mental endowment. He states that the way to discovery must be prepared long beforehand, and that in due course the truth will make it appearance inexorable as if by divine necessity. Apparently therefore he rejected the heroic theory of invention.

Mach's History of Mechanics

Mach's most popular work was his *Science of Mechanics: A Critical and Historical Account of Its Development* (1883), also known as *The History of Mechanics*. This book went through nine editions both in German and in English, seven of which were published in Mach's lifetime. The physicists whose works Mach examined were not phenomenalists, and he set out to write a critical history of mechanics from the perspective of his own phenomenalist philosophy of science. As he stated in the introduction to the first edition, the book's purpose is to clarify ideas, reveal the real significance of the matter, and to purge it of its metaphysics. For Mach this agenda amounted to purging physics of theory. With this aim in mind he critiqued the contributors of the past as he salvaged and reconstructed what he found in their works to be of lasting value. Even the achievements of the great Isaac Newton did not escape his phenomenalist criticism unscathed.

Mach criticized Newton's principle of reaction, his concept of mass, and his concepts of absolute space and absolute time. Starting from his own view that all phenomena are related, Mach concluded contrary to Newton that all masses, all velocities, and all forces are relative, a thesis known as Mach's phenomenalistic relativity. And he proposes his own set of definitions and empirical propositions to replace Newton's.   The outcome of this criticism was to have a large impact on the histories of both philosophy of science and physics.

Mach's rejection of theory in physics resulted in several lines of criticism of his philosophical views. One was Duhem's, which is basically philosophical in nature. This line involves a new philosophy of language, and was eventually taken up into Pragmatic philosophy of language of Willard Van Quine, whose philosophy is examined separately. The second line of criticism evolved within physics, and it evolved due to the two great scientific revolutions in physics, the relativity and quantum theories. It was eventually taken up into the Pragmatic philosophy of science of the philosopher Russell Hanson. This line of development is also examined in greater detail separately. Thirdly both Einstein and Heisenberg, who were initially Positivists, were led to reject Positivism by reflection on their own work in physics. Consider firstly Duhem's philosophy of science and his distinctive semantical metatheory of physical theory.

Duhem on Physical Theory and Metaphysics

Duhem was influenced by Mach, and he called his own philosophy of science Positivist. But there were other intellectual influences in his thought, and as a result Duhem differed from Mach in at lease two important respects: firstly Duhem accepted scientific theory as a valid and integral part of science, and secondly he reserved a place in human knowledge for metaphysics. Mach's philosophy is often called "scientistic", by which is meant the view that only science offers valid knowledge and that no nonphenomenalist discourse, which is summarily called "metaphysical", is valid. While Mach was a physicist, philosopher, historian of science, and atheist, Duhem was a physicist, philosopher, historian of science and believing Roman Catholic. Like Mach, Duhem rejected the mechanistic, atomistic physics although for very different reasons than Mach. But unlike Mach, Duhem wished to retain the natural philosophy and cosmology of the

Aristotelian and Scholastic philosophies upon which had been built the theology of his religion since Thomas Aquinas.

　　The outcome of these differences between Mach and Duhem is a complex philosophy of science that affirms and protects the autonomy of physics from any encroachment by metaphysics, while conversely affirming and protecting the autonomy of metaphysics from any encroachment by physics. This mutual isolation of physics and metaphysics is due to Duhem's view that metaphysics, natural philosophy, and cosmology on the one hand pertain to realities that are hidden and that underlie the phenomenal appearances accessible by the senses, while physics on the other hand pertains only to observed phenomena. Furthermore and contrary to Mach, Duhem maintained that theories are integral to physics and are valid science. The only criterion for scientific criticism of a theory, unlike a phenomenal description, is the theory's ability to make predictions that are correct with a sufficient degree of approximation, i.e. correct within the range of indeterminacy produced by a degree of measurement error that always exists in experimental data. Thus when Duhem rejected mechanism, one reason that he gave is that no mechanical atomic theory has been found to be sufficiently accurate, when judged by his purely scientific criterion for the criticism of theories. But his principal reason for saying that the autonomy of physical theory is protected from the metaphysical thesis that physics must be mechanistic, is that physical theory has a special semantics that forbids interpreting the hypothetical postulates realistically, even if a proposed mechanistic hypothesis were scientifically adequate. Physical theory in Duhem's view can never be given a realistic semantics. No metaphysical or cosmological philosophy may be called upon to supply theoretical physics with its axioms. For this reason Duhem denies that physical theory has any explanatory function in science; only metaphysics is able to "explain", and metaphysics has no place in physics. The distinctive semantics of physical theory is a very strategic part of Duhem's philosophy of science. His religious and other intellectual influences may have operated in his developing this distinctive philosophy of science, but his stratifying the semantics of the language of science into the realistic and the nonrealist has as its basis reasons that are entirely integral to his concept of empirical science itself. These reasons are semantical, and must be examined before attempting an exposition of his philosophy of science.

Duhem's Stratified Semantics for Physics

As mentioned above, the second respect in which Duhem differs from Mach is the former's views on physical theory, and the difference is the most distinctive aspect of Duhem's philosophy of science. Mach had rejected theory as "metaphysical", meaning nonphenomenalist, and he maintained that ultimately in the ideal state of science all theory would be eliminated from science. Duhem's alternative view is set forth in his *Aim and Structure of Physical Theory* (1906). In this work as well in other works he not only recognized a valid metaphysics distinct from science, but also considered theory to be characteristic of science in its highest state of development. Over and above the economy that Mach saw in the empirical laws of science, Duhem furthermore saw an additional economy offered by theory. Theory is a hypothetical axiomatized system of equations that orders the multiplicity of experimental laws by means of a symbolic structure, which is not identical with the empirical laws but which "represents" them in a parallel language.

This symbolic structure consisting of the axiomatized mathematical system which constitutes the theory is a distinctive language in science. It is different from all other language of science including the realistic semantics of common discourse, the nonmathematical generalizations of descriptive sciences such as physiology, and the phenomenalist semantics of mathematically expressed empirical laws of science such as Kepler's laws. The language of theory is distinctive from nontheory language, because the nontheory language has a semantics that describes either the phenomenal or real world, while the language of theory does not have these semantics. Instead the semantics of theory language is called "symbolic", which means that its meaning is a sign of the meanings of the nontheory language. Thus the semantics of science in Duhem's philosophy is stratified into two levels, in which one represents the other.

The basis for Duhem's distinguishing the semantics of theory language from that of all other language is the existence of a numerical indeterminacy caused by the fact that measurements, which may occur in the equations of theory, are always approximate. There are two reasons for the indeterminacy between the equations of theory and the nontheoretical language. The first reason is simply the approximate character of all measurements. When measurements are made, a "translation" must also be made from what Duhem called a "practical" fact to a "theoretical" fact. The

practical fact describes the observed phenomena and circumstances of the experiment; the theoretical fact is the set of mathematical data that replaces the practical fact in the equations of the theory. Duhem calls the method of measurement the dictionary that enables the physicist to make this translation.

For any practical fact there is always an infinity of potential theoretical facts, even though the degree of indeterminacy is reduced with improved instruments and measurement procedures. So long as the one or several equations of a theory are correct, the numbers that are the solution set for the equations will fall within the range of measurement indeterminacy. Duhem illustrates the semantical duality caused by this numeric indeterminacy in his discussion of the different meanings of the phrase "free fall." One meaning is contained in a phenomenal description given by any person who knows nothing about physical theory. And a second meaning occurs in the physical theory that includes the idea of uniform acceleration. These are two distinct meanings; the former may be either a realist or phenomenalist meaning, while the latter is called the symbolic meaning. The latter is a sign of the former, so long as the theory is accurate enough to be accepted as true.

However, the numerical indeterminacy that occasions the semantical distinction between practical facts and theoretical facts is not unique to the variables occurring in the equations of theories, the equations that are the conclusions drawn from the hypotheses which are the postulates of the theory. It also occurs in the variables occurring in the equations of empirical laws, the equations that are developed by experimental or other observational judgments. This creates another occasion for numerical indeterminacy, one which exists between the values of the variables in the equations of theory and the values of the corresponding variables in the equations of the empirical laws that a theory orders. Duhem discusses this numerical indeterminacy and the semantical duality to which it gives rise, when he criticizes Newton's claim that his theory of gravitation is not based on hypotheses. The basic question is whether or not Newton's theory was or could be developed empirically by generalizing from Kepler's laws. Duhem argues that Newton had actually created hypotheses, because the mathematical deduction from these hypotheses produces conclusions that formally contradict Kepler's observational laws. In other words the solution set for the empirical law and that for the theory are not the same. But Kepler's laws are approximate, and therefore admit to an infinity of small deviations. The measurements by Tycho Brahe permit the theorist to choose

a variation of Kepler's laws which is also produced by deduction from Newton's theory. Just as there must be a translation from practical facts to theoretical facts resolving the indeterminacy in measurements, so too there must be a translation from empirical laws such as Kepler's laws to "symbolic" laws such as Newton's dynamics. Here again the numeric indeterminacy causes a semantic duality, and a translation is made in which the new symbolic formulas derived from Newton's hypotheses, are substituted for the old realistic formulas, which are Kepler's observational laws.

Having shown that there are different semantics for theory and nontheory language in science, Duhem then gives two ways in which the meanings of the symbols in theory language differ from the meanings in all the other language of science. The first way, which is most important to him, is that the semantics of theory language is neither realistic nor phenomenalist; it does not describe the world of phenomena as does the semantics of empirical laws, nor the real world as does the semantics of common sense discourse. When Duhem states, therefore, that theories represent laws, he means to be taken literally; he means that theories do not represent the world but instead represent the empirical statements which in turn represent the phenomenal world. Thus he cannot be called an instrumentalist in the sense that he denies that theory language has any semantics. He has stratified the semantics of science such that theory has its own higher level semantics.

He also states that when a theory agrees with experimental laws to the degree of approximation corresponding to the measuring procedures employed, and furthermore when the theory predicts the outcome of an experiment before the outcome has occurred, then there is reason to believe that the theory is not merely an economical representation of the experimental laws. Such a theory is also a natural classification of these laws in which the logical order in which the theory organizes the experimental laws is a reflection of the metaphysician's ontological order that underlies the physicist's phenomenal order. However, professionally the physicist cannot pass judgment on this analogical apprehension of the underlying ontological order, because this order is the proper subject only of metaphysics or natural philosophy.

The second way in which the meanings of the symbols in theory language differ from those in the other language of science is that the meanings of theory are determined by their context, by the statements that constitute the theory itself. Therefore, according to whether the physicist

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adopts one or another theory, the variables in the symbolic law change their meaning, so that the law may be accepted by one physicist who admits one theory while it may be rejected by another physicist who admits an alternative theory. Duhem illustrates this contextual determination of meaning in theory language in his discussion of Kepler's observational laws and the symbolic laws of Newton's theory. The formulas that constitute Kepler's laws refer to orbits, but when they are replaced by the symbolic formulas that are deduced from Newton's dynamics, the symbolic law contains variables referring to forces and masses also. The translation from Kepler's laws into symbolic laws presupposes the physicist's prior adherence to the hypotheses of the theory. The contextual determination of the meanings of theories is Duhem's wholistic concept of theory, a concept that is strategic to his views about scientific criticism of theories. With his wholistic view he says theoretical physics is not like a machine but is more like an organism.

Finally it should be noted that although the higher level semantics of theory language is relatively remote from the phenomena described by the semantics of the nontheory language, nevertheless theory is not remote from the experimental situation. He states that an experiment in physics is not simply the observation of a phenomenon, but is furthermore the theoretical interpretation of it. And this theoretical interpretation is not just a technical language, but one that makes possible the use of instruments. He illustrates this distinction between observation and interpretation in physical experiment by offering two descriptions of an experimental apparatus in a laboratory. One description is given in the vocabulary of the physicist who understands the theory of electricity, and the other description is given in the observational language of the observer innocent of such theoretical understanding. The experimenting physicist actually has two distinct representations of the instrument in his mind. One is the phenomenal image of the concrete instrument that he manipulates in reality. The other is a schematic model of the same instrument constructed mentally with the aid of the symbols from the theories that the physicist accepts. Without knowing the theories that the physicist regards as established and that he uses for interpreting the facts he observes, it is impossible for anyone to understand the meaning he gives to his statements. And when a physicist discusses his experiments with another physicist, who accepts an alternative theory, it is necessary for the two physicists to seek to establish a correspondence between their different ideas and then to reinterpret the experiment. Twenty years before the development of the quantum theory Duhem cited as an

example the two alternative theories of light: Newton's emission theory and Frensel's wave theory. He maintained that the observations and experiments interpreted in the concepts of one theory can be translated into the concepts of the other theory. In his philosophy this is possible, not because he anticipated quantum theory, but because he was a Positivist, who believed that the two theories can be related to a common theory-neutral phenomenalist semantics.

Duhem's stratification of the semantics of the language of theoretical science is central and strategic to his philosophy of science. It is not surprising he stated that the approximate fit between measurements and theory creates a semantical difference, although it might seem more correct were he to have said that the resolution of the indeterminacy in measurement by the calculated value for a variable in a theory actually resolves a semantic vagueness instead of saying, as he does, that it creates two distinct meanings. But it is surprising to find him concluding that the distinct meaning of the symbol in the theory is a "sign" of the phenomenal meaning defined by the experimental measurement method. It is this latter position that stratifies the semantics of science, so that theory cannot be given a realistic or phenomenalistic interpretation. Nonetheless Duhem has a reason for taking this position. In his "The Physics of a Believer", an appendix to *Aim and Structure of Physical Theory*, he reports that earlier in his career after attempting unsuccessfully to conform to Newton's methods set forth in the "General Scholium", he concluded that physical theory is neither a metaphysical explanation nor a set of general laws, whose validity is established, but rather that theory is an artificial construction manufactured with the aid of mathematical magnitudes, and that the relations of the magnitudes to the abstract notion emergent from experiment, is that of sign to thing signified. The key concept seems to be the idea of artificial construction. The artificial nature of theory gives it an artificial semantics, and this artificial semantics is of a different kind than the natural semantics of language that describes the phenomenal world.

Throughout most of the history of philosophy, philosophers believed that while the multiplicity of languages argues for the existence of a conventional aspect in human language, still, as Aristotle said, while men speak different languages, they have the same cognitive experiences. This is the thesis of a naturalistic semantics; all men have the same cognitive experience when in the presence of the same reality, because there is a natural relation between knowledge and reality. Mach's theory of sensations and of their identification with elements of the phenomenal world is a

variation of this thesis. But Duhem could not fit this thesis to the language of physical theory, even while he, like Mach, maintained it for the language of observation. He viewed physical theory as so artifactual, that its meanings could not be natural but had to be artificial. Therefore the language of theory does not describe either the real or the phenomenal world, the world of nature. At the same time he was not led to conclude that theory is meaningless. Thus his reconciliation strategy was to make the artificial semantics of theory language describe or represent the language of science, which is not a phenomenon of nature but rather is an artifact.

Duhem's Philosophy of Science

The Aim of Science

Duhem's statement of the aim of science is similar to Mach`s: the aim of science is economy of thought. Like Mach, Duhem believes that experimental laws contribute an intellectual economy, because they summarize a large number of concrete facts. But unlike Mach, Duhem furthermore says that theories also contribute to the realization of the aim of science. The economy achieved by the substitution of a law for concrete facts is redoubled for the mind, when the mind substitutes theories for the numerous experimental laws. A theory is a system of mathematical propositions deduced from a small number of principles, which aim to represent as simply, as completely, and as exactly as possible, a set of experimental laws. Its aim in other words is economy of thought by schematically representing and logically organizing experimental laws.

Scientific Criticism

Duhem developed a sophisticated theory of scientific criticism, and it is central to his philosophy of science. He is very emphatic in defending the autonomy of empirical science from any encroachment by metaphysics or natural philosophy. Metaphysics pertains to realities that underlie the phenomenal appearances that are hidden by the phenomena, while science pertains to these appearances. Consequently whatever may be the criteria and procedures for criticizing a metaphysical thesis, they are not relevant to empirical science. In empirical sciences that are nonmathematical, the generalizations such as "all men are mortal" may be accepted or rejected as simply true or false. But in mathematical physics the equations both of the empirical laws and of the hypothetical theories are not simply regarded as

true or false, but are approximate. The amount of indeterminacy due to the approximate nature of the values of the variables in these equations will be reduced as experimental and measurement techniques improve. These improvements in theory occur because instruments are improved. And because instruments depend on physical theory, the improvement of instruments occurs in turn due to the improvement in theory. As the range of this indeterminacy becomes smaller, the equations of either the empirical laws or the hypothetical theories that represent the laws may no longer be able to predict values for their variables that fall within the smaller range. When this happens, the equations are no longer satisfactory. Duhem is very emphatic in his thesis that the only criterion that may validly operate in scientific criticism is the ability of the law or theory to make accurate predictions. This exclusion of all prior ontological or metaphysical criteria from scientific criticism has been carried forward into the contemporary Pragmatist philosophy of science. It shows up for example as Quine's rejection of all "first philosophy."

In his theory of scientific criticism Duhem rejected the use of so-called crucial experiments as a means of establishing the validity of a theory. His thesis is that if the physicist is confronted with several alternative theories, the rejection of all but one cannot imply the establishment of the remaining one. As an example he cites the two alternative theories of light: one theory is the hypothesis that light is a stream of high speed projectiles, and the other is the hypothesis that light consists of vibrations whose waves are propagated in an ether. This is not an anticipation of the Copenhagen duality thesis; Duhem is thinking of the wave and particle theories as alternative theories. His position is that the choice is not mutually exclusive, because no one can ever enumerate completely all of the various hypotheses, which may pertain to a determinate group of phenomena. He thus maintains that several alternative theories may fall within the range of indeterminacy of the measurement data and experimental laws, so that more than one theory may be satisfactory. This represents a pluralistic thesis about science, and in the crucial experiment discussion, it means that even if all hypotheses could somehow be enumerated, elimination could not leave but one to be considered as established. More generally his pluralism means that the indeterminacy in measurement, laws, and theories produces indeterminacy in scientific criticism. This pluralism is another aspect of his philosophy of physical theory that has been carried forward into the contemporary Pragmatist philosophy of science.

His theory of scientific criticism also reflects his wholistic or organic view of theories. This wholistic view not only makes the meanings of the mathematical symbols mutually determined by the context consisting of the equations of the theory, it also necessitates testing the theory as a whole together with all the hypotheses used in the experiment including assumptions about the measuring instruments. If the prediction in the test is wrong, not only may the proposition being tested be at fault, but also the whole theoretical scaffolding used by the physicist. The physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses. The only thing that the experiment reveals is that among all the theoretical propositions used to predict the phenomenon, there is at least one error. Thus the failure of the prediction does not inform the physicist where the error lies or reveal which hypothesis should be modified. In Duhem's view physics is not like a machine which lets itself be disassembled; the physicist cannot test each piece in isolation and then make adjustments to the isolated part found wanting. Duhem compares physics to an organism in which one part cannot be made to function except when the parts that are most remote from it are called into play. When there is a malfunction felt in the organism, the physician must ferret out through its effects on the entire system, the organ that needs to be remedied or modified without the possibility of isolating the organ and examining it apart. Duhem says that the physicist confronted with a failed prediction is more like a physician than a watchmaker.

Scientific Discovery

Duhem also has a philosophy of scientific discovery. Unlike Mach's view on discovery and invention in science, Duhem's is not principally a theory of perception. He anticipates later philosophers including the Logical Positivists with his emphasis on the language of science. For him scientific discovery is not reduced to noticing what had previously been overlooked in perception; for him discovery is also the construction of theoretical hypotheses. The construction of a theory involves four successive operations: Firstly certain physical properties are taken as simple, so that other things are combinations of these simple properties. These properties are not simple in any absolute sense like Mach's elements, but are taken as simple for purposes of the theory only. The simple properties are measured, and the magnitudes are assigned to symbolic variables. Secondly the magnitudes are connected by propositions that are hypotheses, and that serve as postulates of the deductive system. Thirdly the postulates are not realistic

or phenomenalist, but are freely created; using them requires only that the logic of algebra be correctly applied for making deductions. Fourthly the conclusions drawn from the postulates are compared with the experimental laws that the theory is intended to represent.

If the conclusions agree with the laws within the degree of approximation corresponding to the measurements taken in the experiments, then the theory is said to be a good theory. Such acceptable theory may in turn be used for the further development of measuring instruments used in experiments, as well as constituting the final product of the scientific endeavor with its maximum economy. Improved theory produces improved instruments, which in turn produce better measurements. These better measurements reduce the range of the indeterminacy in the numerical data, which may cause the theories to fail in their predictions. This failure will occasion two types of responses. The initial response is to modify the theory with corrections, which will enable the predictions made with the theory to fall within the smaller range of indeterminacy produced with improved measurements. But these corrections also complicate the theory, and in due course "good sense" may lead some physicists to decide to refrain from adding more complicating corrections, and instead attempt to revise the hypothetical postulates of the symbolic schema, the whole theory itself. The accomplishment of such a revision is the work of the genius. But Duhem does not subscribe to the heroic concept of invention; history creates the genius as much as the genius creates history. The physicist does not choose the hypotheses on which he will build a new theory; the theory germinates within him. This germination is not sufficiently explained by the contemplation of the experimental laws that the theory must represent. It is a larger cultural development. In due course when the cultural process that he calls universal science has prepared minds sufficiently to receive a theory, it arises in a nearly inevitable manner. Often physicists who do not know one another and who are working great distances from one another, generate the same theory at the same time. In the course of his studies the historian of science according to Duhem often observes this simultaneous emergence of the same theory in countries far from one another.

Scientific Explanation

On Duhem's philosophy theories do not explain the laws nor do the laws explain the facts. Explanation is proper only to metaphysics and not to science. In the opening sentence of the introduction to his *Aim and Structure of Physical Theory*, Duhem says that he offers a simple logical

analysis of the method by which physical science makes progress. While affirming the autonomy of physics with his thesis that agreement with experiment is the sole criterion of truth for a physical theory, Duhem has a distinctive concept of scientific progress, which he elaborates in the appendices to the book. He says that there are two types of development in physics that are occurring simultaneously. One is what today would be called the revolutionary type of development consisting of a succession of alternative theories, in which one theory arises, dominates the scene for the moment, and then collapses to be replaced by another theory. The other is an evolutionary progress in which constantly more ample and more precise mathematical representation of the phenomenal world is disclosed by experiment. When the progress of experimental science goes counter to a theory and compels the theory to be modified or transformed, the purely representative part enters nearly whole into the new theory, bringing to it the inheritance of all the valuable possessions of the old theory, while the hypothetical part falls away in order to give way to another theory. The first type is identified with the mechanistic physical systems including Newtonian physics as well as Cartesian and atomic physics. The second type is identified with general thermodynamics, which Duhem believes will lead physical theory toward its goal. He envisioned this goal as the convergence toward an analogy with Aristotle's physics. He concludes in his discussion of the value of theory, that the physicist is compelled to recognize that it would be unreasonable to work for the progress of physical theory, if theory were not the increasingly better defined and more precise reflection of a metaphysics. He thus concludes his book with the thesis that belief in an order transcending physics is the metaphysical justification of physical theory.

Duhem's History of Physics

Just as Mach had written a history of physics viewed through the lenses of his philosophy of science, so too did Duhem. However, Duhem's effort was relatively monumental; it is a work originally intended to be twelve volumes of which ten were actually written before its author's death in September 1916. This *magnum opus* was his *System of the World: A History of Cosmological Doctrines from Plato to Copernicus.* The central thesis of this work is summarized in a much smaller book begun earlier, *To Save the Phenomena: An Essay on the Idea of Physical Theory from Plato to*

Galileo (1908). The thesis is that the hypotheses of physics and especially the heliocentric hypothesis in astronomy are mere mathematical contrivances for the purpose of saving the phenomena.

Pope Urban VIII condemned Galileo in 1633 for maintaining that Copernicus' heliocentric theory is not merely a mathematical contrivance, but is rather a description of the real world. Formerly Cardinal Bellarmine, the Pope maintained that regardless of how numerous and exact may be the confirmations of a theory by experience, these confirmations can never transform a hypothesis into a certain truth that can be taken realistically, since this transformation would require that the experimental facts should contradict any other hypotheses that might be conceived, a requirement that cannot logically be satisfied. Galileo, on the other hand, maintained that because Copernicus's theory saved the phenomena more adequately than any alternative hypothesis, the Copernican theory had to be a realistic one. Contemporary Pragmatists agree with Duhem's rejection of any prior ontological criteria for the criticism of scientific theory, but contrary to Duhem they furthermore agree with Galileo's practice of scientific realism. Contemporary Pragmatists are realists, who let the most empirically adequate theory decide the ontology. Galileo's argument for realism is the same as Quine's doctrine of ontological relativity, and Feyerabend calls it the Galileo-Einstein tradition of realism. And Heisenberg invoked this tradition, when he referenced Einstein's realistic interpretation of relativistic time in the relativity theory, and then used it as a precedent for his own realistic interpretation of the quantum theory's duality thesis, notwithstanding Bohr's instrumentalist complementarity principle. Duhem, however, denied that science is realistic, and he construed Galileo's argument as a case of the fallacy of the crucial experiment. He argued that it is impossible to enunciate all the possible hypotheses, and establish the truth of one by elimination of all others. The accomplishment that Duhem credits to Kepler and Galileo is the rejection of Aristotle's view that celestial and terrestrial physics are fundamentally different, and that hypotheses of physics must save all the phenomena of the inanimate world.

The New Physics vs. the Old Philosophy

The history of philosophy of science has been greatly influenced by the history of physics. As twentieth-century physicists found themselves departing farther and farther from Newtonian physics, they also found

themselves departing farther and farther from the Positivist philosophy notwithstanding the Positivists' criticisms of Newtonian physics. At the beginning of the century Positivism was not merely the academic philosophy it later became. It was for a time the working philosophy for many physicists including those who produced the revolutionary relativity and quantum theories. It achieved ascendancy in academia during the first half of the century, where it evolved into Logical Positivism with the introduction of the symbolic logic, which made it nearly completely irrelevant to the practice of basic research in physics. But long before academia recognized Positivism as a kind of latter-day decadent scholasticism in the second half of the century, it had fallen into disrepute in the eyes of the physicists who encountered its fundamental inadequacy for the new physics.

In his "Autobiographical Notes" in Schilpp's *Albert Einstein* (1949) Einstein stated that Mach's *History of Mechanics* had exercised a profound influence on him when he was a student. He related that all physicists of the last century saw in classical mechanics a firm and final foundation not only for all physics but also for all natural science, and that it was Ernst Mach who with this book shook his dogmatic faith. At sixty-seven years of age, when he was writing these autobiographical notes, Einstein saw Mach's greatness in the latter's incorruptible skepticism and independence, even though Einstein himself had since rejected Mach's philosophy. Einstein was specifically influenced by Mach's critique of the Newtonian concept of absolute space, time and motion, ideas that are also rejected in Einstein's relativity theory. Initially Mach seemed to support Einstein's views. But Mach and Einstein were fundamentally working at cross purposes: Mach attacked the Newtonian concepts of absolute space, time and motion as part of his critique of all theoretical physics, while Einstein discarded these Newtonian ideas as a means for developing a new theoretical physics.

Another influence on Einstein was a thought experiment that Einstein reports he imagined, when he was sixteen years of age. In this thought experiment Einstein wondered what would happen if an observer traveled at the speed of light, riding on a beam of light. The light would then be at rest relative to the rider, but Einstein concluded that the idea of a light beam at rest is self-contradictory. This thought experiment was imagined many years before Einstein was introduced to Mach's book by his friend Besso, while they were students at Zurich, and Einstein reports that it contributed to his forming the idea that the velocity of light in a vacuum is constant in all reference systems. From the Positivist view the constancy of light is no less

objectionably absolute than the concepts of absolute space or time. Mach's phenomenalist relativity states that all sensations are dependent on all other sensations, while Einstein's relativity theory states that the velocity of light in a vacuum is independent of other phenomena.

Throughout Mach's lifetime Einstein continued to view his relativity theory as a continuation of Mach's philosophy, and in his obituary on Mach in 1916 Einstein expressed the opinion that Mach would have come across the theory of relativity, if when Mach was younger the constancy of the velocity of light had been accepted by physicists. In 1921 Mach's son published his father's *Principles of Physical Optics*. The preface of the book is dated July 1913, and in it Mach opposes Einstein's relativity theory and rejects the idea that he was a forerunner of relativity theory. As it happens, in June of 1913 Einstein had sent Mach a preliminary draft of the general theory of relativity, which uses non-Euclidian geometry. But in the 1912 edition of his *Science of Mechanics* Mach had introduced a lengthy footnote (Ch. IV, Sec IV, 9) opposing Minkowski's use of four-dimensional geometry in physics and stating that the space of sight and touch is three-dimensional. It is unlikely, therefore, that Mach was pleased when he received Einstein's 1913 correspondence, and it may have provoked Mach's footnote comments in the 1913 preface to the book on optics. Eventually Einstein accepted the existence of basic differences between his relativity theory and the philosophy of Mach, and he ultimately rejected Mach's philosophy.

Einstein's general theory of relativity departed even further from Mach's philosophy than did the special theory of relativity, because in the general theory it is not possible to restrict the equations to relations among observable magnitudes. But as the theory became accepted among physicists, the Positivists who followed Mach did not want to reject it, and instead they modified their philosophy. These later or "Logical" Positivists, as the Positivists of the Vienna Circle came to be known, replaced Mach's rejection of theories with the less restrictive idea. They said that the language of science may contain theoretical terms referring to nonobservable entities and magnitudes, on condition that statements referring only to observables could logically be related to those that contain these theoretical terms referring to the nonobservable magnitudes or entities. This later Positivist program is considered below in the discussion of the Logical Positivists and particularly of Rudolf Carnap. Mach accepted Einstein's relativity theory, and persuaded Moritz Schlick, founder of the Vienna Circle and successor to the chair of inductive philosophy held by Mach at Vienna, to accept Einstein's theory also. With this acceptance of Einstein's

relativity theory one of the basic theses of the Positivist philosophy was changed.

Positivism was not without some influence on the contributors to the new quantum physics, whose views became known as the "Copenhagen interpretation." Adherents to this interpretation included Niels Bohr, Werner Heisenberg, and Wolfgang Pauli. Its opponents included Albert Einstein, Erwin Schrödinger, Max Planck, Louis de Broglie and David Bohm. The member of Bohr's Institute for Theoretical Physics in Copenhagen, Denmark, who was initially influenced by the Positivist philosophy, was Werner Heisenberg. In his *Physics and Beyond* (1971) Heisenberg relates how Mach's philosophy operated in his own thinking. In the chapter titled "Understanding in Modern Physics (1920-1922)" he described his Positivist views during the years that preceded his development of his matrix mechanics. At that time he believed that true understanding in physics consists in using only language that refers to direct sense perceptions, and that while the ability to make correct predictions is often a consequence of this Positivist kind of understanding, nonetheless making correct predictions is not the same as having true understanding. Because he accepted the Positivist philosophy of science, Heisenberg rejected Bohr's hypothesis of electron orbits, since the orbits are not observable, but unlike Mach he admitted the existence of the electron itself due to the observable tracks produced by the free electron in the Wilson cloud chamber experiments.

In the chapter titled "Quantum Mechanics and a Talk with Einstein (1925-1926)" Heisenberg relates that on the day that he presented his matrix mechanics to the Physics Colloquium at the University of Berlin, Einstein, who was present in the assembly, expressed interest and invited Heisenberg to talk with him at his home that evening. The matrix mechanics does not postulate the existence of electron orbits around the nucleus of the atom, and when Einstein questioned Heisenberg about his Positivistic views that evening, Heisenberg replied that he did not believe that postulates about orbits are appropriate, because the orbits are not observable. Heisenberg affirmed the view that the physicist should consider only observable magnitudes, and for that reason he developed the matrix mechanics, which treats only of the frequencies and amplitudes associated with the lines in the spectrum of the atom. Heisenberg also stated that he was using the same philosophy that Einstein had used, when the latter had rejected the concept of absolute space and time in developing relativity theory. Einstein then replied that he no longer accepted the Positivist view, because it is the physical theory that describes what the physicist can observe. This idea that

theory determines what is observed is philosophically very strategic, because it contradicts the underlying Positivist assumption that there is a dichotomous distinction between the descriptive language about what is observable on the one hand, and the theoretical language about what is not observable on the other hand. When this dichotomy is denied, the Positivist program of building science on firm foundations of observation is rendered untenable.

In the chapter titled "Fresh Fields (1926-1927)" Heisenberg describes the arguments between Niels Bohr and Erwin Schrödinger concerning the issue of the wave verses the particle views in microphysics and of the statistical approach taken by Max Born in 1927. Born maintained that Schrödinger's wave function can be construed as the measure the probability of finding an electron at a given point in space and time. Heisenberg accepted Born's probability interpretation, but there still remained a problem in Heisenberg's mind: Born's interpretation did not explain how the trajectory of an electron in the cloud chamber could be reconciled with the wave mechanics. Trajectories did not figure in the quantum mechanics, and wave mechanics could only be reconciled with the existence of a densely packed beam of matter if the beam spread over areas much larger than the diameter of an electron. With this problem in mind Heisenberg remembered his conversation with Einstein the previous year, specifically Einstein's statement that it is the theory that determines what the physicist can observe. Einstein's discussion with Heisenberg on the day that Heisenberg had first presented his matrix mechanics in 1926 in Berlin led Heisenberg to recognize in 1927, that it was the classical theory that led him to think that the tracks in the Wilson cloud chamber represent the movement of a particle as having a definite position and velocity that defined its trajectory. Recognition of the interpenetrating of theory and observation led Heisenberg to reconsider what is observed in the cloud chamber. He then rephrased his question about trajectories in terms of the quantum theory instead of the classical theory; he asked: Can the quantum mechanics represent the fact that an electron finds itself approximately in a given place and that it moves approximately at a given velocity? In answer to this new question he found that these approximations could be represented mathematically, and he called this mathematical representation the "indeterminacy principle", also known as the "uncertainty relations." On this principle the limit of accuracy with which both position and momentum can be known is defined in terms of Planck's constant. In the view of Heisenberg and those who advocate the "Copenhagen interpretation" this necessary degree of approximation is not

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merely a measurement inaccuracy, but is imposed by the nature of the universal quantum of action. Einstein's semantical principle, that theory decides what the physicist can observe, became one of the corner stones of the post-Positivist philosophy of science as articulated both by Karl Popper and by the contemporary Pragmatists; it led the contemporary philosophers to reject the Positivist separation of theory and observation.

Heisenberg also describes his thought processes in this discovery experience in his chapter on the history of quantum theory in his *Physics and Philosophy* (1958). There he says that he turned around a question: instead of asking how the known formalism of Newtonian physics could be used to express a given experimental situation, he instead asked whether or not only such experimental situations can arise in nature as can be expressed in the mathematical formalism of the matrix mechanics. This recounting of his thinking gives greater emphasis to the ontological commitment that characterizes the "indeterminacy principle", according to which there does not simultaneously exist in reality both a determinate position and a determinate momentum for the electron. As it happens, Einstein was never willing to accept the ontology of the Copenhagen interpretation, even though Heisenberg attempted to do the same thing with his matrix mechanics that Einstein did with the Lorentz transformation, when the latter interpreted the Lorentz equation in terms of actual time instead of apparent time and redefined the concept of simultaneity. Einstein maintained that a more "complete" microphysical theory is needed, which would satisfy his own ontological criteria for physical reality. In imitating Einstein, Heisenberg was practicing scientific realism according to which ontological commitment is extended to the most empirically adequate theory. The Pragmatist philosophy of language implies this practice, in which it might be said that a *carte blanche* metaphysical realism is presumed, while the ontology describing reality is supplied by empirical science; it is a realism which is a blank check for which scientific theory specifies its cash value, and for which empirical criticism backs its negotiability.

Heisenberg did not escape the influence of Positivism, even though he had departed from it in a very fundamental way to develop the indeterminacy relations. Another influence upon his thinking was Bohr's philosophy of knowledge. Bohr did not explicitly embrace Positivism, but in his view classical physics is permanently valid and must serve as the language of observation, in which all accounts of evidence in physical science must be expressed. Heisenberg's attempt to reconcile the influences of Einstein and Bohr resulted in his developing his semantical theory of

"closed-off theories." This is his attempt at a systematic philosophy of language for science. It is different from the Logical Positivist philosophy, but due to Bohr's influence it is more like Positivism than the contemporary Pragmatism. Einstein and Heisenberg had made very insightful criticisms of Positivism, but neither produced a new systematic philosophy of language adequate to their insights, however portentous these insights have turned out to be. The portended Pragmatist philosophy of language and science was as great an intellectual revolution in philosophy as the revolutions in physics which they themselves produced.

Comment and Conclusion

This chapter examined two variations on Positivism formulated by two turn-of-the-nineteenth-century physicists, and previewed the story of Positivism's rejection by the physicists who made the two great scientific revolutions in twentieth-century physics. This story will be given in greater detail below in the chapter describing Heisenberg's philosophy of quantum theory. But to appreciate these developments more adequately, it is helpful firstly to have examined the development of the Pragmatist philosophy of language. Therefore the next chapter describes the extension of Machian Positivism by Carnap in response to Einstein's development of the theory of relativity, and then Quine's critique of Carnap with Duhem's philosophy of physical theory, which Quine transformed into a general philosophy of language, the contemporary Pragmatist philosophy of language.

RUDOLF CARNAP
ON SEMANTICAL SYSTEMS
AND
W.V.O. QUINE'S
PRAGMATIST CRITIQUE

Rudolf Carnap (1891-1970) was a leading member of a group of philosophers and scientists in Vienna, Austria, during the interwar years, which called itself the "Vienna Circle." A statement of the group's manifesto, "The Scientific Conception of the World", written by Otto Neurath with Carnap's collaboration can be found in Neurath's *Empiricism and Sociology*. The group was scattered when the National Socialists came to power in Germany, and although Carnap was a native German citizen, he and several other members of the group migrated to the U.S. With the aid of Willard Van Quine of Harvard University, Carnap received an appointment to the faculty of philosophy at the University of Chicago in 1935, which he retained until 1952 when he spent two years at the Institute for Advanced Study at Princeton. In 1954 he filled the vacancy created by the death of Hans Reichenbach at the University of California at Los Angeles, and held the position until his retirement from teaching in 1961. However, he continued to write for the ten years of his intellectually active retirement. He died in 1970 and is memorialized in *Boston Studies in the Philosophy of Science* (1971).

Logical Constructionalism

In his "Intellectual Autobiography" published in *The Philosophy of Rudolf Carnap* (ed. Schilpp, 1963) Carnap reports that while he was studying at the University of Jena during the years just before the First World War, he was greatly influenced by one of his teachers, Gottlob Frege,

who maintained that logic should be the foundation for mathematics. Shortly after the war Carnap read Bertrand Russell's *Principia Mathematica*, and was greatly impressed by Russell's theory of relations. But Carnap was even more impressed by Russell's philosophical outlook expressed in *Our Knowledge of the External World*. This book states that the logical-analytical method can provide a method of research in philosophy, just as mathematics supplies the method of research in physics. Carnap reports that upon reading this text he felt that its words had been directed to him personally. As a result of these influences, the construction of logical systems would characterize all of Carnap's philosophical work during his long career. There would be many other influences, but they would only produce variations on his basic agenda of logical constructionalism.

Carnap's philosophy of science was Positivist, and he and the other members of the Vienna Circle were favorably disposed to the philosophies of Mach, Poincare, and Duhem. The antimetaphysical and scientistic character of Mach's philosophy was reinforced by the early writings of Ludwig Wittgenstein. Wittgenstein maintained that all philosophical sentences including most notably all of metaphysics are pseudo sentences, and that in spite of their grammaticalness and common usage, these pseudo sentences are really devoid of any cognitive content. Later Wittgenstein departed from this view and moved away from the constructionalist approach in philosophy. But the earlier views of Wittgenstein expressed in his *Tractatus Logico-Philosophicus* had a lasting influence on the Vienna Circle Positivists. One of the central philosophical tasks that they set for themselves was the use of logical constructionalist methods to implement the Positivist philosophy, and especially the symbolic logic in the *Principia Mathematica* of Russell and Whitehead, and for this reason they are known as the "Logical" Positivists.

Einstein and Mathematical vs. Physical Geometry

Like many philosophers of his generation, Carnap was impressed by Einstein's revolutionary theory of relativity. Philosophers such as Popper found the significance of this successful overthrow of the three-hundred-year reign of Newtonian physics in its implications for scientific criticism. But Carnap found its significance in the distinction between mathematical and physical geometry, or more generally in the role of mathematics as the logic for the physical theory. The central role in the relationship between the formal and the empirical in the development of modern physics became

the axis for Carnap's whole philosophical career. He made it the subject of a distinctive type of metatheory for science, which evolved into his metatheory of semantical systems.

Carnap had started his studies in experimental physics at the University of Jena before the First World War, and then later turned to philosophy after the war. In 1921 he wrote a Ph.D. dissertation titled *Der Raum*, in which he attempted to demonstrate that contradictory theories about the nature of space maintained by the mathematicians, philosophers and physicists, are entirely different subjects. He distinguished three meanings of the term "space" corresponding to the three disciplines that treat it. These are the formal meaning used by mathematicians, the intuitive meaning used by philosophers, and the physical meaning used by physicists. The intuitive meaning used by philosophers is based on the Kantian idea of "pure intuition"; Carnap later rejected this idea and retained only the formal and empirical meanings. A later development in Carnap's thinking on these matters occurred when he read Wittgenstein's *Tractatus*. Wittgenstein had defined formal meaning in terms of tautologies or logical truth. This was the origin of Carnap's thesis of analyticity, and he believed that the concept of logical truth supplied the key to the problem of formal systems such as mathematical geometry, which had enabled Einstein to make his revolutionary relativity physics. In his autobiography Carnap says that due to the doctrine of logical truth, Wittgenstein had the greatest influence on his thinking besides Russell and Frege.

After many years of silence on the subject of geometry, Carnap returned to it in his *Philosophical Foundations of Physics* (1966). There he says that he views the Euclidian, the Lobachevskian, and the Riemannian geometries as different languages in the sense of theories of logical structure, which as such are concerned only with the logical implications of axioms. In this work he references Einstein's *Sidelights on Relativity* (1921; English, 1923) where Einstein says that the theorems of mathematics are certain in so far as they are not about reality, and that in so far as they are about reality they are uncertain. Carnap states that the philosophical significance of Einstein's theory of relativity is that it made clear that if geometry is taken in an *a priori* or analytic sense, then like all logical truths it tells us nothing about reality, while physical geometry is *a posteriori* and empirical, and describes physical space and time.

Carnap notes that in relativity theory Einstein used the Riemannian mathematical geometry as the axiomatic system for his physical geometry, but the reason for the choice of which mathematical geometry to use for a

physical theory is not obvious. Several years before Einstein developed his relativity theory the mathematician Poincare postulated a non-Euclidian physical space, and said that physicists have two choices. They can either accept non-Euclidian geometry as a description of physical space, or they can preserve Euclidian geometry for the description of physical space by adopting new physical laws stating that all solid bodies undergo certain contractions and expansions, and that light does not travel in straight lines. Poincare maintained that physicists would always choose to preserve the Euclidian description of physical space, and would claim that any observed non-Euclidian deviations are due to the expansion or contraction of measurement rods and to the deflection of light rays used for measurement. Einstein's choice of the Riemannian geometry and physical laws for measurement was based on the resulting simplicity of the total system of physics. Relativity theory using Riemannian geometry greatly simplifies physical laws by means of geodesics, such that gravitation as a force is replaced by gravitation as a geometrical structure.

The *Aufbau* and "Rational Reconstruction"

In 1928 Carnap published his *Der Logische Aufbau der Welt*. The book was translated in 1967 with the title *The Logical Construction of the World*, but in the literature the book is always referred to as the *Aufbau*. This work exhibits a detailed design for an ambitious investigation. In the first three of the book's five parts Carnap sets forth the objective, plan, and essentials of this investigation. His objective is the "rational reconstruction" of the concepts of all fields of knowledge on the basis of certain elementary concepts, that describe the immediately given in experience. His phrase "rational reconstruction" means the development of explicit definitions for concepts that originate in the more or less unreflected and spontaneous psychological processes of cognition. The task is not a work in psychology; it is a work in logic. It yields a constructional system, which Carnap states is more than merely a division of concepts into various kinds and an integration of the relations among them. It is furthermore a step-by-step logical derivation or "construction" of all concepts from certain fundamental concepts. The result is a genealogy of concepts, in which each concept has a definite place, because at each level concepts are constructed from others at a lower level, until one reaches the basis of the system consisting of basic concepts. And the logical construction is implemented by means of the theory of relations in Whitehead and Russell's symbolic logic, or "logistic."

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The selected basic elements are "elementary experiences", which are unanalyzable, and the basis contains one basic relation, which takes the elementary experiences as arguments. The basic relation is "recollection of similarity", which in the logic is symbolized as $x\ Rs\ y$. This symbolism means: x and y are elementary experiences, which are recognized as partly similar through the comparison of a memory image of x with y. Carnap illustrates his system in the fourth part of the *Aufbau*, and develops various constructions for concepts such as quality classes, sensations, the visual field, colors, color solids, the space-time world, tactile-visual things, and "my body."

The fifth and concluding section of the book Carnap sets forth his explicit statement of the aim of science. He views the aim of science in terms of his rational-reconstruction and unity-of-science agendas. He says that the formulation of the constructional system is logically the *first* aim of science. From a purely logical point of view statements made about an object become statements in the strictest scientific sense only after the object has been constructed from the basic concepts. Only the constructional formula in the Russellian logistic - as a rule of translation of statements about an object into statements about the basic objects consisting of the relations between elementary experiences - gives a verifiable meaning to such statements, because verification means testing on the basis of experience. The *second* aim in turn is the investigation of the nonconstructional properties and relations of the objects. The first aim is reached by convention; the second aim is reached through experience. Carnap adds that in the actual process of science these two aims are almost always connected, and that it is seldom possible to make a selection of those properties that are most useful for the constructional definition of an object, until a large number of properties of the object are known. Carnap illustrates the relation between the two aims of science with an analogy: the construction of an object is analogous to the indication of the geographical coordinates for a place on the surface of the earth. The place is uniquely determined through the coordinates, so that any other questions about the nature of the place have definite meaning. The first aim of science locates experience, as does the coordinate system; the second aim addresses all other questions through experience, and is a process that can never be completed. Carnap says that there is no limit to science, because there is no question that is unanswerable in principle. Every question consists of putting forth a statement whose truth or falsity is to be ascertained. However, each statement can in principle be translated into a statement about the basic

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relation and the elementary experiences, and such a statement can in principle be verified by confrontation with the given. Fifty years later Quine also uses the coordinate system analogy to express his thesis of ontological relativity. But instead of developing an absolute ontology consisting ultimately of the immediately given in terms of elementary experiences and a basic relation, Quine relativizes ontology to one's "web of beliefs" including science, and ultimately by nonreductionist connection to one's own "home" or native language. The Vienna Circle's unity-of-science agenda is integral to Carnap's view of the aim of science. He sees the task of unified science as the formulation of the constructional system as a whole. By placing the objects of science in one united constructional system, the different "sciences" are thereby recognized as branches of one science.

Carnap's idea of rational reconstruction is different from the views of some contemporary information scientists, who propose that their procedural reconstructions of historic scientific discoveries with computerized artificial-intelligence discovery systems are hypotheses in "cognitive psychology", also known as "cognitive science." However, such efforts can be recast into a linguistic analysis that is more familiar to philosophers and also more like Carnap's procedural approach than a psychological investigation.

Logical Syntax of Language

When Carnap discovered *Gestalt* psychology, he reconsidered the phenomenalist constructionalism that he had undertaken in his *Aufbau*, and concluded that a physicalist language, a "thing language" describing things in ordinary experience, is more suitable as a basis of all scientific concepts. At about the same time he also learned of Hilbert's metamathematics program. The influence of Russell had led the Vienna Circle to prefer the logistic program of the foundations of mathematics to Hilbert's formalist approach. But Carnap was attracted to the idea of a metalanguage, not just for mathematics but for a logic of all science. This was his idea of a "metalogic", which he developed in his *Logical Syntax of Language* (1934). The metalogic is the logical syntax of language viewed as a purely analytic theory of the structure of its expressions. In his autobiography he reports that the whole theory of language structure and its possible applications in philosophy came to him like a vision during a sleepless night when he was ill in January 1931, and that on the following day he wrote down the idea in a manuscript of forty pages titled *Attempt at a Metalogic*, which was the first draft of his *Logical Syntax*.

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One of the central ideas in this book is his distinction between metalanguage and object language. The former contains no reference to the meanings of linguistic signs occurring in the object language; it refers only to the logical structure of the expressions in the object language. Carnap says that his chief motivation for developing this syntactical method was to formulate more precisely philosophical problems that have evaded resolution when expressed in traditional manner. In 1934 he published "On the Character of Philosophical Problems" in the American journal *Philosophy of Science*, which expounded his treatment of metaphysical issues in the German edition of *Logical Syntax* published in the same year. In this work he distinguishes the formal or syntactical perspective from the connotative or material perspective. He identifies logic as a set of metalinguistic transformation rules, and the logic of the language of science, which is the object language, as one in which logical entailment is a formal transformation rule. Thus Carnap defines the "content" of a proposition in science as a class of entailments from a synthetic proposition in the science. Content is thus a purely formal concept, and the difference between the formal and material perspectives is merely a difference between modes of expression. Accordingly philosophical analysis consists of translating statements into the formal mode. Meaningful statements in science can be translated into the formal mode of speech, but the meaningless metaphysical statements cannot be translated into the formal mode. For this reason he maintained that differences between Positivists and realists disappear, when their respective positions are translated into the formal mode. Similarly problems in the foundation of physics are also problems in syntax. For example verification of physical laws is a matter concerning the syntactic deductive coherence between the general law-like propositions and singular propositions called protocol sentences, and the problem of induction is a question of how transformation rules lead from protocol sentences to laws.

In 1937 Carnap published his English edition of *Logical Syntax*. This latter edition contains additional material not in the earlier German edition, and its bibliography includes reference to Quine's "Truth by Convention" published in 1936, in which Quine rejected the idea of analytic truth. Quine viewed the thesis of analytical truth as the Achilles heel of Carnap's philosophy of science, its parallel postulate to be replaced with the new Pragmatist philosophy of language. *Logical Syntax* is divided into five parts. The first three set forth two artificial object languages. Language I is designed to be acceptable to philosophers persuaded of the intuitionist philosophy of mathematics, because it includes no infinities. Language II is

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adequate to all classical mathematics including what the intuitionists would not accept, and it includes Language I as a sublanguage. The fourth part sets forth the general procedures for constructing any artificial language, and is titled "General Syntax." Carnap defines general syntax as a system of definitions of syntactical terms. In general a language is any sort of calculus in the sense of a system of formation and transformation rules concerning expressions, which in turn are defined as finite, ordered series of elements called symbols. Formation rules determine concatenations of symbolic elements to form expressions, and transformation rules determine what transformations produce valid deductions and proofs. The interpretation of a language is the method of learning by explicit statements that are translations from an already interpreted language, and therefore can be formally represented and belongs to syntax. A system of axioms in a calculus may firstly be given, and then interpreted in various ways by translations that establish correlations between the expressions of the language being interpreted and those already interpreted.

The fifth and concluding part of the book pertains to philosophy and syntax, where philosophy is identified with the logic of science. The material for the 1934 article in *Philosophy of Science* was taken from section A of this part. In section B Carnap considers the logic of science as syntax, stating that the logical analysis of physics is the syntax of the physical language. The language must have formation rules both for the protocol sentences, which express observations, and for the postulated or "P-primitive" laws, which have the form of universal sentences of implication and equivalence. The transformation rules of the physical language consist either of only "L-rules", which are logical rules, or of the L-rules together with "P-rules", which are empirical rules. A sentence in physics is tested by deducing consequences using the transformation rules, until finally sentences in the form of protocol sentences are generated. These deduced protocol sentences are then compared with the protocol sentences that are observation reports, and the former are either confirmed or refuted by the latter. If a sentence which is an L-consequence of certain P-primitive sentences, contradicts a sentence which has been stated as a protocol sentence, then some change must be made in the system. But there are no established rules for the kind of change that must or must not be made, nor is it possible to set down any sort of rules as to how new primitive laws are to be established on the basis of actually stated protocol sentences. There are no rules for induction due to the universality of laws; the laws are hypotheses in relation to protocol sentences. Furthermore not only general

laws, but also singular sentences are formulated as hypotheses, i.e. as P-primitive sentences, which are sentences about unobserved processes from which certain observed processes can be obtained.

Carnap also treats the topic of scientific criticism, and maintains that there is no complete falsification or confirmation of any hypothesis. When an increasing number of L-consequences of the hypothesis agree with previously acknowledged protocol sentences, then the hypothesis is increasingly confirmed, but it is never finally confirmed. He states that it is impossible to test even a single hypothetical sentence, because the test applies not to a single hypothesis but also to a whole system of physics as a system of hypotheses. In this context Carnap references Duhem and Poincare. He also says that both P-rules and L-rules including those of mathematics are laid down with the reservation that they may be altered as soon as it seems expedient to do so, and that in this respect P-rules and L-rules differ only in degree with some more difficult to renounce than others.

Carnap's thesis that logical and descriptive language differs only in degree was proposed by Alfred Tarski. Carnap explains that if every new protocol sentence introduced into a language is synthetic, then L-valid (i.e. analytic) sentences differ from synthetic sentences, because such a new protocol sentence can be incompatible only with the P-valid synthetic sentence; it cannot be incompatible with the logical L-valid or analytic sentence. But then he further goes on to say that in spite of the above fact, it may come about that under the inducement of new protocol sentences the language may be altered to such an extent that the L-valid or analytic sentence is no longer analytic. He emphasizes in italics that the construction of the physical system is not effected in accordance with fixed rules, but is a product of convention. These conventions are not arbitrary; they must be tested. The choice of convention is influenced firstly by practical considerations such as simplicity, expediency, and fruitfulness, and secondly by their compatibility with the total system of hypotheses to which the already recognized protocol sentences belong. Thus in spite of the subordination of hypotheses to empirical control by means of protocol sentences, hypotheses contain a conventional element, because the system of hypotheses is never "univocally" determined by empirical material however rich it may be. Carnap never developed this thesis of the empirical underdetermination of a system of hypotheses, and the artifactual theory of language it implies, which was extensively developed by Quine in the 1950's and afterward. Later Carnap rejected Tarski's thesis that logic and

descriptive language differ only in degree, but he always maintained that definitions of L-true sentences are relative to the specific language system under construction.

Semantical Systems: Definitions and Characteristics

Carnap's mature work in semantics is his *Introduction to Semantics* (1943). When he had written his *Logical Syntax* he had believed that metalogic should deal only with the form of expressions of the object language, and that no reference should be made to the meanings of the signs and expressions. In the preface to his *Introduction to Semantics* Carnap states that Tarski was the first to call his attention to the fact that the formal methods of syntax must be supplemented by semantical concepts, and also that these semantical concepts can be defined by means no less exact than those of syntax. He says that his *Introduction to Semantics* owes more to Tarski than to any other single influence, although he also notes that he and Tarski are not in complete agreement on the distinction between syntax and semantics, and on the distinction between logical and descriptive signs. In this new semantical perspective semantical systems were central to his philosophy for the remainder of his life. It is a concept that is fundamental to his views in philosophy of science, his philosophy of probability, and his philosophy of information theory.

Following the Pragmatist tradition, to which he had been introduced by Charles W. Morris in the United States, Carnap describes semiotics as the general theory of signs, which is divided into three parts based on the three factors involved in language. These factors are (1) the expression, (2) the *designatum*, and (3) the speaker. The part of semiotics that deals with all three of these factors is called pragmatics. The second part of semiotics, called semantics, abstracts from the speaker, and contains a theory of the meaning of expressions, which leads to the construction of a dictionary for translating the object language into the metalanguage. Finally the third part of semiotics is called syntax, which abstracts from both the speaker and the *designata* of the signs, in order to consider only the expressions. Carnap further distinguishes between descriptive semantics and syntactics on the one hand, and pure semantics and syntactics on the other. The former are included in pragmatics because they are empirical, while the latter are not because they are analytic. In pure semantics and syntactics the philosopher lays down definitions for certain concepts in the form of rules, and he

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studies the analytic consequences of these definitions. Nearly all of Carnap's work is in pure semantics and pure syntactics, and the terms "semantics" and "syntactics" mean pure semantics and pure syntactics in his texts, unless otherwise noted; Carnap's interest is typically more in constructional systems than in empirical linguistics.

A semantical system presupposes a syntactical system. A syntactical system or calculus, denoted *K*, consists of rules that define syntactical concepts, such as "sentence in *K*" and "provable in *K*." The smallest unit of syntax in the system is called a "sign." Signs are combined into "expressions" according to the formation rules for the calculus. The most important type of expression is the "sentence." Sentences are derivable from other sentences, i.e. are "proved", in accordance with the transformation rules of the calculus. Transformation rules are also called the system's "logic", and for purposes of illustration Carnap typically utilizes Russell's first-order predicate calculus. All the rules of the syntactical system are analytical rules, and are expressed in a metalanguage; the defined language system is the object language.

Carnap defines a semantical system as a system of rules formulated in a metalanguage and referring to an object language, which rules determine a truth condition for every sentence of the language, i.e. a sufficient and necessary condition for each sentence's truth. The semantical system supplies an interpretation of the sentences of the syntactical system or calculus, because to understand a sentence is the same as to know under what conditions it would be true. It may be noted that truth conditions are not truth values. The semantical rules do not determine whether or not a sentence is true; the truth value of the sentence must be determined empirically. The truth condition need not be satisfied for the semantical rule to state it. As a set of definitions, a semantical system denoted *S* must set forth certain things. It must define:

1. the classifications of the signs in *S,*
2. the classifications of the expressions in *S*, such as "term in *S*" and "sentence in *S*",
3. the meaning of "designation in *S*", and
4. the meaning of "true in *S*."

These definitions may be enumerations or they may be recursive definitions. The meanings of expressions that are smaller than sentences are given by statements of designation. For example the rule for designation for

predicates may include " '*H*' denotes the property human." The meanings of sentences are given by statements of truth conditions called Tarski sentences, such as " 'The moon is round', if and only if the moon is round." The sentence in double quotes is in the metalanguage consisting of English, and the symbol or clause in the single quotes is an expression in the object language. The truth condition statement could also be " 'The moon is round' is true, if and only if the moon is round", since to assert that a sentence is true with the predicate "is true" is to assert the sentence. These statements in the metalanguage are called "radical" concepts for the semantical system.

In the *Introduction to Semantics Carnap* describes L-semantics, which consists of L-concepts. In L-semantics an L-term applies whenever the term "true" applies on the basis of merely logical reasons in contrast to factual reasons. This truth is called L-truth or logical truth. The L-concepts are the same as those occurring in syntax, and Carnap states that logic is part of semantics even though it may also be dealt with in syntax. Corresponding to the L-concepts in semantics, there are identical C-concepts in syntax. The relation between syntax and semantics is such that the sentences of a calculus denoted *K* are interpreted by the truth conditions stated in the analytic semantical rules of the semantical system, denoted *S,* provided that *S* contains all the sentences of *K*. However, not all possible interpretations of the calculus *K* are true interpretations. A semantical system *S* is a true interpretation of *K*, if the C-concepts of *K* are in agreement with the corresponding radical concepts in *S*. Furthermore not all true interpretations of the calculus *K* are L-true. The semantical system *S* is called an L-true interpretation for the calculus *K*, if the C-concepts in *K* are in agreement with the L-concepts in *S*.

Later in his *Meaning and Necessity* (1947) Carnap develops a definition of L-truth in terms of his concept of state description. A state description in a semantical system denoted *S*, is a class of sentences in *S* which contains for every atomic sentence either the sentence or its negation but not both. Such a sentence is called a state description, because it gives a complete description of a possible state of the universe of individuals with respect to all the properties and relations expressed by the predicates of the system. It thus represents one of Leibniz's possible worlds or Wittgenstein's possible states of affairs. To say that a sentence holds in a state description means that it would be true if the state description were true, i.e. if all the atomic sentences belong to it were true. Thus the L-concepts are precisely those that are true in all state descriptions, because they are true in all

possible worlds, even though there is only one factually true state description.

Carnap further elaborates on L-truth in his "Meaning Postulates" (1952) reprinted in the appendix of the 1956 edition of *Meaning and Necessity*. His theory of L-truth and state descriptions initially applied to cases where the logically true statement is true only by virtue of the meanings of the logical terms in the statements, as in "Every *x* is either *P* or not *P*." But there are also cases such as "If *x* is a bachelor, then *x* is not married", which are true by virtue of the meanings of the descriptive terms. Meaning postulates are object-langauge sentences introduced into a semantical system, that define the relations among descriptive terms in the sentence in addition to the meanings assigned by rules of designation expressed in the metalanguage. These meaning postulates are not said to be factually true by virtue of empirical investigation, but are true by a decision of the architect of the semantical system, who uses them as semantical rules. Carnap then introduces a modification of his concept of state description to include another kind of statement, that is the conjunction of all meaning postulates in the semantical system. Then he says that a sentence in a given semantical system is L-true, if it is L-implied by this conjunction of meaning postulates. This expanded notion of L-truth with meaning postulates is Carnap's explication of analyticity, by which is meant statements whose truth is known by reference to either the logical form or to the descriptive terms in the statement. Later he refers to this expanded idea of L-truth as A-truth.

Using his concept of state description Carnap defines the concept of ranges: the range of a sentence is the class of all state descriptions in which a sentence holds. Rules of ranges in turn determine the range of any sentence in the semantical system *S*. These rules are semantical rules that determine for every sentence in *S*, whether or not the sentence holds in a given state description. By determining the ranges, these rules together with the rules of designation for the component predicates and individual variables give an interpretation for all the sentences in *S*. This amounts to saying that to know the meaning of a sentence is to know in which of the possible cases it would be true. Carnap thus describes a semantical system in terms of four types of semantical rules: (1) rules of formation for sentences, (2) rules of designation for descriptive constants, (3) rules of truth, (4) rules of ranges.

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Semantical Systems: Ontological vs. Linguistic Issues

Meaning and Necessity has a more specific purpose than the earlier *Introduction to Semantics.* The former is the development of a new method of semantical analysis, which Carnap calls the method of extensions and intensions, and which is based on the customary concepts of class and property respectively. Carnap maintains that these concepts of extension and intension should be substituted for the idea of naming of an abstract entity. In his autobiography he notes that some philosophers [who happen to include Quine and Goodman] reject this way of speaking as the "hypostatization of entities." In their view it is either meaningless or at least in need of proof, to say that such entities as classes and properties actually exist. But Carnap argues that such terms have long been used in the language of empirical science and mathematics, and that therefore very strong reasons must be offered, if such terms as "class" and "property" are to be condemned as incompatible with empiricism or as unscientific. He says furthermore that to label the use of such terms as "Platonistic" or as "Platonistic realism", as is done by these philosophers, is misleading, because these labels neglect the fundamental distinction between, say, physical laws containing real number variables, and ontological theses affirming or denying the reality of universals. Carnap dislikes the term "ontology", and he maintains that the issue between nominalists and realists regarding universals is a pseudo problem, which is devoid of cognitive content.

Carnap says his method of extension and intension is a superior basis for semantical analysis than an alternative method based on the naming relation, because the latter leads to contradictions, when the names are interchanged with one another in true sentences. He thus refers to the "antinomy of the name relation", which is due to the fact that a predicate viewed as a name is ambiguous, since it can refer either to a class or to a property. Some systems avoid this ambiguity by rejecting properties, and Carnap rejects this loss. Others avoid the antinomy by having different names for properties and their corresponding classes, thus resulting in a higher degree of duplication of expressions. In Carnap's method of extension and intension the expressions for properties and for their corresponding classes have the same intension and extension. Thus both classes and properties are admitted without the inelegant duplication and without the antinomy; only one predicate is needed to speak about both a certain property and about its corresponding class.

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The antinomy can be avoided by Carnap's method of prescribing the principle of interchangeability for expressions with the same extension, which is distinctive of extensional contexts. This prescription is achieved by means of the L-equivalence relation, such that extensions are defined in terms of intensions. The extension of a given intension is defined as the one L-determinate extension that is equivalent to the given intension. Extensions are thus reduced to intensions. The result is what Carnap calls a "neutral metalanguage." While the metalanguage for an object language based on the name relation will contain such terms as "the class human" and "the property human", the neutral metalanguage for an object language based on the method of extension and intension contains only the neutral expression "human."

In "Meaning and Synonymy in Natural Language" (1955) also reprinted in the appendix to the 1956 edition of *Meaning and Necessity* Carnap describes how his method of extension and intension is applicable in pragmatics as well as in pure semantics. "Pragmatic" in Carnap's lexicon means empirical linguistics. The purpose of this paper is to give a procedure for determining intension in natural language. This procedure is problematic, because unlike the construction of an artificial language, in which extension can be defined on the basis of intensions, the empirical investigation of an unknown natural language by the field linguist must begin with the identification of extensions that is not problematic. On the basis of either spontaneous or elicited utterances of a native speaker of the unknown natural language, the field linguist can ascertain whether or not the native is willing to apply a given predicate to a thing. By such investigation the linguist determines firstly the extension of the predicate, the class of things to which the native is willing to apply the predicate, secondly the extension of the contradictory class of things to which the native will not apply the predicate, and thirdly the class of things for which the native will neither affirm nor deny the applicability of the predicate. The size of the third class indicates what Carnap calls the degree of extensional vagueness of the predicate. Carnap admits that this determination of extension involves uncertainty and possible error, either due to a failure to recognize an individual case or due to a failure to make the correct inductive inference to the intended thing. But he says that these hazards apply to all concepts in science, and they offer no reason to reject the concepts of the theory of extension.

Carnap's thesis is that the analysis of intension for natural language is a scientific procedure, which is methodologically just as sound as the field

linguist's method of determining extension. And he notes his disagreement with Quine about this thesis. Carnap postulates the case in which two linguists agree on the extension of a native's use of a predicate, but not on the intension. Carnap maintains that in pragmatics the assignment of an intension is an empirical hypothesis, which like any other hypothesis can be tested by observation of linguistic behavior. In the empirical investigation of the native speaker's linguistic behavior, the linguist looks for what Carnap calls intensional vagueness. Extensional and intensional vagueness are related such that a decrease in one produces a decrease in the latter. This search is directed to finding out what variations of a given specimen are admitted within the range of the predicate, where "range" in the context of a discussion of natural languages means those possible kinds of objects for which the predicate holds. These are cases for which the native has never made a decision about the applicability of the predicate under investigation. The description of these possible cases is the intensional vagueness of the predicate. The linguist can therefore describe to the native speaker various imaginary cases, until he hits upon one that differentiates the otherwise co-extensive predicates. Carnap adds that rules of intension are necessary for the language of empirical science, because without them intensional vagueness would remain, and therefore prevent mutual understanding and communication. Carnap apparently believes that all vagueness can be removed from a predicate, when the predicate is taken from everyday discourse into scientific language. Carnap also elaborates his discussion to include intension for a robot. He maintains that from a logical point of view the pragmatical concept for a robot is the same as that for a human. If the internal structure of the robot is not known, however, the same empirical method that is used to determine intension for a human speaker can be used for a robot. In both cases the intension for a predicate for a speaker is the general condition that an object must satisfy for the speaker to apply the predicate to it. And if the intensional structure of the robot is known, the intension of a predicate can be known even more completely.

In his "Empiricism, Semantics and Ontology" (1950) also in *Meaning And Necessity* (1956) Carnap deals further with the problem of classes and properties, which some philosophers such as Quine refer to as abstract "entities.” Again he notes that in the language of physics it is hardly possible to avoid abstract entities, and that using a language referring to them does not imply embracing a Platonistic ontology. He views such language as perfectly compatible both with empiricism and with strictly scientific thinking. In this paper he explains further why this compatibility

is possible. Firstly he notes that there are two kinds of questions concerning the existence or reality of entities. One kind is addressed by creating a system of new ways of speaking, which system is subject to new rules in the construction of a linguistic "framework", i.e. a whole semantical system, for the new entities in question. This first kind of question pertains to the existence of the entities referenced by the system as a whole, and Carnap calls these "external" questions. The other kind of question is appropriately called an "internal" question, since it pertains to the existence of a new kind of entity within the framework. Internal questions can be resolved by either logical or empirical scientific procedures. The question of the reality of a kind of entity described by a theoretical term might serve as an example of an internal question. The problem of abstract entities, however, is an external question, and it is this latter type of question that concerns Carnap in this paper. Carnap maintains that the introduction of a new language framework with its new linguistic forms does not imply any assertion of reality, but rather is merely a new way of speaking. Therefore, the acceptance of a linguistic framework containing terms referring to abstract entities does not amount to the acceptance of Platonism, because the new language framework is not a new metaphysical doctrine. Carnap then invokes his "principle of tolerance", which he had firstly expressed in his *Logical Syntax* many years earlier. The criterion he invokes as a semanticist is not an ontological one, but rather is a pragmatical one. The relevant criterion is whether abstract linguistic forms of variables are expedient or fruitful for the purposes for which the semantical analysis is designed, such as the clarification or construction of languages for the purpose of communication, and especially for communication in science.

Semantical Systems: Physics and the Reduction of Theories

Even before Carnap had published his *Introduction to Semantics*, he had formulated his concept of science as a semantical system, and this concept did not change fundamentally for the duration of his contributing career. The early statements of this concept are set forth in his "Logical Foundations of the Unity of Science" and "Foundations of Logic and Mathematics" in the *International Encyclopedia of Unified Science* (1938). In these works he asserts that philosophy of science is not the study of the activities of scientists, i.e. the pragmatics of science, but rather is the study of the results of the activity, namely the resulting linguistic expressions,

which constitute semantical systems. More specifically the philosopher treats the language of science as an object language, and develops a metatheory about the semantics and syntax of this object language. The metatheory is expressed in a metalanguage.

A physical theory is an interpreted semantical system. Procedurally a calculus is firstly constructed, and then semantical rules are laid down to give the calculus factual content. The resulting physical calculus will usually presuppose a logical mathematical calculus as its basis, to which there are added the primitive signs which are descriptive terms, and the axioms which are the specific primitive sentences of the physical calculus in question. For example a calculus of mechanics of mass points can be constructed with the fundamental laws of mechanics taken as axioms. Semantical rules are laid down stating that the primitive signs designate the class of material particles, the three spatial coordinates of a particle x at time t, the mass of a particle x, and the class of forces acting on a particle x or on a space s at time t. Thus by semantical interpretation the theorems of the calculus of mechanics become physical laws, that constitute physical mechanics as a theory with factual content that can be tested by observations. Carnap views the customary division of physics into theoretical and experimental physics as corresponding to the distinction between calculus and interpreted system. The work in theoretical physics consists mainly in the essentially mathematical work of constructing calculi and carrying out deductions with the calculi. In experimental physics interpretations are made and theories are tested by experiments.

Carnap maintains that any physical theory and even the whole of physics can be presented in the form of an interpreted system consisting of a specific calculus, an axiom system, and a system of semantical rules for interpretation. The axiom system is based on a logicomathematical calculus with customary interpretation for the nondescriptive terms. The construction of a calculus supplemented by an interpretation is called "formalization". Formalization has made it possible to forgo a so-called intuitive understanding of the theory. Carnap says that when abstract, nonintuitive formulas such as Maxwell's equations of electromagnetism were first proposed as new axioms, some physicists endeavored to make them intuitive by constructing a "model", which is an analogy to observable macroprocesses. But he maintains that the creation of a model has no more than aesthetic, didactic, or heuristic value, because the model offers nothing to the application of the physical theory. With the advent of relativity theory and quantum theory this demand for intuitive understanding has waned.

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A more adequate and mature treatment of physics as a semantical system, and especially of the problem of abstract or theoretical terms in the semantical system, can be found in Carnap's "The Methodological Character of Theoretical Concepts" (1956) and in his *Philosophical Foundations of Physics: An Introduction to the Philosophy of Science* (1966). Firstly some preliminary comments about terms and laws: All the descriptive terms in the object languages used in science may be classified as either prescientific or scientific terms. The prescientific terms are those that occur in what Carnap calls the physicalist or thing-language. This language is not the same as the phenomenalist language advocated by Mach. Carnap had earlier in his career attempted to apply constructionalist procedures to the construction of a phenomenalist language in his *Logical Structure of the World* (1928). But later he decided to accept a language in which the idea of a physical thing is not linguistically constructed out of elementary phenomena, because he came to believe that all science could be reduced to the thing-language. This thing-language refers to things and to the properties of things; in Russell's predicate calculus things and properties are symbolized as two distinct types of signs: instantiation signs and predicate signs. But the thing language is also expressible in a natural language such as English. The predicates or other descriptive signs referring to properties are of two types: observation terms and disposition terms. Observation terms are simply names for observable properties such as "hot" and "red." These words are called "observable thing-predicates." Disposition terms express the disposition of a thing to a certain behavior under certain conditions. They are called "disposition predicates" and are exemplified by such words as "elastic", "soluble", and "flexible." These terms are not observable thing-language properties, but by use of conditional reduction sentences they are reducible to observation predicates. Opposed to prescientific terms are scientific terms. Carnap classified all scientific terms as "theoretical terms" in a broad sense, even though physicists, as he notes, customarily refer to such terms as "length" and "temperature" as observation terms, because their measurement procedures are relatively simple. More abstract theoretical terms are exemplified by "electron" or "electrical field." A discussion of theoretical terms requires some further discussion of semantical rules in physical theory. There are two types of semantical rules: definitions and conditional reduction sentences. A reduction sentence for a descriptive sign is a conditional statement that gives for the sign the conditions for its application by reference to other signs. The reduction sentence does not give the complete meaning for the descriptive sign, but it gives part of its

meaning. It is a "method of determination" enabling the user to apply the term in concrete cases. A definition is a special case of a reduction sentence that gives all of the meaning of a descriptive term, because it is an equivalence or biconditional sentence. There is never more than one definition for a univocal term, but there may be many reduction sentences for a univocal term, each of which contributes to the term a part of its meaning. Unfortunately Carnap seems never to have elaborated on how the meanings of terms can have parts. Both types of semantical rules - definitions and reduction sentences - introduce new terms into an object language. If one language is such that every descriptive term in it is expressible by reduction sentences in terms of another language, then the second language is called a "sufficient reduction basis" for the first language. For all scientific terms the scientist always knows at least one method of determination, and all such methods always either are reduction sentences or are introduced into an axiomatic system of physics by explicit definition in the axiomatic system.

Carnap states that he disagrees with the philosophy of the physicist Paul W. Bridgman, who stated in his *Logic of Modern Physics* (1927) that, every quantitative concept must be defined uniquely by the procedures for measuring it. This principle is called "operationalism", and it implies that there are as many different concepts of temperature or length as there are different ways of measuring temperature or length. Carnap maintains that these different operational rules for measurement should not be considered definitions giving the complete meaning of the quantitative concept. He prefers his idea of reduction sentences in which statements of operational procedures are semantical rules giving only part of the meaning of the theoretical term. In Carnap's philosophy what distinguishes theoretical terms from observation terms is precisely the fact that the meanings of theoretical terms are always partial and incomplete. This view distinguishes Carnap from Heisenberg and from other Positivists such as Nagel, who prefer equivocation to partial meanings. In Carnap's view statements of operational rules understood as reduction sentences together with all the postulates of theoretical physics function to give partial interpretations to quantitative concepts. These partial interpretations are never final, but rather are continually increased or "strengthened" by new laws and new operational or measurement rules that develop with the advance of physics. Such in brief is Carnap's taxonomy of terms.

Consider next Carnap's views on scientific laws: Carnap classifies scientific laws as empirical laws and theoretical laws. This division does not

correlate exactly to the division between observation terms and theoretical terms in the broader and less abstract sense of his meaning of "theoretical term." The distinction is based on how the laws are developed. Empirical laws are also called empirical generalizations, because they are developed by inductive generalization, which to Carnap means recognition of regularities by observation of repeating instances. The empirical laws contain observation predicates or magnitudes that are measured by relatively simple procedures that can be expressed in reduction sentences or definitions. Empirical laws therefore may contain theoretical terms, such as "temperature", "volume", and "pressure", as occur in Boyle's gas laws, as well as observation terms as may occur in such universal generalizations as "all ravens are black." The scientist makes direct observations or repeated measurements, finds certain regularities, and then expresses the regularities in an empirical law. Theoretical laws on the other hand cannot be made by inductive generalization, because they contain theoretical terms in the narrower or more abstract sense; these theoretical terms are too abstract for making laws by generalization. Examples of these terms are "electron", "atom", "molecule", and "electromagnetic field." These are the descriptive terms that the physicists also call theoretical and unobservable, and measurements associated with these theoretical terms cannot be acquired in simple or direct ways. The development of theoretical laws proceeds by the physicists' imaginative construction of theories in the object language of their science.

Having examined Carnap's classification of the types of terms and of scientific laws, it is now possible to discuss the construction of physical theories. Logically there is firstly a calculus. Conceivably the calculus might be completely uninterpreted, but most often the calculus is supplied by what Carnap calls the logicomathematical calculus with its semantical rules for its logical terms supplying the "customary" interpretations. In other words the physicist seldom develops his own logic or mathematics, although he may use a pre-existing mathematics that had never previously been used in physics, e.g. a non-Euclidian geometry. The physicist then postulates certain axioms, and the descriptive terms in the axiomatic system will either be primitive terms or will be completely defined by reference to primitive terms given in the axioms. In the axiom system the primitive terms may be classified either as elementary terms or as theoretical terms in the narrow or more abstract sense. Elementary terms are either observation terms, or are simple magnitudes which are theoretical terms in the less abstract sense. The elementary terms are given their semantical interpretation by semantical

rules that either define them or give methods of determination by conditional reduction sentences.

The aim of the early Positivists was to make all the primitive terms elementary terms. In this way the semantics of the primitive terms would be given by semantical rules that would either designate them as observation predicates, or designate them by reference to experimental measurement procedures. And since none of the abstract theoretical terms are primitive in the axiomatic system, any such terms would have to be defined by reference to the primitive terms. This method would completely satisfy the early Positivist requirement that all the semantics in the physical theory be supplied by semantical rules that constitute an effective reduction of the theory to observations or to experimentally based measurements. This would surely insure that there would be no contamination of science by metaphysical "nonsense."

However, there is a problem with this approach, even though it would satisfy the requirements of the early Positivists. The theories actually constructed by physicists contain abstract theoretical terms that cannot be defined by reference to elementary descriptive terms having semantical rules directly giving them their empirical meanings. As Carnap states, what physicists actually do is not to make all the primitive terms elementary terms, but rather to make the abstract theoretical terms primitive in the axiomatic system and to make the axioms of the systems very general theoretical laws. In this constructional procedure the semantical rules initially have no direct relation to the primitive theoretical terms. Carnap borrows Carl G. Hempel's metaphorical language describing the axioms with their primitive terms as "floating in the air", meaning that the theoretical hypotheses are firstly developed by the imagination of the physicist, while the elementary terms occurring in the empirical laws are "anchored to the ground." There remains to connect the theoretical laws with the empirical laws. This is achieved by a kind of reduction sentence that relates the abstract theoretical terms in the theoretical laws with the elementary terms in the empirical laws. This reduction sentence is called the "correspondence rule." It is a semantical rule that gives a partial and only a partial interpretation to the abstract theoretical terms. Thus the axiomatic system is left open, to make it possible to add new correspondence rules when theories are modified and as physics develops, until one day the theory is completely replaced in a scientific revolution by a newer one with different axioms. The new correspondence rules add more empirical meaning to the theoretical terms as theory is developed, and they also enable the physicist to derive

empirical laws from the theoretical laws. The logical connection between the two types of laws enables the theoretical laws to explain known empirical laws, but Carnap maintains that the supreme value of a theory is its power to predict new empirical laws; explaining known laws is of minor importance in his view. He observes that every successful revolutionary theory has predicted new empirical laws that are confirmed by experiment.

But there still remains a problem for the Logical Positivist. In this more complicated relationship between theory and experiment, there is a question of how abstract theoretical terms can be distinguished from metaphysical "nonsense." Many philosophers of science, such as Popper, maintain that this is a pseudo problem that cannot be solved. But it was resolved to Carnap's satisfaction by the Ramsey sentence. The Cambridge logician, Frank P. Ramsey, proposed that the combined system of theoretical postulates and correspondence rules constituting the theory be replaced by an equivalent sentence, which does not contain the theoretical terms; in the Ramsey sentence the theoretical terms are eliminated and are replaced by existentially quantified dummy variables. The Ramsey sentence has the same explanatory and predictive power as the original statement of the theory, but without the metaphysical questions that are occasioned by the original formulation with its theoretical terms. Carnap reports that Ramsey did not intend that physicists should abandon their use of theoretical terms; theory is a convenient "short hand" that is useful to the physicist.

Finally mention must be made of another application of the reductionist logic, the unity of science. Both Mach and Duhem expressed the belief that there is a basic unity to all science. In the Vienna Circle the principal advocate of using constructional methods for advancing the unity of science was Otto Neurath, a sociologist who was interested in the sociology of science as well as its linguistic analysis. In his autobiography Carnap stated that Neurath's interest in this effort was motivated by the belief that the division between natural sciences and sociocultural sciences, a division that is characteristic of the Romantic tradition, would be a serious obstacle to the extension of the empiricological method to the social sciences. Neurath expressed a preference for the physicalist or thing language rather than the phenomenalist language, since the former is easier to apply in social sciences. His own views are given in his "Foundations of the Social Sciences" in the second volume of the *International Encyclopedia of Unified Science* (1944). But before Neurath had published his views, Carnap had published his "Logical Foundations of the Unity of Science" in the first volume of the *Encyclopedia* (1938), where he set forth the

constructionalist procedures for the logical reduction of the descriptive vocabulary of the empirical sciences to the observational thing language. The use of the thing language presumes in Carnap's view a philosophical thesis called physicalism, the view that the whole of science can be reduced to the physical language, the language of physical things. Carnap says that the physiological and behavioristic approaches in psychology and social science are reducible to the observational thing language, but that the introspective method may not be reducible. The aim of Carnap's constructionalist program is the logical reduction only of the descriptive terms in science to the observational thing language; this effort is not a reduction of the empirical laws of the sciences to one another. The reduction of laws occurs as a part of the development of the sciences themselves and is the task of the empirical scientist, not of the philosopher of science. The constructionalist procedures for the reduction of descriptive terms for the unity of science are the same as those that Carnap had developed for the reduction of theoretical terms.

Semantical Systems: Probability and Induction

In his article "Testability and Meaning" in *Philosophy of Science* (1936) Carnap abandoned the idea of verification, because he concluded that hypotheses about unobserved events in the physical world can never be completely verified by observational evidence. Thus he proposed instead the probabilistic idea of confirmation. He became interested in the philosophy of probability in 1941, when he considered that the concept of logical probability might supply an exact quantitative explication of the concept of confirmation of a hypothesis with respect to a given body of evidence, such that it would become possible to speak of a degree of confirmation in a measurable sense. Up to that time there were fundamentally two kinds of concepts of probability, which were proposed by their advocates as alternatives. The earlier view is the frequency concept advanced by Richard von Mises and Hans Reichenbach. The other view is the logical concept advanced by John Maynard Keynes in 1921 and by Harold Jeffreys in 1939, and also considered by Ludwig Wittgenstein in his *Tractatus*, where he defined probability on the basis of the logical ranges of propositions. Wittgenstein's interpretation construes a probability statement to be analytic unlike the frequency concept, which construes it to be synthetic or factual. Carnap believed that the logical concept of probability

is the basis for all inductive inference, and therefore he identifies the concept of logical probability with the concept of inductive probability.

In 1950 Carnap published *Logical Foundations of Probability*. This work on probability is not a development in the calculus of probability or in the techniques of statistical inference. It is Carnap's contribution to the interpretation of probability theory with the constructionalist approach, a further development of his metatheory of semantical systems. Here his distinction between object language and metalanguage serves as the basis for his relating the concepts of logical and statistical probability. Statements of statistical probability occur in an object language and are empirical statements about the world. Statements of logical probability occur in the metalanguage and are about the degree of confirmation of statements in the object language. Carnap also refers to the statements in the metalanguage for scientific theory as "metascientific" statements. However, for Carnap metascientific statements are not empirical, but rather are analytic or L-true; he does not recognize an empirical metascience. He accepts the frequency interpretation for the statistical probability asserted by statements in the object language; statistical probability therefore is the relative frequency of an occurrence of an event in the long run. Logical probability is the estimate of statistical probability, and it is the measure of the degree of confirmation. Symbolically he expresses this logical probability as:

$$c(h,e) = r$$

which means that hypothesis h is confirmed by evidence e to the degree r. The variable r is the measure of the degree of confirmation, such that r can take values from 0.0 to 1.0; it is the estimate of the relative frequency and is expressed as:

$$r = m(e*h)/m(e)$$

where $m(e*h)$ is the number of observation sentences describing observed confirming instances e of hypothesis h, and $m(e)$ is the number of observation sentences e describing the total number of observed instances, both confirming and disconfirming. He calls m a measurement function.

In Carnap's view the logical foundation of probability is logic in the sense of L-truth, and he therefore draws upon his metatheory of semantical systems, in which his ideas of state description and range have a central role. A state description is a conjunction containing for every atomic sentence that can be formed in a language, either its affirmation or its negation but not

both. Thus every L-true sentence is true in all the state descriptions, and every L-false or self-contradictory sentence is false in every state description. The F-true or factually true sentences are true in only some state descriptions but are not true in others. When the idea of state description is related to the concept of logical probability, the L-true sentences have a degree of confirmation of 1.0, and the L-false sentences have a degree of confirmation of 0.0. The F-true sentences on the other hand have a degree of confirmation between 1.0 and 0.0. A closely related concept is that of the range of a statement. The range is defined as the class of all state descriptions in which an empirical statement is true, and it may also be defined as those state descriptions that L-imply the statement. Using the concept of range the equation $r = m(e*h)/m(e)$ may be said to be the partial inclusion of the range of e in h as measured by r. Therefore the equation $c(h,e) = r$ is analogous to the statement that e L-implies h except that the range of e is not completely contained in h. Both types of statements are analytical or L-true statements in the metalanguage, because both are statements in logic, one in inductive logic and the other in deductive logic. In Carnap's philosophy the logical foundations of probability is logic in the sense of L-truth.

In 1952 Carnap published *The Continuum of Inductive Methods,* which was to be the volume on the theory of induction that followed *Logical Foundations of Probability,* but he became dissatisfied with this treatment. For many years he continued to work on induction. At the time of his death in 1970 he had completed "Inductive Logic and Rational Decisions" and "A Basic System of Inductive Logic, Part I", which were published in S*tudies in Inductive Logic and Probability,* Volume I (ed. Carnap and Jeffrey, 1971). Carnap did not complete Part II of "A Basic System", and it was edited for publication in 1980 by Jeffrey in *Studies in Inductive Logic and Probability*, Volume II. In "Inductive Logic and Rational Decisions" Carnap is concerned with Bayesian decision theory. In this context the term "probability" does not mean relative frequency, but rather means degree of belief. He distinguishes the psychological concept of actual degree of belief from the logical concept of rational degree of belief. The former is empirical and descriptive, while the latter is normative for rational decision making. Carnap considers the former to be subjective, since it differs from one individual person to another, while the latter is objective. Carnap maintains that contrary to prevailing opinion relative frequency is not the only kind of objective probability. He also calls the former "actual credence" and the latter "rational credence." Rational credence is the link between descriptive

theory and inductive logic, and like inductive logic it is formal, deductive and axiomatic. The concepts of inductive logic and of normative decision theory are similar but not identical. The latter are quasi psychological, while the former have nothing to do with observers and agents, even as these are generalized so that the decision theory is not subjective. Hence there are separate measure functions and confirmation functions for rational decision theory and for inductive logic. In his "A Basic System of Inductive Logic" Carnap develops a set-theoretic axiomatic system, which uses set connectives instead of sentence connectives, and which is equivalent to the customary axiom systems for conditional probability.

Semantical Systems: Information Theory

In 1953 Carnap and Yehousha Bar-Hillel, professor of logic and philosophy of science at the Hebrew University of Jerusalem, Israel, jointly published "Semantic Information" in the *British Journal for the Philosophy of Science*. A more elaborate statement of the theory may be found in chapters fifteen through seventeen of Bar-Hillel's *Language and Information* (1964). This semantical theory of information is based on Carnap's *Logical Foundations of Probability* and on Shannon's theory of communication. In the introductory chapter of his *Language and Information* Bar-Hillel states that Carnap's *Logical Syntax of Language* was the most influential book he had ever read in his life, and that he regards Carnap to be one of the greatest philosophers of all time. In 1951 Bar-Hillel received a research associateship in the Research Laboratory of Electronics at the Massachusetts Institute of Technology. At the time he took occasion to visit Carnap at the Princeton Institute for Advanced Study. In his "Introduction" to *Studies in Inductive Logic and Probability*, Volume I, Carnap states that during this time he told Bar-Hillel about his ideas on a semantical concept of content measure or amount of information based on the logical concept of probability. This is an alternative concept to Shannon's statistical concept of the amount of information. Carnap notes that frequently there is confusion between these two concepts, and that while both the logical and statistical concepts are objective concepts of probability, only the second is related to the physical concept of entropy. He also reports that he and Bar-Hillel had some discussions with John von Neumann, who asserted that the basic concepts of quantum theory are subjective and that this holds especially for entropy, since this concept is based on probability and amount of

information. Carnap states that he and Bar-Hillel tried in vain to convince von Neumann of the existence of the differences in each of these two pairs of concepts: objective and subjective, logical and physical. As a result of the discussions at Princeton between Carnap and Bar-Hillel, they undertook the joint paper on semantical information. Bar-Hillel reports that most of the paper was dictated by Carnap. The paper was originally published as a *Technical Report* of the MIT Research Laboratory in 1952.

In the opening statements of "Semantic Information" the authors observe that the measures of information developed by Claude Shannon have nothing to do with what the semantics of the symbols, but only with the frequency of their occurrence in a transmission. This deliberate restriction of the scope of mathematical communication theory was of great heuristic value and enabled this theory to achieve important results in a short time. But it often turned out that impatient scientists in various fields applied the terminology and the theorems of the theory to fields in which the term "information" was used presystematically in a semantic sense. The clarification of the semantic sense of information is very important, therefore, and in this paper Carnap and Bar-Hillel set out to exhibit a semantical theory of information that cannot be developed with the concepts of information and amount of information used by Shannon's theory. Notably Carnap and Bar-Hillel's equation for the amount of information has a mathematical form that is very similar to that of Shannon's equation, even though the interpretations of the two similar equations are not the same. Therefore a brief summary of Shannon's theory of information is in order at this point before further discussion of Carnap and Bar-Hillel's theory.

Claude E. Shannon published his "Mathematical Theory of Communication" in the *Bell System Technical Journal* (July and October, 1948). The papers are reprinted together with an introduction to the subject in *The Mathematical Theory of Communication* (Shannon and Weaver, 1964). Shannon states that his purpose is to address what he calls the fundamental problem of communication, namely, that of reproducing at one point either exactly or approximately a message selected at another point. He states that the semantical aspects of communication are irrelevant to this engineering problem; the relevant aspect is the selection of the correct message by the receiver from a set of possible messages in a system that is designed to operate for all possible selections. If the number of messages in the set of all possible messages is finite, then this number or any monotonic function of this number can be regarded as a measure of the information produced, when one message is selected from the set and with all selections

being equally likely. Shannon uses a logarithmic measure with the base of the log serving as the unit of measure. His paper considers the capacity of the channel through which the message is transmitted, but the discussion is focused on the properties of the source. Of particular interest is a discrete source, which generates the message symbol by symbol, and chooses successive symbols according to probabilities. The generation of the message is therefore a stochastic process, but even if the originator of the message is not behaving as a stochastic process, the recipient must treat the transmitted signals in such a fashion. A discrete Markov process can be used to simulate this effect, and linguists have used it to approximate an English-language message. The approximation to English language is more successful, if the units of the transmission are words instead of letters of the alphabet. During the years immediately following the publication of Shannon's theory linguists attempted to create constructional grammars using Markov processes. These grammars are known as finite-state Markov process grammars. However, after Noam Chomsky published his *Syntactical Structures* in 1956, linguists were persuaded that natural language grammars are not finite-state grammars, but are potentially infinite-state grammars.

In the Markov process there exists a finite number of possible states of the system together with a set of transition probabilities, such that for any one state there is an associated probability for every successive state to which a transition may be made. To make a Markov process into an information source, it is necessary only to assume that a symbol is produced in the transition from one state to another. There exists a special case called an ergodic process, in which every sequence produced by the process has the same statistical properties. Shannon proposes a quantity that will measure how much information is produced by an information source that operates as a Markov process: given n events with each having probability $p(i)$, then the quantity of information H is:

$$H = \sum_{i=1}^{n} p(i) \, log \, p(i).$$

In their "Semantic Information" Carnap and Bar-Hillel introduce the concepts of information content of a statement and of content element. Bar-Hillel notes that the content of a statement is what is also meant by the

Scholastic adage, *omnis determinatio est negatio*. It is the class of those possible states of the universe, which are excluded by the statement. When expressed in terms of state descriptions, the content of a statement is the class of all state descriptions excluded by the statement. The concept of state description had been defined previously by Carnap as a conjunction containing as components for every atomic statement in a language either the statement or its negation but not both, and no other statements. The content element is the opposite in the sense that it is a disjunction instead of a conjunction. The truth condition for the content element is therefore much less than that for the state description; in the state description all the constituent atomic statements must be true for the conjunction to be true, while for the content element only one of the constituent elements must be true for the conjunction to be true. Therefore the content elements are the weakest possible factual statements that can be made in the object language. The only factual statement that is L-implied by a content element is the content element itself. The authors then propose an *explicatum* for the ordinary concept of the "information conveyed by the statement *i*" taken in its semantical sense: the content of a statement *i,* denoted *cont(i),* is the class of all content elements that are L-implied by the statement *i.*

The concept of the measure of information content of a statement is related to Carnap's concept of measure over the range of a statement. Carnap's measure functions are meant to explicate the presystematic concept of logical or inductive probability. For every measure function a corresponding function can be defined in some way, that will measure the content of any given statement, such that the greater the logical probability of a statement, the smaller its content measure. Let *m(i)* be the logical probability of the statement *i.* Then the quantity *1-m(i)* is the measure of the content of *i*, which may be called the "content measure of *i*", denoted *cont(i).* Thus:

$$cont(i) = 1- m(i).$$

However, this measure does not have additivity properties, because *cont* is not additive under inductive independence. The *cont* value of a conjunction is smaller than the *cont* value of its components, when the two statements conjoined are not content exclusive. Thus insisting on additivity on condition of inductive independence, the authors propose another set of measures for the amount of information, which they call "information measures" for the idea of the amount of information in the statement *i*, denoted *inf(i),* and which they define as:

$$inf(i) = log \ \{1/[1-cont(i)]\}$$

which by substitution transforms into:

$$inf(i) = - log \ m(i).$$

This is analogous to the amount of information in Shannon's mathematical theory of communication but with inductive probability instead of statistical probability. They make their use of the logical concept of probability explicit when they express it as:

$$inf(h/e) = - log \ c(h,e)$$

where *c(h,e)* is defined as the degree of confirmation and *inf(h/e)* means the amount of information in hypothesis *h* given evidence *e*. Bar-Hillel says that *cont* may be regarded as a measure of the "substantial" aspect of a piece of information, while *inf* may be regarded as a measure of its "surprise" value or in less psychological terms of its "objective unexpectedness." Bar-Hillel believed that their theory of semantic information might be fruitfully applied in various fields. However, neither Carnap nor Bar-Hillel followed up with any investigations of the applicability of their semantical concept of information to scientific research. Later when Bar-Hillel's interests turned to the analysis of natural language, he noted that linguists did not accept Carnap's semantical views.

Shreider's Semantic Theory of Information

Carnap's semantic theory of information may be contrasted with a more recent semantic information theory proposed by the Russian information scientist, Yu A. Shreider (also rendered from the Russian as Ju A. Srejder). In his "Basic Trends in the Field of Semantics" in *Statistical Methods in Linguistics* (1971) Shreider distinguishes three classifications or trends in works on semantics, and he relates his views to Carnap's in this context. The three classifications are ontological semantics, logical semantics, and linguistic semantics. He says that all three of these try to solve the same problem: to ascertain what meaning is and how it can be described. The first classification, ontological semantics, is the study of the various philosophical aspects of the relation between sign and signified. He

says that it inquires into the very nature of existence, into the degrees of reality possessed by signified objects, classes and situations, and that it is closely related to the logic and methodology of science and to the theoretical foundations of library classification.

The second classification, logical semantics, studies formal sign systems as opposed to natural languages. This is the trend in which he locates Carnap, as well as Quine, Tarski, and Bar-Hillel. The semantical systems considered in logical semantics are basic to the metatheory of the sciences. The meaning postulates determine the class of permissible models for a given system of formal relations. A formal theory fixes a class of syntactical relations, whence there arises a fixed system of semantic relations between a text describing a possible world.

The third classification, linguistic semantics, seeks to elucidate the inherent organization in a natural language, to formulate the inherent regularities in texts and to construct a system of basic semantic relations. The examination of properties of extralinguistic reality, which determines permissible semantic relations and the ways of combining them, is carried considerably farther in linguistic semantics than in logical semantics, where the question is touched upon only in the selection of meaning postulates. However, linguistic semantics is still rather vague and inexact, being an auxiliary investigation in linguistics used only as necessity dictates. Shreider locates his work midway between logical and linguistic semantics, because it involves the examination of natural language texts with logical calculi.

Shreider's theory is a theory of communication that explains phenomena not explained by Shannon's statistical theory. Bibliographies in Shreider's English-language articles contain references to Carnap's and Bar-Hillel's 1953 paper, and Shreider explicitly advocates Carnap's explication of intensional synonymy in terms of L-equivalence. But Shreider's theory is more accurately described as a development of Shannon's theory, even though Shreider's theory is not statistical. English language works by Shreider include "On the Semantic Characteristics of Information" in *Information Storage and Retrieval* (1965), which is also reprinted in *Introduction to Information Science* (ed. Tefko Saracevic, 1970), and "Semantic Aspects of Information Theory" in *On Theoretical Problems On Information* (Moscow, 1969). Furthermore comments on Shreider and other contributors to Russian information science (or "informatics" as it is called in Russia) can be found in "Some Soviet Concepts of Information for

Information Science" in the *American Society for Information Science Journal* (1975) by Nicholas J. Belkin.

Like many information scientists who take up semantical considerations, Shreider notes that there are many situations involving information, in which one may wish to consider the content of the message signals instead of the statistical frequency of signal transmission considered by Shannon's theory. But Shreider furthermore maintains that a semantical concept of information implies an alternative theory of communication in contrast to Shannon's "classical" theory. Shannon's concept pertains only to the potential ability of the receiver to determine from a given message text a quantity of information; it does not account for the information that the receiver can effectively derive from the message, that is, the receiver's ability to "understand" the message. In Shreider's theory the knowledge had by the receiver prior to receiving the message is considered, in order to determine the amount of information effectively communicated.

More specifically, in Shannon's probability-theoretic approach, before even considering the information contained in a message about some event, it is necessary to consider the *a priori* probability of the event. Furthermore according to Shannon's first theorem, in the optimum method of coding a statement containing more information requires more binary symbols or bits. In Shreider's view, however, a theory of information should be able to account for cases that do not conform to this theorem. For example much information is contained in a statement describing a newly discovered chemical element, which could be coded in a small number of binary symbols, and for which it would be meaningless to speak of an *a priori* probability. On the other hand a statement describing the measurements of the well known physicochemical properties of some substance may be considerably less informative, while it may need a much more extensive description for its coding. The newly discovered element will change our knowledge about the world much more than measurement of known substances. Shreider maintains that a theory of information that can take into account the receiver's ability to "understand" a message must include a description of the receiver's background knowledge. For this reason his information theory includes a thesaurus, by which is meant a unilingual dictionary showing the semantic connections among its constituent words.

Let **T** denote such a thesaurus to represent a guide in which there is recorded our knowledge about the real world. The thesaurus **T** can be in any one of various states, and it can change or be transformed from one state to another. Let **M** represent a received message, which can transform the

thesaurus **T.** Then the concept of amount of information, denoted $L(\mathbf{T,M})$, may be defined as the degree of change in the thesaurus **T** under the action of a given statement **M.** And for each admissible text **M** expressed in a certain code or language, there corresponds a certain transformation operator θ, which acts on thesaurus **T.** The salient point is that the amount of information contained in the statement **M** relative to the thesaurus **T** is characterized by the degree of change in the thesaurus under the action of the communicated statement. And the understanding of the communicated statement depends on the state of the receiver's thesaurus. Accordingly the thesaurus **T** can understand some statements and not others. There are some statements that cannot be understood by a given thesaurus, and the information for such a thesaurus is zero, which is to say $L(\mathbf{T, M})\mathbf{=0}$, because the thesaurus **T** is not transformed at all. One such case is that of a student or a layman who does not have the background to understand a transmitted message about a specialized subject. Another case is that of someone who already knows the transmitted information, so that it is redundant to what the receiver already knows. In this case too there is no information communicated, and again $L(\mathbf{T,M})\mathbf{=0}$, but in this case it is because the thesaurus **T** has been transformed into its initial state. The interesting situation is that in which the receiver's thesaurus is sufficiently developed that he understands the transmitted message, but still finds his thesaurus transformed into a new and different state as a result of receipt of the new information. If the rules of construction of the transformation operator θ are viewed as external to the thesaurus **T**, then the quantity $L(\mathbf{T,M})$ depends on these rules. And when the transformation operator θ is also revised, a preliminary increase of the knowledge stored in the thesaurus **T** may not only decrease the quantity of information $L(\mathbf{T,M})$, but can also increase it. Thus someone who has learned a branch of a science will derive more information from a special text in the branch than he would before he had learned it. This peculiar property of the semantic theory of information basically distinguishes it from the Shannon's classical theory, in which the increase in *a priori* information always decreases the amount of information from a message statement **M**. In the classical theory there is no question of a receiver's degree of "understanding" of a statement; it is always assumed that he is "tuned." But in the semantic theory the essential role is played by the very possibility of correct "tuning" of the receiver.

In his 1975 article Belkin reports that Shreider further developed his theory of information to include the idea of "meta-information." Meta-information is information about the mode of the coding of information, i.e.

the knowledge about the relation between information and the text in which it is coded. In this sense of meta-information the receiver's thesaurus must contain meta-information in order to understand the information in the received message text, because it enables the receiver to analyze the organization of the semantic information, such as that which reports scientific research findings. Shreider maintains that informatics, the Russian equivalent to information science, is concerned not with information as such, but rather with meta-information, and specifically with information as to how scientific information is distributed and organized. Therefore, with his concept of meta-information Shreider has reportedly modified his original theory of communication by analyzing the thesaurus **T** into two components, such that **T=(Tm,To).** The first component **Tm** consists of the set of rules needed for extracting elementary messages from the text **M**, while the second component **To** consists of the factual information that relates those elementary messages systematically and enables the elements to be integrated in **T.** The relationship between **Tm** and **To** is such that a decrease in the redundancy of coding of **To** requires an increase of the meta-information in **Tm** for the decoding of the coding system used for **To**. Hence the idea of meta-information may be a means of realizing some limiting efficiency laws for information by analyzing the dependency relation between information and the amount of meta-information necessary to comprehend that information.

It would appear that if the coding system is taken as a language, then Shreider's concept of meta-information might include to the idea of metalanguage as used by Carnap and other analytical philosophers, or it might be incorporated into the metalanguage. Then the elements **Tm** and **To** are distinguished as metalanguage and object language respectively, although the philosophers have had little interest in examining the inverse dependency between them.

The Philosophy of Science

Aim of Science

Carnap's explicit statement of the aim of science is set forth in his *Aufbau*. The aim of science consists in finding and ordering true propositions firstly through the formulation of the constructional system - the introduction of concepts - and secondly through the ascertainment of the empirical connections between the concepts. This is completely

programmatic, and says nothing about what scientists actually do in their research practices. For most contemporary philosophers a discussion of the aim of science is a discussion in the pragmatics of science, that is, what the scientist does as a user of scientific language when he does research. But Carnap identifies the pragmatics of language with the empirical investigation of historically given natural languages. He always constructs his own languages usually using Russell's symbolic logic, and then uses these artificial languages to address the philosophical problems of interest to the Positivist program for philosophy, namely, the reduction of theoretical terms to demonstrate their meaningfulness and the reduction of the vocabulary of science to the common basis set forth in the *Aufbau*, to advance its unification.

Scientific Explanation

Carnap also has explicit views on scientific explanation: He says it always involves laws, and he classifies scientific laws as either empirical laws or theoretical laws. Empirical laws explain facts, which are statements that describe individual instances. The explanation has the logical structure of a deduction. The premises of the deduction consist of at least one law that has the form of a conditional statement, and statements of fact that describe individual instances in the same terms as those occurring in the antecedent sentences of the conditional law. The conclusion is also a factual sentence that describes the individual instances in the same terms as those in the consequent sentence of the conditional law. In this manner empirical laws explain observed instances described by factual statements. Theoretical laws are related to empirical laws in a way that is analogous to the way that empirical laws are related to facts. The theoretical law is more general. It helps to explain deductively empirical laws that are already known and to permit the derivation of new empirical laws, just as the empirical laws help to explain facts that have been observed and to predict new facts. Furthermore the theoretical law puts several empirical laws into an orderly pattern, just as the empirical generalization puts many facts into an orderly pattern. The supreme value of theory is its power to predict new empirical laws; explaining known laws is of minor value. Every revolutionary theory in the history of science has predicted new empirical laws that are confirmed by empirical tests.

Unlike Duhem, Carnap does not stratify the semantics of physics. To say that theoretical laws explain empirical laws is not for Carnap to say as Duhem did, that the theory is an axiomatic system with conclusions that are

statements which parallel the empirical laws, and that have their own semantics that in turn refers to the empirical laws. In Carnap's view the theoretical terms receive all their semantics from the observation terms by means of reduction sentences which he calls "correspondence rules." When Carnap says that theoretical laws explain empirical laws, he means that a deductive relationship is established between the axioms of the theory and the empirical laws, and that the relationship is mediated by the correspondence rules. The postulated axioms, which are the theoretical laws, together with the correspondence rules enable the physicist to explain empirical laws by logical deduction. In Carnap's philosophy the numerical approximation that Duhem saw existing between the solution sets for the equation deduced from the axioms on the one hand and the solution sets for the equation the empirical laws on the other hand, has no semantical implications and is not problematic. The post-Positivist philosophers agree with Duhem, and maintain that while the numerical difference between theoretical and empirical laws are experimentally indistinguishable due to measurement error, nonetheless the solution sets from the two types of laws are logically distinguishable, such that it is incorrect to say that experimental laws are logically derived from theoretical postulates. In Popper's phraseology the derived theoretical laws (such as Newton's) "correct" the experimental laws (such as Kepler's) purporting to describe the same phenomena.

Scientific Criticism

Carnap's philosophy of scientific criticism is his thesis of confirmation. Both theoretical and empirical laws may be more or less confirmed, but empirical laws are confirmed directly by observation or measurement, while theoretical laws are confirmed indirectly through the confirmation of the empirical laws deductively derived from them. Both empirical and theoretical laws may be classified as either universal or statistical laws. Most of Carnap's discussion of this distinction is in the context of empirical laws. All empirical laws are statements expressing observed regularities as precisely as possible. If a certain regularity is observed at all times and in all places, then that regularity is expressed in the form of a universal law. But if the law asserts that an event or the relation of one event to another occurs in only a certain percentage of cases, then the statement is called a statistical law. Both types of laws occur in the object language of science, and both are empirical statements. Statements about either universal and statistical laws occur in the meta-language, that refers to

the object language of science in which the law and theory statements are expressed, and for either types the statements in the metalanguage may refer to the degree of confirmation of the laws. Statements of the degree of confirmation are statements of logical probability associated with both universal and statistical laws. Logical probability is an estimate of the long-term relative frequency stated by the statistical laws, and takes values between zero and one inclusively. The statements associating the degree of confirmation to a statement in the object language are statements in the metalanguage. The metalanguage is a language of the philosopher of science, and philosophy is not in Carnap's view an empirical or factual science. Philosophy of science is the logic of science, and the statements in the metalanguage are L-true or analytic. Logical probability is the logical relation similar to logical implication. By a logical analysis of a stated hypothesis h and the stated evidence e, one may conclude that h is not deductively implied but is partially implied by e to the degree r. For any pair of sentences e and h inductive logic assigns a number giving the logical probability of h with respect to e. In this way Carnap views the metalanguage to consist of analytic statements as opposed to the synthetic statements in the object language consisting of laws of nature.

Scientific Discovery

Carnap's philosophy of scientific discovery gives different accounts for the discovery of empirical laws and the discovery of theoretical laws. His philosophy of discovery of empirical laws is inductivist; induction is the measurement of the degree of regularity in observed instances known either passively by casual observation or actively by experimentation. His philosophy of discovery of theoretical laws recognizes the role of the creative imagination. He gives consideration to the use of computers. He expresses doubts that rules can be established to enable a scientist to survey millions of sentences giving various observational reports, and then by a mechanized procedure applying these rules to generate a general theory consisting of a system of theoretical laws that would explain the observed phenomena. This is because theories deal with unobservables and use a conceptual framework that goes far beyond the framework used for the description of observations. Creative ingenuity is needed to create theories. Therefore Carnap concludes that there cannot be an inductive machine, a computer system into which the scientist can input all the relevant observation sentences, and then get an output consisting of a system of laws that explain the observed phenomena. He only believed that given

observation *e* and hypothesis *h*, there could be an inductive machine which will mechanically determine the logical probability or degree of confirmation of *h* on the basis of *e*. It may be noted in this connection that the post-Positivist philosophers of science rejected the Positivist's strong distinction between theory and observation. Like Einstein and Heisenberg, they maintained that theory determines what is observed. Therefore, they maintain that there exists no theory-independent framework for observation.

Hempel's Critique of Analyticity

Carl G. Hempel (1905-1997) was one of Carnap's more sympathetic colleagues, and had been Carnap's assistant just after immigrating to the U.S. from Nazi Germany. In the *New York Times* (23 November 1997) obituary for Hempel, Quine was quoted as describing Hempel as a "moderate Logical Positivist", and as saying that Hempel's views had been succeeded by relativist doctrines, which would make science a matter of fads, and which Quine are "anti-scientific." In his later years Quine concluded that his wholistic view of observation statements implies a relativistic theory of truth, and he retreated from the implications of his "Two Dogmas of Empiricism" (1952). After reading Quine's "Two Dogma's of Empiricism" in which Quine criticized Carnap's concept of analyticity, Hempel gave serious reconsideration to Carnap's analyticity thesis. Hempel does not reject Carnap's concept of L-truth. His disagreement is only with the concept of A-truth, the truth that Carnap calls meaning postulates, which are known to be true by virtue of the meaning relations among the descriptive terms in the sentence.

Hempel's critique of A-truth is set forth in "Implications of Carnap's Work for the Philosophy of Science" in Schilpp's *The Philosophy of Rudolf Carnap* (1963) and relevant comments are to be found in his earlier work, "Theoretician's Dilemma" in *Minnesota Studies* (1958). Firstly Hempel considers problems of empirical significance presented by analyticity. After contrasting Carnap's concept of reduction sentences with the idea of definition, taking note that the reduction sentence offers convenient schema for a partial operational meaning, Hempel states that contrary to Carnap the reduction type of sentence does not eliminate all dependency on general empirical laws in these sentences. He says that Paul W. Bridgman had advocated operational definitions with one definition for every method of measurement, because defining any measurement concept by more than one method of measure incurs the risk of an invalid empirical generalization,

even if the different methods yield the same measurement value. The reduction type of sentence eliminates this risk, because in it only one generalization is used. However, Hempel says that an inductive risk is still incurred even for reduction sentences, since even if only one operational criterion is used any application of a term requires a generalization. Therefore reduction sentences "fuse" together two functions of language, which had traditionally been thought to be totally different. These are firstly the specification of meanings and secondly the description of contingent fact. He maintains that the fruitful introduction of new concepts in science is always intimately bound up with the establishment of new laws.

Hempel then generalizes on his thesis that reduction sentences have the two functions of meaning specification and empirical law, to produce his own general conception of a semantical or "interpretative" system. Firstly he distinguishes an observational and a theoretical vocabulary. Then he states that a theory T characterized by a set of postulates with primitive theoretical terms constituting the theoretical vocabulary, is made an interpreted system by the set of sentences J satisfying three conditions: (1) J is logically compatible with T; (2) J contains no extralogical (descriptive) terms that are not an element of the observational or theoretical vocabulary; (3) J contains elements of the observational and theoretical vocabulary in an essential way, i.e. in a manner that does not make J logically equivalent to some set of sentences in which neither the observational or the theoretical terms occur. Interpretative systems so conceived share the same two characteristics that distinguish reduction sentences from definitions. Firstly they give only partial definitions of the theoretical terms they specify, and secondly they are not purely stipulative in character, but imply certain statements containing only observational terms. However unlike Carnap's concept of a semantical system with reduction sentences, Hempel's general concept of an interpretative system does not provide an interpretation, complete or incomplete, for each theoretical term individually in the whole system. Therefore in the interpretative system J the theoretical terms are not dispensable, and Hempel argues that in his definition of an interpretative system, the distinction between the theory and its interpretative sentences is arbitrary, because these two types of sentences have the same status and function. It is only in conjunction with the interpretative sentences that the theory can imply observational sentences, and the interpretative sentences no less than the theory may be theoretical laws. Furthermore, when discrepancies between predictions and experimental data call for modification of the predictive apparatus, suitable adjustments may be made

not just by changing the theory but alternatively by changing the interpretative sentences. Therefore interpretative sentences must have the same status as the sentences constituting the theory, thus making it difficult to identify either theory or interpretative sentences as analytic. Following a similar line of argument Hempel rejects Carnap's proposal of introducing predicates by means of meaning postulates, which purport to separate the meaning specification function from the empirically descriptive function. Hempel questions the rationale for separating these two functions. He asks what distinctive status is conferred on a meaning postulate, since any statement once accepted in empirical science may conceivably be abandoned for the sake of resolving a conflict between theory and the stated body of available evidence. He says that apart from logical and mathematical truths, there can be no scientific statements that satisfy conditions for analytic meaning postulates.

　　　In addition to discussing problems for empirical significance of analytical sentences, Hempel also discusses problems of empirical testing. He references Carnap's *Logical Syntax of Language*, where Carnap references Poincare and Duhem, saying that no statement accepted in empirical science is taken to be immune from criticism and revision. Carnap furthermore stated that a statement in a scientific theory cannot be tested in isolation, but must be tested with other accepted statements, such that it is the entire theoretical system that is tested. And this is what Quine also maintains in his "Two Dogmas", which Hempel references in this context. Hempel relates that on Carnap's view of a semantical system, in which theoretical terms are viewed as being introduced by reduction sentences based on an observation vocabulary, it is possible to speak of individual sentences containing theoretical terms as being confirmable by reference to observation sentences. But Hempel notes that in his general concept of an interpreted theory, this idea has no useful counterpart, because one would have to say that the experimental import of a sentence relative to an interpreted theory is expressed by the class of nonanalytic observation sentences implied by the sentences and the theory. His view renders the notions of testability and experiential significance relative to a given theory, assigning all sentences of the theory the same experiential import represented by the class of all observation sentences implied by the theory. This is because testability and empirical significance are attributable not to scientific statements in isolation, but only to interpreted theoretical systems. Furthermore, as Kuhn notes in *The Road Since Structure* (1993), a few years after writing "Theoretician's Dilemma" Hempel began speaking of

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"antecedently available terms" instead of "observation terms", thus implicitly adopting what Kuhn describes as a developmental or historical view of science.

Hempel concludes that these considerations make it doubtful that the basic tenants of Positivism and empiricism can be formulated in a clear and precise way. The circumstance that empirical significance and testability requirements are applicable to entire theoretical systems, make these requirements extremely weak. For the Positivist that weakness permits the disturbing possibility of adding to contemporary physical theory an axiomatized metaphysics of Being and Essence that would be an empirically significant system. One alternative is to exclude theoretical terms altogether, but Hempel invokes the criterion of simplicity. He concludes that the problem of giving a precise explication of this aspect of scientific theories presents a new and challenging task for the philosophy of science.

Carnap's Reply to Hempel

Carnap replies to Hempel's attack on the analytic-synthetic distinction both in the Schilpp volume containing Hempel's critique and in the concluding two chapters of his *Philosophical Foundations of Physics* (1963). He maintains that the analytic-synthetic distinction is of supreme importance for philosophy of science. The theory of relativity could not have been developed had Einstein not recognized the sharp dividing line between pure mathematics, in which there are many logically consistent geometries, and physics, in which only experiment and observation can determine which of these mathematical geometries can be applied most usefully to the physical world. This reply made late in Carnap's career reveals how influential Einstein's development of relativity theory was on Carnap's philosophical thinking.

Firstly however Carnap takes up the identification of the analytic-synthetic distinction in natural language. He notes that natural language is sufficiently imprecise that not everyone understands every word in the same way, such that some sentences may be ambiguous as to whether they are analytic or factual. The division depends on what characteristics described by the predicate terms are taken to be essentially or definitively related to one another. For example does red colored head plumage define a redheaded woodpecker? If not, then a green headed bird may be classified as a redheaded woodpecker, if it has other characteristics deemed definitive

of the species. Carnap maintains that while certain statements may be ambiguous due to the vagueness of the predicates, the analytic-synthetic distinction as such is not therefore problematic for the same reason.

Carnap next turns to the analytic-synthetic distinction in an artificial observation language. In this case the distinction is determined by laying down precise rules, which are the meaning postulates or A-postulates. These rules determine what characteristics described by predicate constants are essential to their subjects. To the extent that these rules are vague, there will be sentences that are vague with respect to the analytic-synthetic status. But Carnap says that in such cases the distinction between analytic and synthetic is not as such vague.

Then he turns to the determination of the distinction in an artificial theoretical language, where the fact that theoretical terms cannot be given complete interpretations causes special difficulties. He takes as an example the track in the Wilson cloud chamber, which can be observed and can be explained in terms of an electron passing through the chamber. Such observations provide only a partial and indirect empirical interpretation of the entity referenced by the theoretical term "electron", to which the observed track is linked by correspondence rules. The problem is to find a way to distinguish in the linguistic network of correspondence postulates and theoretical postulates, those sentences that are analytic and those that are synthetic. It is easy to identify the L-true sentences, because descriptive terms are not involved in determining L-truth. But A-truth, the truth of analytic sentences, is problematic in this case. To recognize analytic statements in a theoretical language, it is necessary to have A-postulates that satisfy the meaning relations holding among the theoretical terms. But the theoretical postulates alone cannot serve as A-postulates, since without the correspondence rules the theoretical postulates have no interpretation at all. Yet the theoretical postulates together with the correspondence postulates cannot be analytic, because then the theory would have no empirical content.

Carnap notes Hempel's proposal that there is a double role for the theoretical and correspondence postulates, that defies the analytic-synthetic distinction, such that these postulates both stipulate meaning and also make empirical assertions. But Carnap proposes another way that preserves the empirical content of scientific theories while admitting the analytic-synthetic distinction. His proposal utilizes the Ramsey sentence, but without Ramsey's final step of eliminating the theoretical terms from the semantical system, since he believes that eliminating theoretical terms is too inconvenient for the scientists, who find that theoretical terms simplify their

work enormously. Instead of splitting an interpreted theory into theoretical postulates and correspondence rules, Carnap proposes splitting it into analytic and factual sentences with the factual part consisting of a Ramsey sentence equivalent to the empirical content of the interpreted theory. The Ramsey sentence therefore implies the whole interpreted theory, and this implication is itself analytic; it is the analytic part of the theory. Carnap maintains that this analytic implication provides a way to distinguish between analytic and synthetic statements in the theoretical language, because the analytic implication is that if there exist entities, that are referenced by the existential quantifiers of the Ramsey sentence, that are of a kind bound together by all the relations expressed in the theoretical postulates of the theory, and that are related to observed entities by all the relations specified by the correspondence postulates of the theory, then the theory itself is true.

In his "Theoretician's Dilemma" Hempel had criticized the Ramsey sentence as avoiding reference to theoretical entities only in Greek variables rather than in spirit. The Ramsey sentence still asserts the existence of certain entities of the kind postulated by a physical theory without guaranteeing any more than does the physical theory that those entities are observable or at least are fully characterizable in terms of observables. Therefore, the Ramsey sentence provides no satisfactory way of avoiding theoretical concepts.

In his replies to Hempel in Schilpp's book Carnap says that he agrees with Hempel that the Ramsey sentence does refer to theoretical entities by the use of abstract variables. But he argues that these entities are not unobservable physical objects like atoms or electrons, but rather are purely logicomathematical entities such as natural numbers, classes of such numbers, or classes of classes. The Ramsey sentence for a physical theory is a factual statement that says that the observable events in the world are such that there are natural numbers, classes of such numbers, or classes of classes, that are correlated with the events in a prescribed way, and which have among themselves certain relations.

Quine's Pragmatist Critiques

Willard Van Orman Quine (1908-2000) was born in Akron, Ohio. In 1930 he graduated *summa cum laude* in mathematics from Oberlin College, and then entered Harvard University's graduate school of philosophy. He

wrote his doctoral dissertation under the direction of Alfred North Whitehead, the co-author with Bertrand Russell of the *Principia Mathematica*, and he published it as *A System of Logistic* in 1934. Quine became a faculty member of Harvard's department of philosophy in 1936, where he remained for the duration of his long career. He enjoyed traveling, and wrote an autobiographical travelogue as *The Time of My Life* in 1985. Quine described his long acquaintanceship with Carnap in "Homage to Rudolf Carnap" (1970), a memorial article published in the year of Carnap's death, and reprinted later in Quine's *Ways of Paradox* (1976). Quine met Carnap during his European travels in the 1930's, and their dialogues continued after Carnap relocated to the United States in 1935. While Quine might be regarded as Carnap's principal protagonist, their philosophies are much more similar than different. In the memorial article Quine refers to Carnap as a towering figure, who dominated philosophy in the 1930's as Russell had in previous decades, and he also refers to Carnap as his greatest teacher. Their private correspondence has been published under the title *Dear Carnap, Dear Van* (ed. Creath, 1990), which reveals nothing about their philosophical views that is not already known from their published works, but exhibits their enduring friendship notwithstanding their widening philosophical differences.

Quine's best known criticism of Carnap's philosophy is his rejection of the analytic type of statement. This criticism together with several others has their basis in Quine's Pragmatist view of empiricism. Quine published a brief statement of his own doctrine of empiricism as "The Pragmatist's Place in Empiricism" (1975), later appearing in his *Theories and Things* (1981) as "Five Milestones of Empiricism." This paper is ostensibly a history of empiricism in terms of five historical turning points, but the five historical milestones also happen to be the central theses of Quine's own Pragmatist philosophy. He summarizes these five historical turning points as follows:

1. The shift from ideas to words
2. The shift of semantic focus from terms to sentences
3. The shift of semantic focus from sentences to systems of sentences
4. The abandonment of the analytic-synthetic distinction
5. The abandonment of any first philosophy prior to natural science

Quine's several criticisms of Carnap's Positivist version of empiricism may be viewed as having a basis in these five distinctive aspects of his Pragmatist version of empiricism. The first two of the five points are the

basis for Quine's criticism of Carnap's doctrine of intensions, as well as a critique of the idea of propositions. The third point, sometimes known as the Duhem-Quine thesis, is the basis for Quine's critique of logical reductionism and for his wholistic thesis of semantical indeterminacy and his thesis of ontological relativity. The fourth is his rejection of analyticity, which follows from the third point. And the fifth and final point is Quine's critique of Carnap's doctrine of "frameworks" and of the distinction between "internal" and "external" questions. Each of these criticisms is considered in greater detail below.

Quine's Critique of Intensions and Propositions

At the close of his "Foreword" to Quine's *A System of Logistic* Whitehead commented that logic prescribes the "shapes" of metaphysical thought. The logic under consideration of course was that in Whitehead and Russell's *Principia Mathematica*, and the metaphysics that is "shaped" by the Russellian syntactical categories - giving the existential claim to the quantifiers - is nominalism. There was probably no expositor of this logic that both illustrated and advocated Whitehead's comment more consistently than Quine. For more than a decade after *System of Logistic* Quine published a number of articles which describe how the Russellian symbolic logic and specifically how its theory of quantification enables the user of the logic to exhibit explicitly his ontological commitments, the shape of his metaphysics. The user's ontological commitment to the kinds of things he believes exists, is exhibited by the variable, the symbol that is bound by either the existential or the universal quantifier. The term "variable" in this context has a distinctive meaning that it does not have in mathematics. In his "A Logistical Approach to the Ontological Problem" (1939) reprinted in *Ways of Paradox* (1966) Quine expresses the role of logical quantifiers with the memorable refrain: To be is to be the value of a variable. This means that what entities there are from the viewpoint of a given discourse in the logic depends on what symbols are accessible to binding by quantifiers to become variables in the symbolic logic, and a shift from one discourse to another may involve a shift of ontology.

In 1947 Quine published "On Universals" in *Journal of Symbolic Logic* and "Logic and the Reification of Universals" in *From A Logical Point of View* (1953). In these papers he describes how the nominalist and realist views toward the historic problem of universals are expressed in the

Russellian notation. The nominalist view is that only individuals exist, and it is expressed in the Russellian notation by limiting the quantifiers to ranging only over symbols referencing individual entities. On the other hand the universalist view affirms that attributes or properties exist. In the Russellian notation the existence of attributes is expressed by placing predicates within the range of quantifiers. For this reason Quine calls the universalist view the "Platonist" view, and he calls the attributes "abstract entities." Or when the abstract entities are said to exist in the human mind as meanings or concepts, Quine calls them "mental entities." The Russellian logic thus imposes a dichotomy that reduces both realism and conceptualism to distorting caricatures that philosophers since Plato have dismissed. The notational role of the quantifier is referential, such that whatever type of symbol may assume the role of a variable bound by a quantifier, thereby assumes the role of referencing an entity. Ostensibly Quine's purpose is not to advocate one or the other ontological thesis, but to advocate the role of the quantifiers as making a philosopher's ontological commitment explicit.

Quine has his own view on the issue of universals. In 1947 he co-authored with Nelson Goodman "Steps Toward A Constructive Nominalism" in *The Journal of Symbolic Logic*. Unlike most papers appearing in academic journals, this article was not so much an analytical paper, as it was a kind of manifesto advocating a nominalist programme for applying the symbolic logic. Quine has denied that he is a nominalist, because he accepts the existence of classes, which he views as a kind of abstract entity. And he accepts the existence of classes, because he could not eliminate them in the logistic reductionist programme. But he denies that descriptive predicates have any signification with a foundation in reality, and offers no explanation as to why classes are anything but arbitrary collections. Typically nominalists do not reject classes. What they reject is that there are either mental concepts or real attributes that are the basis for classes, and they view classes as merely collections of entities that are referenced by terms. Thus notwithstanding Quine's attempt to separate his views from nominalism, he is a *de facto* nominalist, because he explicitly rejects the existence of such abstract entities or mental entities as properties, attributes and intensions, such as are propounded not only by Carnap but also by the majority of Pragmatist philosophers today. Today philosophers of science investigating scientific revolutions and also those developing computational systems have come to accept the existence of a three-level cognitive semantics of words, intensions and extensions, instead of a two-level referential semantics of only words and extensions. Nominalists are

always troubled by coreferential terms having the same extension but having different meanings or intensions. One reason that Quine rejects these latter types of abstract entities is that they can be eliminated from the logistic reductionist programme as he construes it. The second reason is that he denies that Carnap's intensions can be treated extensionally, as Carnap attempts to treat them by relating them to classes by analytical statements, a type of statement that Quine rejects.

In "Five Milestones" Quine notes that the first of the five turning points in the history of empiricism, the shift from ideas to words. In his *Word and Object* he calls this shift "semantic assent", which he advocates because philosophical discourse is carried into a domain where participants are better agreed on the objects, i.e. the words. In "Five Milestones" he says that the shift originated with the medieval nominalists. He argues against the reification of universals, and says that affirming the existence of abstract or mental entities is due to a common confusion, in which descriptive predicates are given a referential function that is properly had by bound variables. In "Ontological Relativity" he describes this error as a case of the copy theory of knowledge, which he says is an uncritical semantics. He ridicules this error as the "myth of the museum" and the "fantasy of the gallery of ideas", by which he means that words are mistakenly understood to be labels for ideas or meanings, as though they were exhibits. He views the confusion between names and descriptions to be a particularly pernicious philosophical error, and he maintains that Russell's theory of descriptions offers the way to avoid it. This is the technique used by Russell in his "On Denoting" in *Mind* (1908). In his "On What There Is" (1948) reprinted in *Logical Point of View* Quine says that Russell's theory of descriptions enables the philosopher to transform names into predicates, such that names should not be taken as an ontological criterion for deciding what is real. The correct criterion for determining the ontology of a language is the use of the quantified symbol or variable, so that predicates are not confused with names, and no claims are made to the effect that predicates name entities, unless the predicates are explicitly quantified.

Closely related to the first milestone, the second is the shift of semantic focus from terms to sentences. In "Five Milestones" Quine explains that the meanings of words are abstractions from the truth conditions of the sentences that contain them, and that it was the recognition of this semantic primacy of sentences that gave us contextual definition. Quine traces the development of contextual definition, which he calls a revolution in semantics, to Jeremy Bentham's technique of "paraphrasis",

which is a kind of paraphrasing or circumlocution. If Bentham found some terms convenient but ontologically embarrassing, contextual definition enabled him in some cases to enjoy the services of the term, while disclaiming its denoting. In "Russell's Ontological Development" (1966) reprinted in *Theories and Things* (1981) Quine joins Ramsey's characterization of Russell's theory of descriptions as a paradigm of philosophical analysis, and he says that our reward for the paraphrasis technique is the recognition that the unit of communication is the sentence and not the word.

In his *Meaning and Necessity* Carnap explicitly affirms that intensions are not names either of concepts or of abstract entities. He maintains that like physical properties intensions may be said to be objective without invoking any hypostatization, and that they are indifferent to either concrete or abstract objects. Carnap's intensions are reminiscent of the Scholastic logicians' distinction between *suppositio* and *significatio* for terms, although Carnap never makes this comparison. According to the theory of *suppositio* a univocal term's *significatio* or meaning is the same whether the term occurs either as a subject or as a predicate in an affirmative categorical proposition. But its *suppositio* or supposition as a subject is called "personal", because it references the individual members of the class according to its associated quantifier, while its supposition as a predicate is called "simple", because no reference is made to the members of the class it signifies, and its meaning is used indifferently with respect to instantiation. It is the use of simple supposition that enables both the Aristotelian-Scholastic logician and the ordinary-language user to say, "Every raven is black" and affirm the reality of the attribute blackness without also affirming the existence of a Platonic entity called "blackness." The Aristotelian logician can distinguish names and predicates while still affirming that the descriptive predicates describe something real. This capability is denied the user of the Russellian predicate logic, who can only affirm the reality of blackness by quantifying the predicate and therefore treat it as an entity; he can only distinguish names and predicates by being nominalist, by denying that descriptive predicates describe anything. As it happens, when Quine attacks Carnap's admission of attributes and intensions, as he does in "On the Individuation of Attributes" (1975) in *Theories and Things*, he attacks Carnap's use of analytic statements and does not claim that Carnap has confused names and predicates. But even apart from the issue of analyticity, Carnap's theory of intensions is inconsistent, because he also accepts the Russellian predicate logic. In the section of *Meaning and Necessity* in which he discusses

variables, Carnap explicitly agrees with Quine's view that the ontology to which one's use of language commits oneself comprises simply of the objects that one treats as falling within the range of values of one's variables, and he explicitly accepts Quine's refrain that to be is to be the value of a variable. Quine and Whitehead recognized, as Carnap had not, that one's logic shapes one's metaphysics, and Quine's papers on theory of reference had as their basis the thesis that the Russellian logic expresses existence exclusively by means of the instantiating quantifiers.

The Russellian manner of expressing ontological commitment has its peculiar and controversial aspects, which are clear when contrasted with the earlier Aristotelian logic. In the Aristotelian logic the quantifier does not affirm existence. Instead existence is affirmed by the copula term "is", as in "Every raven is black." The noteworthy difference is that in the Russellian notational conventions the only existence that can be affirmed is the entities referenced by the quantified variable, such that any attempt to affirm the reality of attributes or properties must describe them as entities referred to by a quantified predicate. In the Aristotelian logic, however, the reality of what may be called an attribute signified by the predicate need not be hypostatized as some kind of Platonic entity. Quine is therefore consistent in his use of the Russellian logic, when he describes the reality status of red, the property, as an abstract "entity", and when he describes the reality status of red, the meaning, as a mental "entity." According to the syntactical categories admitted by the Russellian logic all philosophers are either nominalists or Platonists, since they must either deny attributes as real by not quantifying the predicate, or they must affirm them as Platonic entities by quantifying over the predicate. In the Russellian logic attributes, properties, aspects, and accidents have no reality status except as subsisting entities. Carnap's attempt to admit intensions or meanings and properties that are not hypostatized, is inconsistent with his use of the Russellian logic and with his agreement with Quine that ontology is described by means of bound variables. And his complaint about erroneously labeling philosophers "Platonists" is similarly inconsistent. Other and more consistent philosophers have recognized the Russellian logistic to be an Orwellian-like "newspeak" for advocating a nominalist agenda hidden in its notational conventions, which the pontificating Quine would enforce as a "canonical notation."

In his *Medieval Logic and Metaphysics* (1972) the University of Manchester British philosopher, David P. Henry, asks how modern logic, caught as it is in the "entanglement" of the expression of existence in the

quantifiers, can recapture the untrammeled approach to existence enjoyed by its medieval predecessors. He proposes reconsideration of the modern formal logic of the Polish logician S. Lesniewski (1886-1939), which is unfamiliar to most modern logicians. In his autobiography Quine recounts his arguing with Lesniewski about "abstract entities" (Quine's characterization) while visiting Warsaw in the 1930's. Henry notes that Lesniewski's logic employs an interpretation of the quantifiers, which enables their dissociation from its currently conventional entanglement with the notion of existence. Henry gives examples of how Lesniewski's interpreted system with its ontology may be used in the analysis of medieval themes including *suppositio* with an artificial language designed by Henry. In the present context the significance of Henry's work is that it shows how Quine's ontological agenda does not imply a simplistic dichotomy between modern mathematically expressed logic and antiquated colloquially expressed Aristotelian logic, but rather depends on very specific notational conventions distinctive of the Russellian logistic, to which there can and do exist alternatives. Quine's *weltanschauung* seen through the lenses of Russellian logistic with its ontological agenda reducing attributes either to "abstract entities" or to unreality is terminal case of the mathematician's disease, and it invites comparison with the obliviously contemplative noblemen of the airborne floating island of Laputa in Swift's satirical *Gulliver's Travels*. The Laputians viewed the world through the lenses of Cartesian geometry with Descartes' ontology of primary and secondary qualities. In Descartes' philosophy only geometrical or "primary" qualities have objective reality, while all others are "secondary" in the sense of subjective and unreal. The Laputian noblemen were so obliviously faithful to their distorted Cartesian view of the real world, that they viewed all reality as geometrical figures including even their wives, who were not similarly faithful to the Cartesian ontology, and who therefore felt so neglected that they were inclined to be unfaithful to their husbands. Comparison with Gulliver's travelogue is not merely rhetorical. Quine's rejection of properties, attributes and qualities denies such qualitative differentiation its foundation in reality, and renders Quineian reality as starkly nominalist as Descartes' was extensionalist. And it may be added that attempted paraphrase by quantifying predicates does not evade nominalist ontology; it only incurs a fallacy that Whitehead called "misplaced concreteness, the Platonic hypostatization of properties which earlier logicians had avoided by their theory of *suppositio*. Also the nominalism built into the Russellian notational conventions by combining

existence and quantification is a prior ontological commitment, which is as inconsistent with Quine's ontological relativity as his Positivist behaviorism. Like the Laputian nobility, professors of Russellian predicate logic would greatly benefit, if their graduate-student assistants, who must humor the professor's pretenses, were what Gulliver called "flappers", *i.e.* assistants who swat their superiors in the face whenever the superiors lost touch with reality.

Quine's Critique of Reductionism

Quine took Whitehead's comment, that logic shapes metaphysical thought, beyond logic, and made it a general theory of language. One of the implications is Quine's thesis of the system-determined nature of semantics. Thus the third milestone in "Five Milestones" is the semantical shift from sentences to whole systems of sentences. This shift to a wholistic (or holistic) view of the semantics of language is a central characteristic of Quine's philosophy, although it went through some retrogression. He came to think that his earlier and more radical Pragmatism implies an unwanted cultural relativistic view of truth. Consequently in the 1970's he attempted to restrict the extent of his semantical wholism, so that the semantics of theory is not viewed as contributing to the semantics of observation language.

His first statement of his wholistic thesis is what he later calls his metaphorical statement given in "Two Dogmas of Empiricism" (1951), one of his best known papers, reprinted in his *Logical Point of View* and often found in anthologies. The two dogmas he criticizes in this paper are the Logical Positivist theses of analyticity and reductionism. He defines the reductionist thesis as the belief that each meaningful sentence is equivalent to some logical construct based on terms referring to immediate experience. And he notes that Carnap was the first empiricist who was not content with merely asserting the reducibility of science to terms of immediate experience, but who actually took steps toward carrying out the reduction in the *Aufbau*. Then Quine says that while Carnap later abandoned this radical reductionist effort, the dogma of reductionism continues in the idea that to each synthetic (i.e. empirical or nonanalytic) statement there is associated a unique range of possible sensory events, such that the occurrence of any of them would add to the likelihood of truth of the statement. Similarly for each synthetic statement there is associated another unique range of possible

sensory events whose occurrence would detract from that likelihood. This dogma is implicit in the verificationist theory of meaning, and it survives in the thesis that each statement taken in isolation can admit of either confirmation or "infirmation", which is to say, either verification or falsification.

The view of empiricism that Quine advocates as his alternative to reductionism is the thesis that statements about the external world face the tribunal of sense experience not individually, but only as a corporate body. Quine references Duhem in this context and his alternative view of empiricism has since come to be known as the "Duhem-Quine Thesis." However, while Quine references Duhem in "Two Dogmas", his wholistic view is more radical than Duhem's, because Quine purges Duhem's philosophy of physical theory of its Positivism by ignoring Duhem's two-tier semantics, which led to Duhem's distinction between "practical facts" and "theoretical facts." Quine's treatment here of the difference between observation and theory is not a Positivist semantical metatheory. Furthermore, Quine's radical wholism does not admit a distinctive semantical status even for pure mathematics and formal logic. Speaking metaphorically Quine says that the totality of our beliefs including mathematics and logic is a man-made fabric, which impinges on experience only along the edges. Then mixing metaphors he describes total science as a field of force whose boundary conditions are experience in which the laws of logic and mathematics are simply statements in the field that are more remote from experience. Any conflict with experience at the periphery occasions adjustments in the interior of the field, such that truth values must be redistributed over some statements, and a re-evaluation of some statements entails re-evaluation of others due to the logical connections among them.

The enabling feature of Quine's wholistic doctrine of empiricism is his thesis that the total field is so empirically "underdetermined" by its boundary conditions, which are experience, that there is much latitude for choice as to what statements to re-evaluate in the light of any single contrary experience. And the criterion governing the choice of beliefs in the underdetermined system is entirely pragmatic, where the objective is a relatively simple conceptual scheme for predicting future experience in the light of past experience. The thesis of the empirical underdetermination of language can be traced to Duhem's view of scientific theory. Duhem said that there could be many theories, all equally empirically adequate, that explain the same phenomenon. But Quine furthermore extends Duhem's thesis to include not

just theory but all of language including observation language. He maintains that no statement is immune from revision, and he notes that revision even of the law of the excluded middle has been proposed as a means of simplifying quantum physics. Quine notes that there is a natural tendency when making revisions to disturb one's existing system of beliefs as little as possible, with the result that those statements that we are least likely to revise are those that have sharp empirical reference, while those that we are most likely to revise are those more theoretical statements that are relatively centrally located within the total network or web of beliefs. Later in his *Philosophy of Logic* (1970) this natural tendency becomes the "maxim of minimum mutilation", an idea similar to James' thesis of "minimum disturbance" in the latter's *Pragmatism* (1907).

Quine's most elaborate statement of his wholistic thesis is set forth in his first full-length book, *Word and Object* (1960). Instead of the metaphorical statement of his view in "Two Dogmas" a decade earlier, here he expresses his thesis in the literal vocabulary of behavioristic psychology. Much of the book is an exposition of his thesis of semantic indeterminacy as it is manifested in translation between languages, and thus appears as his indeterminacy of translation thesis. In the translation situation he portrays the field linguist in the same situation that Carnap postulates in "Meaning and Synonymy in Natural Language", where Carnap attempted to describe how the field linguist can ascertain a term's intension by identifying its extension from the observed behavior of native speakers of an unknown language. Carnap admitted that this determination of extension involves uncertainty and possible error due to vagueness, but he excused this uncertainty and risk of error because it occurs even in the concepts used in empirical science. While this admission of extensional vagueness in science made the fact unproblematic for Carnap, it had just the opposite significance for Quine. For Quine extensional vagueness is an inherent characteristic of language that he calls "referential inscrutability", and which he later calls "ontological relativity." And what Carnap called the intensional vagueness, Quine prefers to consider as a semantical indeterminacy in stimulus meaning but without admitting intensions.

Quine rejects Carnap's thesis of intensions, explicates his own theory of meaning in terms of behavioristic psychology, and proposes his doctrine of "stimulus meaning." Stimulus meaning is a disposition by the native speaker of a language to assent or dissent from a sentence in response to present stimuli, where the stimulus is not just a singular event but rather a "universal", a repeatable event form. Stimulus meaning is the semantics of

those sentences that Quine had earlier described metaphorically as positioned at the edge of the system of beliefs viewed as a force field, as opposed to the more theoretical sentences that are in the interior of the field. In Quine's philosophy the idea of stimulus meaning is not a special semantics, but rather is an attempt to isolate the net empirical content of each of various single observation sentences without regard to the theory that contains them yet without loss of what the sentence owes to that containing theory. This attempt to isolate the semantics of observation language is a move away from his earlier critique of reductionism, where reductionism is understood as statements having a unique range of possible sensory events, such that the statements can be criticized in isolation. But at this stage Quine still retains his original thesis of empirical underdetermination, in which empirical underdetermination is integral to his wholistic thesis of semantical indeterminacy or vagueness.

The underdetermination thesis admitting multiple and alternative observation sentences for the same stimulus situation presents a question: how can the same stimuli yield alternative stimulus meanings? One of Quine's answers is that the alternative theories or belief systems in which the stimulus situation is understood, supply different significant approximations. But there still remains the question of how stimulus meanings are to be construed as approximations. Quine has a theory of vagueness that he sets forth in the third and fourth chapters of *Word and Object*, which resembles the latter Wittgenstein's thesis of paradigms, except that Quine explicitly invokes the behavioristic stimulus-response analysis of learning. On this analysis Quine rejects the view that stimulations eliciting a verbal response "red" are a well defined or neatly bounded class. He maintains that the stimulations are distributed about a central norm, which when a language is initially being learned, may be a very wide distribution. The penumbral objects of a vague term are the objects whose similarity to those for which verbal response has been socially rewarded in the learning process, is relatively slight. The learning process is an implicit induction on the part of the subject regarding society's usage, and the penumbral cases are those words for which that induction is most inconclusive for want of evidence, because the evidence is not there to be gathered. And society's members have had to accept similarly fuzzy edges when they were learning. There is an inevitability of vagueness on the part of terms learned by ostension, and it carries over to other terms defined by context on the basis of these ostensively learned terms.

CARNAP AND QUINE

Since Russell Hanson's *Patterns of Discovery* (1958) the participation of theoretical concepts in the semantics of observation language is often expressed by saying that observation is "theory-laden." And this semantical participation of theory in observation has made problematic the objectivity of observation, and therefore the decidibility of scientific criticism. In 1968 in "Epistemology Naturalized" in *Ontological Relativity* Quine states that Kuhn and Hanson among others have tended to belittle the role of evidence in science and to accentuate cultural relativism, and that such philosophers represent a wave of epistemological nihilism. He notes Hanson maintains that observations vary from observer to observer according to the amount of knowledge that the observers bring with them. Thus one man's observation is another man's closed book or flight of fancy, with the result that observation as the impartial and objective source of evidence for science is bankrupt. At this stage of Quine's thinking the semantical contribution of theory to observation is still problematic for him, but he continued to characterize observation language in terms of behavioristic theory of learning. In the chapter titled "Observation" in his *The Web of Belief* (1970) Quine says that an observation sentence is a sentence that can be learned ostensively by the association of heard words with things simultaneously observed, an association which is conditioned and reinforced by social approval or successful communication, and which becomes habitual. And due to the social character of its learning, the observation sentence must be understandable by all competent speakers of the language who might be asked to assent to it. Thus according to Quine the sentence "That is a condenser" is not an observation sentence, even if experts agree to it. Quine maintains contrary to the Positivists, that what qualifies a sentence as observational is not the lack of theoretical terms that may occur in theory formulations, but just that the sentence taken as an individual whole commands assent consistently or dissent consistently when the same global sensory stimulation is repeated. This behavioristic characterization initially enabled Quine to evade reference to semantics in his identification of observation language, and thereby to separate his view from that of the Positivists, who defined observation language in semantical terms. But in attempting to avoid a cultural relativist view of truth he thought he found in the likes of Hanson, Quine found himself getting back into the semantics of observation with the very Positivist objective of keeping the semantics of observation uncontaminated by that of theory.

After *Word and Object* and *Web of Belief* Quine further developed the Duhem-Quine thesis in his "On Empirically Equivalent Systems of the

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World" in *Erkenntnis* (1975), which as it happens had in 1930 been made the official journal of the Vienna Circle. This development of the Duhem-Quine thesis represents a further restriction on Quine's earlier version on his wholistic semantical thesis of observation. Previously he had viewed empirical underdetermination as integral to semantical indeterminacy or vagueness in his semantical wholism. But in this paper he revises the concept of empirical underdetermination of language, and separates it from the wholistic view of the Duhem-Quine thesis. The scientific hypotheses that purport to describe things beyond the reach of observation are related to observation sentences by a kind of one-way implication, such that many alternative hypotheses may imply the same set of observation sentences, but not vice versa. Observation sentences do not uniquely imply just one theory purporting to explain the observable events. It now is in this sense that natural science is "empirically underdetermined" by all possible events. Quine says that underdetermination lurks where there are two irreconcilable theory formulations each of which implies exactly the desired set of observation conditionals plus extraneous theoretical matter, and where no formulation affords a tighter fit. In Quine's vocabulary the phrase "observation conditional" is an empirical generalization expressed in conditional form and implying an observation sentence describing an individual event. And his phrase "theory formulation" is a conjunction of the axioms of a deductive theory, which implies observation conditionals. This is a different sense of "empirical underdetermination" than what Quine meant in "Two Dogmas", because it resurrects the idea of a semantically neutral observation language, which philosophers such as Hanson, Kuhn and Feyerabend reject. These philosophers find a phrase such as "same observation sentences" when speaking of sentences implied by alternative theories to be very problematic; they deny that different theories can have the same set of observations due to the contribution of the semantics of theory to the semantics of observation language.

Having revised "empirical underdetermination", Quine then distinguishes his revised concept from the wholistic doctrine of the Duhem-Quine thesis. He reiterates that the wholistic doctrine says that scientific statements are not separately vulnerable to adverse observations, since it is only jointly as a theory that they imply their observable consequences, with the result that any one of the statements can be adhered to in the face of adverse observations by revising others. Then he states that wholism lends credence to the underdetermination thesis, because in the face of adverse observations we are free always to choose among various adequate

modifications of our theory, and all possible observations are insufficient to determine theory uniquely.

Also in this work Quine considers several criticisms or "reservations" about the wholism of the Duhem-Quine thesis, and in his defenses he will pick and choose between underdetermination (revised) and wholism (unrevised). The first criticism is that some statements closely linked to observation are separately susceptible to tests of observation, while at the same time these statements do not stand free of theory because they share much of the vocabulary of the more remote theoretical statements. Quine answers that the Duhem thesis does not imply equal status for all statements. He says that the Duhem thesis applies even for observation statements, since scientists do occasionally revoke observation statements when these statements conflict with a well attested body of theory, and when the experiment yielding the observation cannot be replicated. This is such a weak concession to semantical wholism and the indeterminacy of observation, that it effectively limits wholistic theory participation in the semantics of observation language to the status of errors of observation.

A second reservation pertains to the breadth of the theory: If it is only jointly as a theory that scientific statements imply their observable consequences, then how inclusive does that theory have to be? Does the wholistic scope have to include the whole of science taken as a comprehensive theory of the whole world? Quine sees science as an integrated system of the world as science exists at any point in its historical development, but unlike the Positivists he does not view it as integrated by reductionism into a single unified science. He says that Duhem wholism admits that science is neither discontinuous nor monolithic, but as variously joined and loose in its joints in varying degrees. Later in "Five Milestones" Quine elaborates on this idea by saying that all sciences interlock to some extent not only due to a common logic and mathematics, but also because small "chunks" may be ascribed their independent empirical meaning nearly enough, since some vagueness in meaning must be allowed for. This defense based on vagueness calls upon the semantical indeterminacy that enables wholism.

A third reservation is that the semantical and ontological wholism may imply a cultural relativistic view of truth. Quine denies that his wholism implies a cultural relativistic view of truth. His first argument is external to the wholistic thesis. He finds a paradox in the thesis of cultural relativism: if truth were culture bound, then the advocate of cultural relativism ought to see his own culture-bound truth as absolute. The cultural

relativist cannot proclaim cultural relativism without rising above it, and he cannot rise above it without giving it up. Quine then turns to the issue of irrationality of theory choice, the argument for cultural relativism that is internal to wholism. He argues that the choice between empirically equivalent alternative systems need not be irrational; he says he will settle for a "frank dualism." He says that oscillation between rival theories is standard scientific procedure, because it is thus that one explores and assesses alternative hypotheses. In this defense Quine switches between underdetermination and wholism. Rationality of theory choice is based on comparability of theories permitted by a neutral observation language, that is admitted by Quine's revised underdetermination thesis, since it is only theories and not observations that are incompatible. The dualism is therefore merely one due to empirical equivalence. But the idea of empirical underdetermination as newly revised in this article is not the context in which the issue of irrationality of theory choice emerges. It emerges in the context of wholism where theory participates in the semantics of observation language. Quine switches to the wholistic context, when he says that what-ever we affirm, we affirm as a statement within our aggregate theory of nature as we now see it, and that there is no extratheoretic truth. Quine's frank dualism has not been very frank in this defense. Quine's revised concept of empirical underdetermination is not consistent with his semantical wholism. The revised concept of underdetermination permits a neutral observation language, while the Duhem-Quine wholism continues to permit theory to resolve the vagueness in the semantics of observation language.

Quine eventually recognized this inconsistency. Just as he imposed logical one-way restrictions for his revised concept of empirical underdetermination, he found that he must impose semantical one-way restrictions in the semantical wholism of the Duhem-Quine thesis. In his "Empirical Content" (1981) in *Theories and Things*, which he notes contains "echoes" from "Empirically Equivalent Systems of the World", Quine explicitly uses Hanson's terminology saying that observation sentences are "theory-laden." But Quine reconstrues the intended meaning of Hanson's phrase to mean that the terms embedded in observation sentences may recur in theory formulations. Thus while Quine here says that observation sentences are theory-laden, he denies to the semantics of theory any participating role in the semantics of observation. In fact in Quine's construing of "theory-laden" it is not observation language that is theory-laden, but rather theory that is observation-laden. At least he did not revert

to the old Carnapian reduction sentences, to make theory observation-laden. Still later in "Truth" in his *Quiddities* (1988) he is explicitly reconciled about refusing to admit theory any resolving function in the semantics of observation. There he says that we work out the neatest world system, and we tighten the squeeze by multiplying the observations. Tightening the squeeze in observation sentences is the progressive reduction of vagueness but only by the addition of information in additional observation sentences. Quine's limitation on which contexts may resolve vagueness and which ones may not is arbitrary and *ad hoc*. His wish to make observation sentences semantically uncontaminated by theory is a Positivist atavism, even though his motivation is not characteristically Positivist. His point of departure was not a preconceived semantics for observation; he attempted a behavioral (behavioristic) characterization of observation language instead. Still, he believed that an unrestricted wholistic, theory-dependent, context-determined semantics encompassing both theory and observation language implies a relativistic and subjectivist philosophy of truth. Fear of a relativistic view of truth led him to revise his original version of his Duhem-Quine thesis.

Quine the logician always saw theory language as an axiomatic system with observation language serving as its derived theorems. For Quine, Isaac Newton's mechanics is still "theory" today. On the Pragmatist concept of scientific theory, however, theory language is identified not by contrast to an observation semantics or by semantics at all, but by reference to its function or pragmatics in science: it is discourse that is proposed for testing in contrast to that which is presumed for testing. Thus, observation language need not be exclusively identified as either theory or nontheory language (unless the Pragmatist simply chooses to define "observation" correlatively to his functional definition of "theory"). And all contexts consisting of explicitly or implicitly universally quantified sentences believed to be true operate to resolve the vagueness in the meanings of their common univocal terms. Quine's view is not a Pragmatist view of theory based on the function of theory in empirical basic science, but is better characterized as an archival concept of theory, or what Hanson called an "almanac" view. Correspondingly his concept of observation language is an archival concept of observation language. Quine believed that this archival view would enable him to make observation language a repository of permanent truth. And his motive is his wish to evade the relativistic view of truth, which he believed is implied by the unrestricted context determination of semantics.

More recently a member of Quine's intellectual entourage, Donald Davidson, has attempted to evade semantical relativism with a turn to instrumentalism. Davidson's principal statement of his thesis is set forth in his "The Very Idea of a Conceptual Scheme" (1974) and "Belief and the Basis of Meaning" (1974) reprinted in his *Inquiries into Truth and Interpretation* (1984), a book he dedicates to Quine with an inscription "without whom not." He rejects the representationalist view of the semantics of language, which he considers a third dogma of empiricism after the first two referenced by Quine in the latter's 1952 "Two Dogmas" article. Like Dewey's rejection of the dualism of "experience" and "nature" Davidson rejects the dualism of "scheme" and "world", of "conceptual scheme" associated with language and "empirical content", of "organizing system and something waiting to be organized", that he finds in the views of Whorf, Kuhn, and Feyerabend. In this manner he remains more faithful to Quine's original behaviorism than Quine did. Given the mutual and reciprocal determination of between belief and semantics, the decision necessary for interpreting another's discourse is to maximize our shared beliefs, such that there can be no basis for concluding that others have concepts or beliefs radically different from one's own. Davidson concludes that in giving up the dualism of scheme and world, we do not give up the world, but rather re-establish "unmediated touch" with the familiar objects that make our sentences and opinions true or false. Thus Davidson argues that there is no conceptual relativism, because there are no conceptual schemes to be relativistic.

But Davidson's conclusion is a *non sequitur*. Firstly he confuses two distinct questions: one is the question of what is meaning, and the other is the question of what is the meaning of a term, sentence, or theory and how is this determination made. The existence of conceptual schemes is an answer to the former question, and his behavioristic procedure is his answer to the latter one. The answers are made interdependent only because Davidson is a behaviorist, which is to accuse him of being a Positivist. And his Positivism makes him inconsistent with Quine's and his acceptance of ontological relativity, because Positivism requires a prior ontological commitment. Davidson does not practice ontological relativity in his own philosophical discourse. Secondly the word "unmediated" in his phrase "unmediated touch", which purportedly justifies his denying language its representational semantics, is a weasel word. In fact the interpreter's charitable decision required for interpretation does not imply any rejection of the representational nature of the semantics of language. This interpretative

decision is operative when someone uses a dictionary with the charitable assumption that its lexical entries are true, so that he can assimilate the meanings of the terms he is researching. And also when a community of scientists in a profession considers an experiment and agrees on the validity of the test design statements, so that the scientists can describe the phenomenon under examination and the experiment's outcome. Neither the thesis of the charitable decision required for communication nor the thesis of the interdependence between truth nor meaning imply any rejection of the representational nature of the semantics of language; representationalism is perfectly consistent with both theses. "Representation" may be a weasel word, because there survives an atavistic belief residual from modern philosophy including Positivism, that the knower is a spectator to his ideas. Of course the knower can be a spectator of his ideas, but this inspection is a reflection *ex post facto* to his firstly already having the inspected knowledge of the real world. Apart from this secondary reflective knowledge, the spectator thesis about knowledge of the real world is readily rejected, when we realize that what we know firstly is not our ideas, but the real world, and most notably that our knowledge is thus constituted by our ideas rather than the ideas being an object of knowledge. Contrary to Davidson, therefore, these and their schemes are quite admissible, and they very much involve semantical relativism.

Both Quine and Davidson are motivated to evade semantical relativism, because both mistakenly believe that a relativistic, context-determined, semantics implies a relativistic thesis of truth. Regardless of how culture-bound and context-determined may be the semantics of a language, it is not possible capriciously either to affirm or to deny truthfully just anything expressed by sentences made with those concepts. The empirical underdetermination of language implies that many alternative sentences can be said which are consistent with the same observations. Still, the empirical constraint imposed exogenously on sentences by the recalcitrant real world - even when not yet interpreted - forbids just any arbitrary distribution of truth-values over a set of logically related, semantically interpreted grammatical sentences. When any subset of these sentences is given definitional force to specify its semantics, then only some of the remainder sentences containing the same descriptive terms can also be true. Truth is always relative to what is said, but the real world in which all language users live forbids ingenuously asserting just any old thing in the semantically interpreted language. Therefore, semantical relativity does not imply relativism of truth, but just the opposite: with a metatheory of

semantical description exhibiting the composite nature of meanings, semantical relativity explains the partial equivocation that makes it impossible for the same sentences occurring in two different belief systems, to be completely true in one belief system and completely false in another. It explains how the same sentence is not simply and completely the same statement in each system, but is partially the same in each, and to that extent true in both systems. And for the same reason it also explains why the semantics of observation language need not be quarantined from the semantics of theory, in order to assert the objectivity of truth. Observation statements, which pragmatically defined are merely singular test design statements, may be common to pragmatically defined contrary theories, such that belief in the test design statements makes the test outcome contingent and not willfully or necessarily verifying, and makes a falsifying test outcome of one of the theories an objective truth.

Each person acquires the semantics of what Quine calls observation sentences from his own personal experiences, and he acquires it publicly and ostensively in the circumstances of his learning situation in his personal history. There is a wide variation among people between what is learned ostensively and contextually, but even for those simple statements learned ostensively by most people, intersubjectivity is increased with successive approximation, as the web of belief grows and imposes increasingly more shared truth conditions on the ostensively acquired semantics. The entire web of beliefs may be viewed on analogy with an underdetermined system of conditional equations, in which the addition of a new equation further restricts the range of numeric values that the set of variables may accept as solution sets. One difference between the mathematical system and the language system is that with just a sufficient number of restrictions the equation system may admit to only one solution set, whereas language is never restricted to a unique interpretation. Another noteworthy departure from the mathematical analogy is that the mathematical variables can take only one numeric value at a time without becoming ambiguous, while each of the descriptive terms, including those used as mathematical variables in applied mathematics in empirical science, simultaneously take on the semantic values distinguishable in the explicitly related universal statements in the system of beliefs, subject only to the preservation of univocity. Thus all the terms explicitly related by the sentences in the web of beliefs may participate in one another's univocal semantics, and thereby resolve one another's vagueness in relation to each other. Furthermore as implicit

statements are made explicit by deduction, the vagueness in the meanings of the terms of the system is even further resolved.

But Quine viewed meanings as abstract or mental "entities", and then developed his behavioristic theory of stimulus meanings, which he called "behavioral dispositions" to evade the representative function of language. He could not be expected to have developed a metatheory of semantical description enabling him to describe how meanings participate in one another. The closest Quine came to the idea of semantical participation was the idea of the resolution of vagueness. His rejection of the dichotomous analytic-synthetic distinction is a worthy start toward such a metatheory, but his rejection of the distinction was actually a rejection of analyticity as such, except in the cases that he called "analytical hypotheses" used for translations. As it happens, rejection of the analytic-synthetic dichotomy does not imply the rejection of analyticity as such. Universally quantified statements believed to be true for empirical reasons may also be used analytically to exhibit the complexity in the meanings of their constituent terms by displaying their component semantic values that constitute the discriminating capability in the descriptive function of the language. In other words all universal empirical statements in the web of beliefs are analytical hypotheses. And theories are those that are viewed as relatively more hypothetical than other empirical statements.

Quine's Critique of Analyticity

The fourth of the five milestones that Quine finds in the history of empiricism is the abandonment of analyticity in the traditional analytic-synthetic dichotomy. He calls his exclusive acceptance of synthetic statements "methodological monism." The rejection of analyticity is one of the earliest theses in Quine's philosophy of language. In his *Dear Carnap, Dear Van* Creath reports that when Quine had first met Carnap in March 1933, Quine was reading the manuscript for Carnap's Logical Syntax as Carnap's wife was typing it. Creath notes that a brief shorthand note later found among Carnap's archived papers reveals that Quine had asked whether or not the difference between the analytic axioms of arithmetic and the synthetic empirical claims about physical bodies is merely a difference of degree, which reflects our relative willingness to abandon the various beliefs under consideration. Quine's first published statement of the rejection of the traditional analytic-synthetic distinction is in his "Truth by

Convention" (1936) originally in *Philosophical Essays for A.N. Whitehead*, and later reprinted in his *Ways of Paradox*. Analytic statements are those that are true by linguistic convention, and they include the propositions of logic and mathematics. Essentially his argument in this paper is based on the rejection of an infinite regress; he argues that some logic is needed and is presupposed to develop logic. Thus he asks whether or not it makes any sense to say that the truths of logic and mathematics are destined to be maintained independently of our observation of the world, so that truth by convention may apply.

Fifteen years later Quine's critique of analyticity took a different tack in "Two Dogmas", where he formulated the Duhem-Quine thesis of semantical wholism, and attacked linguistic synonymy upon which analyticity is based. The statement "No bachelor is married" is made analytic by substitution of synonyms "bachelor" and "unmarried man" in the statement "No unmarried man is married", because the latter statement is true in all interpretations of its nonlogical or descriptive terms. Quine notes that Carnap explained analyticity by appeal to state descriptions; a statement is analytic if it is true in all state descriptions. Quine says that appeal to state descriptions works only if the atomic statements of the language are mutually independent, i.e. if the language has no extralogical synonym pairs such as "bachelor" and "unmarried man." Thus on Quine's thesis, Carnap's criterion for analyticity in terms of state descriptions is a reconstruction at best of logical truth, not of analyticity. Quine argues that all instances of synonymy except those occurring in purely stipulative definitions introducing notational abbreviations are based on observed synonymy occurring in natural language. These include synonymies occurring in reduction sentences, analytic sentences and Carnap's semantical rules; and they all depend on the thesis contrary to Duhem's thesis, that it is possible to determine the truth or falsehood of sentences in isolation from one another. Invoking Duhem's thesis Quine rejects the distinction between a factual component and a linguistic component in the truth of any individual statement, which is the basis for the analytic-synthetic distinction.

Shortly after writing "Two Dogmas" Quine wrote "Carnap and Logical Truth" (1954) in *Philosophy of Rudolf Carnap* (1963). This critical essay's most distinctive characteristic relative to Quine's prior essays is its treatment of the effects of linguistic and scientific change on analyticity and logical truth. Carnap's interest in philosophy was originally inspired by Einstein's use of non-Euclidian geometry and by Hilbert's formalistic approach to mathematics. Quine says that the initial tendencies to treat

geometries as true by convention together with the tendency toward formalization were extended to mathematical systems generally. But Quine maintains that formalist mathematics has been "corrupted" by supposing that postulates are true by convention, and he rejects the idea of semantically uninterpreted postulates. Quine treats the subject of postulates in a manner similar to his earlier treatment of definitions in "Two Dogmas." He distinguishes two types of postulates: "legislative" and "discursive." The former type is a stipulative definition that merely introduces previously unused notation, and it initiates truth by convention. Discursive postulation on the other hand is a selection from a pre-existing body of truths, of certain ones for use as a basis from which to derive others initially either known or unknown. Most notably what discursive postulation fixes is not truth, but only some particular ordering of the truth. All postulation may be said to be conventional, but only legislative postulation admits to truth by convention. The importance of the distinction, however, is that it refers to an act and not to any enduring consequences. The conventionality in postulation is a passing trait, which is significant at the moving frontier of science, but which is useless in classifying the sentences behind the lines. Conventionality is a trait of events and not of sentences. And if legislative postulates are subsequently singled out in some later exposition, they have the status of discursive postulates in the subsequent exposition. The artificiality of legislative truth does not linger as a localized quality, but suffuses with the corpus and becomes integral with it. Quine does not explicitly reference Duhem in this context, but Duhem's wholism is clearly operative. Quine says that legislative postulation occurs continually in the theoretical hypotheses of natural science. The justification of any theoretical hypothesis can at the time of hypothesizing consist in no more than the elegance or convenience which the hypothesis brings to the containing body of laws and data. There is indirect but eventual confrontation with empirical data, but this can be remote. Furthermore, some such remote confirmation with experience may be claimed even for pure mathematics and logic. A self-contained theory that can be checked with experience includes not only its various theoretical hypotheses of so-called natural science, but also such portions of logic and mathematics as it makes use of. There is no line to be drawn between hypotheses that confer truth by convention and hypotheses that do not; logic and mathematics are not qualitatively different from the rest of science.

Quine elaborates by illustration: Suppose a scientist introduces a new term for a certain substance or force by an act of legislative definition or

postulation. Progressing, he then evolves hypotheses regarding further traits of the named substance or force. And then further progressing he identifies this substance or force with one named by a complex term built up of other portions of his scientific vocabulary. This new identity will figure in the ensuing developments quite on a par with the identity which first came by the act of legislative definition, or on a par with the law which first came by the act of legislative postulation. And revision in the course of further progress can touch any of these affirmations equally. Quine says that scientists proceeding in this way are not slurring over any meaningful distinction. Legislative acts occur routinely. Carnap's dichotomy between analytic and synthetic, between truth by meaning postulate and truth by force of nature, has no clear meaning, even as a methodological ideal. The fabric of our sentences, our web of beliefs as Quine calls them later, develops and changes through more or less arbitrary and deliberate revisions and additions of our own, more or less directly occasioned by the continuing stimulation of our sense organs.

Carnap replies at the end of the volume in which Quine's critique was published. He emphasizes that his explication of "analytic" has always been for a formalized language, one for which explicit semantical rules are specified and that lead to the concept of truth. He rejects Quine's demand that semantical concepts such as analyticity and synonymy must also be explicated pragmatically by an empirical criterion in behavioristic terms applicable to natural language. He therefore maintains that Quine's objections are not directed against his semantical *explicata*, and that A-truth is not objectionable. Carnap then turns to Quine's critique of analyticity in situations where there is a change in artificial language, from *L(n)* to *L(n+1)*. Firstly Carnap agrees with much of what Quine says in "Two Dogmas", where Quine sets forth his neo-Duhemist wholistic thesis. Carnap agrees that a scientist who discovers a conflict between his observations and his theory and who must therefore make a readjustment somewhere in the total system of science, has much latitude with respect to the places where a change is to be made. Remarkably Carnap also agrees that in this procedure of readjustment, no statement is immune to revision, not even statements of logic or mathematics. But Carnap rejects Quine's characterization of an analytic statement as one held true come what may. And Carnap furthermore denies that a change in language invalidates the analytic-synthetic distinction. In defense of analyticity Carnap distinguishes two types of linguistic change. The first type is a change of language from *L(n)* to *L(n+1)*. He says that this type constitutes a radical alteration and perhaps

a revolution. It occurs only at certain historically decisive points in the development of science. The second type is a mere change in or an addition of a truth-value ascribed to an indeterminate statement. An indeterminate statement is one having a truth-value that is not fixed by the rules of the language, i.e. by postulation of logic, mathematics, or perhaps physics. This second type of change occurs "every minute" according to Carnap. He says that his concept of analyticity has nothing to do with the first type of transition; his concept of analyticity refers only to some given language, *L(n)*. The truth of a sentence, *S*, in *L(n)* is based on meanings in *L(n)* of the terms occurring in *S*. In *L(n)* analytic sentences cannot change their truth-value, and furthermore neither can the synthetic postulates of physics and their logical consequences.

Quine's critique of analyticity is principally directed against what Carnap called A-truth, which is truth based on the semantics of the descriptive vocabulary in the sentence, a lexical basis. As a symbolic logician Quine continues to rely on logical truth, on the kind of sentence that Carnap calls L-truth, but his reasons are different than Carnap's. In "The Ground of Logical Truth", the eighth chapter in his *Philosophy of Logic,* Quine admits to an acceptable sense of logical truth, the truth that is evident due to the grammatical structure of the logically true sentence. But Quine rejects Carnap's doctrine of linguistic truth, the thesis that language alone can make logical truth independently of the nature of the world. In view of Carnap's defense of analyticity, it is doubtful that Carnap continued to maintain such a view. In any event, Quine maintains that the validity of logical truth depends on the relation of grammatical structure to the structure of the real world. He argues that the distinction between the lexical and the grammatical is variable not only among different languages, but also within the same language.

Quine's Rejection of First Philosophy

Quine's taking Whitehead's comment that logic shapes metaphysical thought beyond logic and making it his general theory of language, has another and even more important implication: Quine's thesis of ontological relativity. Thus the last of the five milestones in Quine's history of empiricism is what he calls the abandonment of the goal of a first philosophy. By first philosophy he means any philosophy that is prior to natural science. Traditionally metaphysics and epistemology are considered

to be first philosophy. Quine calls his position "naturalism." The term "naturalism" has meant many different things in the history of philosophy. A term that Quine does not use is "scientism." In "Five Milestones" Quine defines his naturalism as the view that natural science is an inquiry into reality, a fallible and corrigible inquiry, but not answerable to any suprascientific tribunal, and not in need of any justification beyond observation and the hypothetico-deductive method. This statement by Quine is not merely an affirmation of the autonomy of empirical science from metaphysics, as may be found in Duhem's philosophy of science. Quine rejects the view that there is any philosophical tribunal for science, by which he means any knowledge separate from empirical "common sense" that he views to be continuous with science in his wholistic philosophy of language. Furthermore, Quine maintains that epistemology is an empirical discipline that he assimilates into empirical psychology, which for him is behavioristic psychology.   He describes the scientific epistemologist as asking how animals, presumably human, can have managed to have arrived at science from the limited information from surface stimulations, and as pursuing this inquiry to yield an account that pertains to the learning of language and the neurology of perception.

Quine gives two reasons for his naturalism by which he rejects all first philosophy. One reason is what he calls an "unregenerate" realism, the robust state of mind of the natural scientist who has never felt any qualms beyond the negotiable uncertainties internal to his science. He expresses his realism even more emphatically in his "Scope and Language of Science" (1954) reprinted in *Ways of Paradox*. There he states that we cannot significantly question the reality of the external world or deny that there is evidence of external objects in the testimony of our senses. For to do so is to dissociate the terms "reality" and "evidence" from the very application which originally did most to invest these terms with whatever intelligibility they may have for us. He maintains that the notion of reality independent of language is derived from our earliest impressions, and then carried over into science as a matter of course. The second reason for Quine's realism is what he calls the despair of being able to define theoretical terms generally in terms of phenomena even by contextual definitions. This is a rejection of the Logical Positivist problem for which reductionism of theoretical terms was thought to provide an answer. On the Positivist philosophy there is no justification for affirming the reality of theoretical entities, unless these terms are firstly established as semantically meaningful. The purported solution is the reduction of theories to observation sentences, which are the

source for the semantics and ontology of theories. Quine rejects the Positivists' problem, because it involves a prior ontology or first philosophy consisting in the Positivists' observation language. In Quine's view Positivism is a kind of metaphysics, Positivists' antimetaphysical rhetoric notwithstanding.

Fundamental to Quine's second reason for rejecting first philosophy is his thesis of ontological relativity. This thesis can be found in Quine's literary corpus even before he came to call it "ontological relativity" in the mid-1960's. In "Two Dogmas" after rejecting the dogma of reductionism, he says that physical objects are conceptually imported into the linguistic system as convenient intermediaries, as irreducible posits comparable epistemologically to the gods of Homer. What he calls the "myth" of physical objects is epistemologically superior to others including the gods of Homer, in that it has proved to be more efficacious than other myths as a device for working a manageable structure into the flux of experience. Microphysical entities are posited to make the laws of macroscopic objects and ultimately to make the laws of experience more manageable. Science is a continuation of common sense, and it continues the commonsense expedient of swelling ontology to simplify theory. Shortly later in "Posits and Reality" (1955) Quine says that if we have evidence for the existence of bodies of common sense, we have it only in the way in which we may be said to have evidence for the existence of molecules. All science is empirically underdetermined, and the only difference between positing microphysical and macrophysical entities is that the theories describing the former are more underdetermined. In this context Quine is using the term "underdetermined" in same sense as he used it in "Two Dogmas" to express his neo-Duhemist wholistic view of language.

The thesis of ontological relativity is also prefigured in *Word and Object*. Just as Carnap recognized extensional vagueness, Quine recognized referential indeterminacy, which he calls referential "inscrutability." Inscrutability of reference is due to the semantic indeterminacy of direct ostension. This indeterminacy is encountered when the field linguist attempts to translate a previously unknown language, but it also occurs more generally in all language, and is not distinctive of the translation situation. The context-dependence of semantics makes reference and ontology completely system-determined in the linguistic context that determines the semantics of a discourse including notably the context constituted by a scientific theory. In chapter six of *Word and Object* Quine says that everything to which we concede existence is a posit from the standpoint of

the theory-building process, and is simultaneously real from the standpoint of the theory that is built. His phrase "ontological relativity" itself is set forth in "Ontological Relativity" (1968) in *Ontological Relativity*. Quine uses the phrase explicitly on analogy with Einstein's relativity theory in physics. He maintains that reference is nonsense except in relation to a coordinate system, where the coordinate system is some background language. Asking for ontological reference in any more absolute way than by reference to a background language is like asking for absolute position or absolute velocity, rather than for position or velocity relative to a frame of reference. The ultimate background language to which we take recourse in practice is our mother tongue, in which we take words at face value with their primitively adopted and ultimately inscrutable ontology. Any subordinate theory must be interpreted by reference to this home language. Quine opposes his thesis of ontological relativity to Carnap's thesis of the distinction between external ontological questions and internal factual questions set forth in "Empiricism, Semantics and Ontology." In Quine's view there can be nothing like Carnapian external questions which are external to the home language. In "Carnap's Views on Ontology" (1951) reprinted in *Ways of Paradox* Quine maintains that ontological questions are on a par with questions in natural science. Within science there is a continuum of gradations from the statements that report observations to those that reflect basic features of quantum theory and relativity theory. Similarly statements of ontology and even of mathematics and logic form a continuation of this continuum, though these are more remote from observations than the central principles of quantum theory or relativity theory. Quine says that the differences along this continuum are only differences of degree and not differences in kind.

While the semantical wholism of the Duhem-Quine thesis has received much attention, it is seldom realized that Quine's rejection of all first philosophy is one of its most consequential implications for philosophy of science. When the Duhem thesis of physical theory is extended to the whole of language, not only is all semantics made context-determined, but also all ontologies described by the semantics are made vulnerable to empirical criticism; there are no longer any privileged or protected ontologies. Quine's thesis of ontological relativity has the historic and revolutionary effect of excluding all ontological considerations from the criteria for scientific criticism. In his philosophy it is empirical adequacy of scientific theories that decides ontological questions, rather than prior ontological commitments that decide the acceptability of scientific theories.

CARNAP AND QUINE

Quine subordinates all questions of ontology to the empirical adequacy of the theory affirming the ontological claims in question. He maintains that the human knower can never do better than to occupy the standpoint of one or another theory, whether the theory purports the existence of either macrophysical or microphysical entities. All entities are "posits" affirmed by one or another theory, and all are worthy of our patronage just to the extent that the theory positing them is empirically adequate. However detailed may be the relevant observation language, empirical underdetermination (in Quine's earlier sense) and its consequent semantical indeterminacy always admit alternative choices of theory. And the consequent referential inscrutability may admit to as many correspondingly alternative choices of entities.

Quine's rejection of prior ontological criteria in scientific criticism is also consistent with scientific realism, which gives the tested and nonfalsified explanation the role of defining ontology. Realism is not established by science; it is a prior prejudice. But science lets empirical justify the ontological claim that the explanation describes the real world. This thesis is not only characteristic of the contemporary Pragmatist philosophy, but was also the practice of Galileo, Einstein and Heisenberg. In developing his theory of relativity Einstein posited relativistic time as real instead of Newton's absolute time, and he rejected Lorentz's relegation of relativistic time to the status of apparent time and Lorentz's retention of Newton's absolute time as real. A central thesis of the Copenhagen interpretation, or at least Heisenberg's noninstrumentalist version, is its realistic claims about the wave-particle dualism and the indeterminacy principle, and Heisenberg referenced Einstein's realism in relativity theory as a precedent. However, the Copenhagen wave-or-particle dualism thesis cannot be affirmed on the basis of the mathematical equations of the quantum theory, since the mathematical expression has no syntactical categories for referencing entities. And Heisenberg's *potentia* ontology for the indeterminacy relations is also an added ontological claim about entities no less so than deBroglie-Bohm deterministic pilot-wave-and-particle ontology making the indeterminacy relations due to errors of measurement that are in principle correctable. The ontological claim justified by the empirical adequacy of the tested and nonfalsified mathematically expressed theory is limited to what the theory actually says, and the explanation is otherwise silent about ontology, and awaits further experimental findings. The practice of letting the empirical adequacy of a theory operate as the criterion for the acceptability of its ontology did not begin with Einstein or

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Heisenberg. A historic and well known example is Galileo's realistic interpretation of the Copernican theory, which placed him in conflict with the Aristotelian ontology enforced by the Roman Catholic Papacy.

This is a distinctively and thoroughly Pragmatist view that separates Quine from both his Positivist and Romanticist predecessors. Ironically it also separates him from certain other aspects of his own philosophy. One such aspect is his behavioristic epistemology. The Romanticists insist upon and the Positivists insist against the introduction of "mentalism" in explanations in the social and behavioral sciences. But on the contemporary Pragmatist philosophy of science, this ontological issue is decided by the empirical adequacy of the behavioral and social science theories. Different theories in different sciences at different times or even at the same time will admit different ontologies. Quine's behavioristic "naturalized" epistemology is actually an exception to his thesis of ontological relativity.

Another such inconsistent aspect is Quine's ontological reductionism and his consequent *de facto* nominalism. In his "Introduction" to his *Dear Carnap, Dear Van* Richard Creath states that Quine's ontological reductionist agenda was due to Quine's interpreting Carnap's *Logical Syntax* in a manner that was nearly wholly unintended by Carnap. Carnap argued in *Logical Syntax* that talk which appears to be about possibilities, properties, relations, numbers, etc. can be reconstrued to be talk about sentences, predicates, etc. Creath says that in Quine's "Lectures on Carnap", a prepublication report on the theses of *Logical Syntax* given to the Society of Fellows at Harvard in 1934, Quine had interpreted Carnap to mean that there are no such metaphysical entities, and that philosophy therefore is syntax as a program of ontological reduction. Creath states that in fact Carnap actually rejected both the affirmation and the denial of the existence of such metaphysical entities as properties, because Carnap believed at the time that such discourse is metaphysical nonsense. Later Carnap took a more pragmatic view of such entities as intensions and properties. But for the duration of his career Quine continued in his ontological reductionist agenda, which apparently resulted from his early misinterpretation of Carnap, notwithstanding Quine's later formulation of his ontological relativity thesis. This persistence is inconsistent; ontological relativity renders logical elimination for the purpose of ontological reduction a philosophically pointless exercise, because its acceptance implies the rejection of any and all prior ontological commitments that would motivate the ontological reductionism. Ontological relativity makes all ontological commitments *a posteriori* to empirical criticism, and together with the

empirical underdetermination of all theories results in ontological pluralism, not reductionism. But Quine is neither the first nor the last philosopher-king to exercise a sovereign's right of eminent domain in his own philosophy, and exempt his preferred convictions from his own laws.

Comment and Conclusion

Mach and Duhem were not only Positivist philosophers of science; they were also practicing research physicists, who furthermore wrote histories of physics. Carnap on the other hand was neither a practicing research physicist nor a historian of physics. His philosophical work was remote from the physicists' research practices, because the Vienna Circle had an epistemological (i.e. metaphysical) agenda for scientific criticism, which did not actually operate in research physics. Carnap aimed to construct a metalogic for science, but he did not apply his constructionalist techniques to the language used by scientists. Instead he used the symbolic logic of Russell and Whitehead to substitute for the object language that he claimed he was investigating. But the symbolic logic is not useful to the physicist. Carnap and others such as Russell and Braithwaite hailed the development of the Ramsey sentence as a great philosophical achievement. But it would be a rare physicist who would consider the Ramsey sentence at all consequential to either the practice or the history of physics. The situation is aptly stated by Radnitzky in the "Epilogue" in the first volume of his *Contemporary Schools of Metascience* (1968), where he says that the logical empiricists had not produced any metascience at all, because they did not study the producers of scientific knowledge or the production or even the results. The post-Positivist philosophers rejected Positivism because they correctly recognized its irrelevance to research science and its inadequacy as a philosophy of science.

When the post-Positivist philosophers rejected Positivism, many of them also rejected its constructionalism. Many Pragmatists in particular found their wholistic concept of the semantics of language incompatible with the mechanistic and procedural character of logical constructionalism. In the wholistic view the semantics of science makes the development of science a nonlogical process. But they rejected too much, because the Logical Positivists' linguistic-analysis approach is more valuable than either the Russellian symbolic logic or the Logical Positivist philosophy of science, which used the logic. In this age of the computerized discovery system

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Carnap's constructionalism and his metatheory of semantical systems may with certain noteworthy modifications be carried forward into contemporary and future methodology of science. Some such modifications are as follows:

1. A first important modification is that the object language that is constructed by a discovery system is not the Russellian symbolic logic; it is the mathematical equations or other technical language actually used in the relevant science. Scientists never use the Russellian symbolic logic for the expression of their theories, and Carnap's use of the symbolic logic to express empirical science was never more than a caricature. In his distinctive *Primer of Quantum Mechanics* Marvin Chester explicitly renders Dirac's notational conventions as descriptive language. Given Carnap's interest in physics, his philosophical linguistic analyses would have been infinitely more interesting had he chosen Dirac's operator calculus to illustrate the syntax, semantics, and pragmatics of an object language in science, especially with respect to his thesis of intensions and extensions. Carnap's philosophy might have evolved considerably in the process of developing such a linguistic analysis.

2. A second modification of Carnap's work is the use of a computer language for the metalanguage. The computer language gives the metalanguage a disciplined and procedural character that a colloquial metalanguage does not have. The computer language in which the discovery system is written operates as a metalanguage in which the formation rules of the object language are expressed in computer instructions. The discovery system in other words is a metalanguage expressing a mechanized generative grammar.

3. A third modification pertains to Carnap's concept of semantical rules that interpret a semantical system. The semantical rules for interpreting a mechanically generated semantical system might be viewed as analogous to Carnap's meaning postulates, in that all of them are stated in the object language instead of the metalanguage, and are not like Carnap's rules of designation, which occur in the metalanguage. Two relevant types of semantical rules may be distinguished. One type consists of those semantical rules that are the mechanically generated statements and equations. These consist only of the statements constituting a mechanically generated and empirically acceptable theory, the outputted theory statements that are believed to be true. But not all the semantical rules occurring in the object language are mechanically generated. A second type consists of test design statements, which are accepted independently of any statements of theory generated by the

system, so that the generated theory is not tautological and can be tested independently.

But the semantical rules for mechanically generated semantical systems are unlike Carnap's meaning postulates, because they are not just analytical sentences. With Quine's rejection of any distinctively analytic truth it is possible to view sentences as both analytic and synthetic, and the semantical rules that describe the semantical interpretation of the object-language statements must be viewed as both analytic and synthetic sentences. They are more like Quine's analytical hypotheses or discursive postulates. These semantical rules might also be viewed as similar to Carnap's reduction sentences, which he says determine only "part" of the meaning of theoretical terms. But Carnap has never explained how it is possible for the meanings of terms to have parts. Viewing the sentences as both analytic and synthetic enables the empirical statements constituting the generated theory to exhibit the parts of the meanings of their constituent terms, just as analytic statements always have. Test design statements and generated theory statements, both of which are believed to be true for empirical reasons and not due to the meanings of their constituent terms, are object-language statements functioning as semantical rules, each of which contribute parts to the meaning of each of their common descriptive terms.

4.   A fourth modification pertains to Carnap's idea of a state description. The Carnapian state description is not a useful concept for describing the semantical systems generated by mechanized discovery systems. In fact it is not useful for science at all. It consists of "atomic" statements expressed in Russellian logic, and was conceived with the intent of explicating precisely the ideas of L-truth and A-truth. The semantical systems generated by the discovery systems contain only universal statements constituting the theories generated with the formation rules in the computerized generative grammar. In contrast to the semantical systems in Carnap's philosophy, which were devised for static analyses, the semantical systems in metascience are intended to describe the semantical changes occurring in the development of new theories, which is a dynamic procedure. Accordingly the Carnapian idea of a state description must be revised for describing the computer system input and output object language, in order to reveal the semantical changes produced by the discovery system. The inputted information for the discovery system is drawn from the current cumulative state description consisting of the several theories that have been advanced to date by the

particular scientific profession. These theories supply the vocabulary inputted to the computerized discovery system. This vocabulary has its semantics specified by semantical rules consisting of test design statements, which are common to both input and output state descriptions. These test design statements are not changed by the discovery system, and they supply semantical continuity for identifying the subject of the theories independently of the theories. The computerized discovery system generates a set of outputted state descriptions consisting of alternative empirically adequate theories, which are semantical rules describing the semantics of the new theories.

5. A fifth modification consists of replacing Carnap's theory of information with Shreider's semantical metatheory, if the concept of state description as revised in the manner described above is identified with Shreider's concept of thesaurus. But unlike Shreider's theory there are actually two types of transformations involved. Firstly there is the mechanized syntactical transformation, the generation of new theories which are the output messages. And secondly there is also the semantical transformation on the part of the system users who communicate with the computer, when they attempt to interpret its output. The computer system is a transmitter and information source that generates message texts consisting of new theories. And the user receiving the message and having a thesaurus consisting of one of the input semantical systems, i.e. an old theory, must transform his thesaurus to conform to one of the output semantical systems, a new theory. Thus the amount of information transmitted to a user depends on the degree of transformation between his initial thesaurus and the outputted theory that must transform his thesaurus for him to understand the new theory.

The psychological resistance might be large, if the amount of information communicated is large. And there may also a philosophical resistance depending on the using-scientist's philosophy of science. If the scientist is a Romantic, he will be philosophically ill disposed to accept the newly generated theories containing large amounts of information, because he will find they are not "intuitively plausible" and do not "make sense." Romanticism retards the development of science, because it forbids the unfamiliar. The Positivist like Mach will be less affected by such philosophical cognition constraints. Positivists believe in the special importance of the familiar, which they call the "observable." But some, like Carnap, opposed "models" in which technologically less accessible microphysical processes are explained on analogy with more

familiar macrophysical processes. The philosophy of science that offers the least impediment to the reception of new information is Pragmatism, according to which no ontology may even serve as a criterion for scientific criticism.

WERNER HEISENBERG
AND THE SEMANTICS OF
QUANTUM MECHANICS

Werner Heisenberg (1901-1976) was born in Wurzburg, Germany, and studied physics at the University of Munich, where he wrote his doctoral dissertation under Arnold Sommerfeld in 1923 on a topic in hydrodynamics. He became interested in Niels Bohr's atomic theory and went to the University of Gottingen to study under Max Born. In 1924 he went to Bohr's Institute for Theoretical Physics in Copenhagen, where he developed the quantum matrix mechanics in 1925, and then developed the uncertainty principle in 1927. From 1927 to 1941 he was a professor of physics at the University of Leipzig. In 1932 he was awarded the Nobel Memorial Prize for Physics. In the Second World War, he was the director of the Kaiser Wilhelm Institute for Physics in Berlin. After the war he established and became director of the Max Planck Institute of Physics initially at Gottingen, and then after 1958 at Munich. His principal publications in which he set forth his philosophy of physics consist of the "Chicago Lectures of 1930" published as *The Physical Principles of the Quantum Theory* (1950, [1930]), *Philosophical Problems of Nuclear Science* (1952) currently published under the title of *Philosophical Problems of Quantum Theory* (1971), *The Physicist's Conception of Nature* (1955), an interpretative history of physics, *Physics and Philosophy: The Revolution in Modern Science* (1958), his intellectual autobiography published as *Physics and Beyond* (1971), and *Across the Frontiers* (1974).

Heisenberg's philosophy of science was not significantly influenced by the doctrines of professional philosophers, although he was a Positivist early in his career and later rendered Bohr's view of observation in neo-Kantian terms, even though neither he nor Bohr were metaphysical idealists. The formative intellectual influences on his philosophy were Einstein and Bohr. These two philosophical influences were contrary to each other, and each pulled Heisenberg's thinking in opposite directions.   Therefore, consider firstly the philosophical views of Einstein and Bohr.

Heisenberg's Discovery and Einstein's Semantical Views

Reference was made above in the discussion of the philosophy of Mach, about the influence of Einstein's admonition on Heisenberg's development of the indeterminacy relations. This episode in the history of science, which Heisenberg relates in "Quantum Mechanics and a Talk with Einstein (1925-1926)" in *Physics and Beyond*, is a watershed event for the contemporary Pragmatist philosophy of science. His description of his personal experience and thought processes deserve close examination. Firstly he discusses why he had been led to believe that he could develop a quantum theory exclusively on the basis of observed magnitudes. He writes that in the summer of 1924 he had attempted to guess the formula that might successfully describe the line intensities of the hydrogen spectrum using methods involving the idea of electron orbits, which he thought would be successful in view of the previous work of Kramers in Copenhagen. When use of these methods hit a dead end, he became convinced that he should ignore the idea of electron orbits. He decided instead that he should treat the frequencies and amplitudes associated with the spectral line intensities as substitutes, because the line intensities are observable directly, while the electron orbits are not. He was led to this approach because he recalled a conversation years earlier in which a friend told him that Einstein had emphasized the importance of observability in relativity theory. In May of 1925 Heisenberg suffered a severe hay fever attack and had to absent himself from his academic duties. While recuperating on the island of Heligoland he continued to work on the problem by considering nothing but observable magnitudes, and during this period of isolation he developed his matrix mechanics.

About a year later he was invited to give a lecture at the University of Berlin physics colloquium to present his matrix mechanics. Einstein was in the assembly, and after the lecture he asked Heisenberg to discuss his views with him in his home that evening. In that discussion Einstein argued that it is in principle impossible to base any theory on observable magnitudes alone, because in fact the very opposite occurs: it is the theory that decides what the physicist can observe. He argued that when the physicist claims to have observed something new, he is actually saying that while he is about to formulate a new theory that does not agree with the old one, he nevertheless must assume that the new theory covers the path from the phenomenon to his consciousness and functions in a sufficiently adequate way, that he can rely on it and can speak of observations. The claim to have introduced

nothing but observable magnitudes is actually to have made an assumption about a property of the theory that the physicist is trying to formulate. Heisenberg was thus using his idea of observation as if the old descriptive language could be left as it is. Heisenberg replied that Einstein was using language a little too strictly, and that until there is a link between the mathematical quantum theory and the traditional language, physicists must speak of the path of an electron by asserting a contradiction, notably Bohr's complementarity. Heisenberg also replied by referencing Mach's view that a good theory is no more than a condensation of observations in accordance with the principle of thought economy. Einstein explained that Mach thought a theory combines complex sense impressions just as the word "ball" does for a child. But he also stated that the combination is not merely a psychological simplification but is also an assertion that the ball really exists, because it makes assertions about possible sense impressions that may occur in the future. Einstein thus affirmed a realistic philosophy, and criticized Mach for neglecting the fact that the real world exists, that our sense impressions are based on something objective, and that observation cannot be just a subjective experience. Heisenberg accepted Einstein's realism on these grounds, and admitted that theory reveals genuine features of nature and not just of our knowledge.

In the preface to *Physics and Beyond* Heisenberg states that conversations cannot be reconstructed literally after several decades, and that the book is not intended as a collection of memoirs. But he notes that careful attention has been paid to the precise "atmosphere" in which the conversations took place, because in that conversational atmosphere the creative process of science is made manifest. His book contributes to explaining how the cooperation of different people may culminate in scientific results of the utmost importance. Heisenberg stated that his purpose is to convey even to readers who are remote from atomic physics, some idea of the mental processes that have gone into the genesis and development of science. In a chapter titled "Fresh Fields (1926-1927)" Heisenberg offers a description of his own mental processes in his development of the uncertainty relations. To the contemporary reader this description has value apart from his philosophy. Just as Newton attempted to philosophize about his work with his denial that he created hypotheses, so too did Heisenberg attempt to philosophize about his work in his own systematic and explicit philosophy of language, his doctrines of closed-off theories and of perception. But the recollections of his cognitive experiences in "Fresh Fields" are not an attempt at a systematic philosophy; they are more simply

his recollection of his own cognitive experiences as a central participant in the development of the quantum theory, and are valuable as a historical document. As it happens, in the contemporary philosophical perspective these recollections are more valuable than his explicit attempt to philosophize on the nature of language and perception.

These writings reveal that his development of the uncertainty relation was occasioned by several historical circumstances. One of these that he discusses in "Fresh Fields" was the development of the wave mechanics by Schrödinger and its disturbing effects on the thinking of the physicists at Copenhagen. The wave equation did not contain Planck's constant as did Heisenberg's matrix mechanics, while Planck's constant was thought by Bohr and the Copenhagen physicists to be central and necessary for any modern microphysical theory. Then Max Born, formerly a teacher of Heisenberg, proposed a probability interpretation of the wave equation, such that for each point in space and instant in time the equation gives the probability of finding an electron at a given point and instant. The upshot was that while neither the matrix mechanics nor the wave mechanics could be rejected for empirical reasons, they nevertheless seemed to be logically incompatible. In addressing this problem Bohr and Heisenberg took different approaches. Bohr attempted to admit simultaneously to the validity of both theories by maintaining that both the classical wave and the classical particle concepts used to describe the experimental observations are necessary for characterizing atomic processes, even though in the language of ordinary discourse and of classical physics, these two concepts are mutually exclusive. This semantic inconsistency became Bohr's complementarity principle. But Heisenberg relates that he did not like this approach, and that he wanted a "unique", that is, a consistent and unequivocal physical interpretation of the magnitudes in the mathematical formalism, one that is derivable from the matrix mechanics by strict logic. Heisenberg reports that this objective was one of the reasons that led him to derive the uncertainty relation.

A second reason leading him to the uncertainty principle was the fact that neither the wave mechanics nor the matrix mechanics seemed capable of explaining the observed phenomenon of the trajectory of the electron in the Wilson cloud chamber. Such ideas as trajectories and orbits do not figure in the mathematical formulations of the matrix mechanics, and the wave mechanics could only be reconciled with the existence of a densely packed beam of matter, if the beam spread over volumes that are much larger than the dimensions of an electron. This problem of the observed phenomenon in

the cloud chamber led Heisenberg to reformulate the questions he was asking himself in his statement of the problem; he attempted to relate the observed path of the electron in the cloud chamber to the mathematics of the matrix mechanics. In February and March of 1927 Bohr was vacationing in Norway and Heisenberg was again alone with his thoughts, as he had been when he had first developed the matrix mechanics. At this time his attempt to relate the cloud chamber observations to the matrix mechanics bought to mind his discussion with Einstein the prior year in Berlin, and specifically Einstein's statement that it is the theory that decides what can be observed. In "Fresh Fields" he describes his thinking processes when he attempted to employ Einstein's advice: Firstly he reconsidered the idea that what is observed in the cloud chamber is a trajectory. The idea of a trajectory is a concept in Newtonian physics. Therefore, when he thought that he was observing the trajectory of an election in the cloud chamber, the theory that was deciding what was being observed was the Newtonian theory, not his quantum theory. Then secondly after reconsidering the Newtonian observations and recognizing that it is not necessary to think in Newtonian terms, he viewed the phenomenon as merely a series of ill defined and discrete spots through which the electron had passed, somewhat like the water droplets which of course are much larger than the dimensions of the electron. Then thirdly he reformulated his problem, and asked how quantum theory instead of Newtonian theory can represent the fact that an electron finds itself approximately in a given place and that it moves approximately with a given velocity. Using Einstein's thesis that the theory decides what can be observed, Heisenberg concluded that the processes involved in any experiment or observation in microphysics must satisfy the laws of quantum theory. The magnitude of the observed water droplets suggested room for approximation for the minute electron, and Heisenberg asked whether it is possible to make these approximations so close that they do not cause experimental difficulties. He then derived the mathematics of the uncertainty principle in which the approximations are governed by a limit that is a function of Plank's constant.

It may be noted parenthetically that Heisenberg's use of Einstein's thesis may be contrasted with Duhem's Positivist view. Unlike earlier Positivists, Duhem admitted that theory has a valid place in science, but he did not view the concepts used for observation as dependent upon theory, as did Einstein. Instead, Duhem maintained that while theory provides an interpretation for observation, observation without theory is not only possible but furthermore offers a certitude that theory cannot offer, because

he viewed theoretical interpretation as something added to fundamental and uninterpreted observation. Thus, the characterization of the tracks in the cloud chamber as a series of water droplets would on Duhem's thesis be an example of an atheoretical and uninterpreted observation. On Einstein's thesis, however, there is no observation without theory, and the characterization of the condensed track in the Wilson cloud chamber as a series of water droplets is no less interpreted than the characterization of the phenomenon by means of Newtonian or quantum theoretical concepts. Even though the interpretation in terms of water droplets does not employ a mathematically expressed theory, the concepts of water droplet, of the cloud chamber, and even of the reflected light needed to view the water droplets in the cloud chamber, have built into them many hypotheses, or as Einstein says "expectations", that are tacitly assumed in order to make the observations.

Heisenberg had formulated his uncertainty principle by the time Bohr had returned to Copenhagen from his vacation in Norway. Initially Bohr objected to the idea, while at the same time Heisenberg disliked the complementarity idea that Bohr had developed. After several weeks of argument they finally agreed that the two approaches are related. The uncertainty principle reconciles at the microphysical level and in the mathematical formalism of quantum mechanics, what cannot be avoided yet what cannot be stated consistently in the language supplied by classical physics and ordinary language, which is suitable only to describe phenomena at the macrophysical level. What is expressed consistently with the mathematical formalism of the uncertainty principle is the impossibility of measuring simultaneously both the position and the impulse of the electron with a degree of accuracy greater than the limit imposed by Planck's constant, a limit that is imposed by virtue of the nature of the microphysical phenomenon itself and not merely by the measurement technique. What are described inconsistently at the macrophysical level and in the language of classical physics by means of complementarity, are the observable wave and particle manifestations of the unitary phenomenon. This concession to Bohr was at variance to Heisenberg's acceptance of Einstein's semantical thesis that the theory decides what the physicist can observe.

Heisenberg's description based on his own experience of the interpretative character of all perception and observation and of the role of scientific theory in determining the interpretation, articulates one of the most characteristic features of the contemporary Pragmatist philosophy of science. It is more valuable than Duhem's exemplification of the theoretical

interpretation of the laboratory apparatus in the opening passages of the chapter titled "Experiment in Physics" in *Aim and Structure of Physical Theory*, not only because Duhem's explanation is Positivist, but also because Heisenberg's description of his experiences is given in the context of his development of the uncertainty principle, one of the most noteworthy achievements of twentieth-century theoretical physics. As it happens, Heisenberg did not like Pragmatism, or at least the Pragmatism he encountered during his visit to the United States and described in "Atomic Physics and Pragmatism (1929)" in *Physics and Beyond*. Even though his description of the interpretative character of perception and observation actually contributed to the contemporary Pragmatism, Heisenberg himself was influenced by Bohr in ways that impeded his developing a philosophy of science that is consistent with Einstein's thesis that theory determines what is observed. And this influence places Heisenberg's explicit philosophy of science closer to the Positivist philosophy than either Einstein's or the Pragmatists' views. This influence of Bohr consisted of a naturalistic philosophy of the semantics of language, and the result is Heisenberg's neo-Kantian philosophy of perception and his doctrine of closed-off theories.

Heisenberg's Discovery and Einstein's Ontological Criteria

An ontology consists of those entities and aspects of the real world that are described by the semantics of a discourse, such as a scientific theory, which is believed to be true. Unlike Bohr, who took an instrumentalist view of the equations of the quantum theory, Heisenberg believed that quantum theory has an ontology, that is, that the equations constituting the language of the theory describe aspects of the real world. And he maintained that the ontology of quantum theory includes the Copenhagen duality thesis, the thesis that wave and particle are two aspects of the same physical entity, and are not two separate physical entities. Initially, however, his ontological views were not based in the language of the mathematically expressed quantum theory, but were based in the ordinary everyday language that can be used to express experimental findings. In the opening sentence of the "Introductory" chapter of his *Physical Principles of the Quantum Theory* (1930), a book based on lectures he gave at the University of Chicago in the Spring of 1929, Heisenberg says that the experiments of physics and their results can be described in the language of daily life. He adds that if the physicist did not demand a theory to explain his results and could be content with a description of the lines appearing on photographic plates, then

everything would be simple and there would be no need for an epistemological discussion. He states that difficulties arise only in the attempt to classify and synthesize the results, to establish the relations of cause and effect between them - in short, to construct a theory. The concept of everyday language appears again later in Heisenberg's doctrine of closed-off theories.

No contemporary Pragmatist would accept the Positivist thesis that there is any completely nontheoretical or pretheoretical observation language. With an adequate metatheory of semantical description the Pragmatist philosopher maintains that the everyday observational description, which is part of the test design language used in experiments, is sufficiently vague that it neither affirms nor denies any specific microphysical theory proposed for testing. But in his *Physical Principles* Heisenberg is not thinking of the vagueness of everyday language. Here he wishes to argue that the everyday description of certain experimental findings implies the Copenhagen ontology, and he proceeds to give a brief description of several experiments which show that both matter and radiation sometimes exhibit the properties of waves and at other times exhibit the properties of particles. He notes that it might be postulated that two separate entities, one having all the properties of a particle and the other having all the properties of wave motion, are combined in some way to form light. But he then adds that such a theory is unable to bring about the "intimate relation" between the two entities, which seems required by the experimental evidence. He argues that both light and matter are single entities, and that the apparent duality, the properties of wave and particle, arises in the limitations of our language. This thesis of the limitations of language reveals the influence of Bohr's philosophy. Other physicists such as de Broglie, Einstein, and Bohm did not agree with Heisenberg's view that there is any such compelling experimental evidence for the Copenhagen ontology. Both philosophers and scientists have had different ontological commitments, because they hold different criteria for determining which among alternative descriptive discourses is true, and more fundamentally because they maintain different philosophies of language.

It may be said that Einstein had two different ontological criteria for physics, one explicitly set forth by him, and another that he tacitly used and which therefore may be called his implicit criterion. In Newtonian physics and in relativity theory these two different criteria are not easily distinguished, because in each case they yield similar ontologies, but in

quantum theory they yield fundamentally different ontologies. Einstein's explicit ontological criterion for deciding what is physically real is set forth in his "Can Quantum Mechanical Description of Physical Reality be Considered Complete?" in *Physical Review* (1935), in his "Physics and Reality" in *The Journal of the Franklin Institute* (1936), and in his "Reply to Criticisms" in *Albert Einstein* (ed. Schilpp, 1949). There are several statements. One that he sets forth as his "programmatic aim of all physics" is his criterion of logical simplicity, which he sets forth as the aim of science: The aim of science in Einstein's view is a comprehension as complete as possible of the connections among sense impressions in their totality, and the accomplishment of this aim by the use of a minimum of primary concepts and relations. He goes on to say that the essential thing about the aim of science is to represent the multitude of concepts and theorems close to experience as theorems logically deduced from and belonging to a basis, as narrow as possible, of axioms and fundamental concepts, which themselves can be chosen freely. Thus the aim of science is the logical unity of the world picture. Einstein interprets the history of physics as an evolution under the direction of this aim of science. This criterion requires that microphysical and macrophysical theories affirm one single consistent ontology, and use the same basic concepts of what is physically real. He also says that the conviction that field theory is unable to give a solution to the molecular structure of matter and to the quantum phenomenon, is a false prejudice. He demands that the ontology of field theory supply the uniform fundamental ontology, and he uses this explicit ontological criterion to criticize the Copenhagen statistical interpretation of quantum physics.

In a famous article titled "Can Quantum Mechanical Description of Physical Reality be Considered Complete?" in *Physical Review* co-authored with Podolsky and Rosen, Einstein describes the Copenhagen interpretation as "incomplete". By this he meant that further research is needed to make quantum theory consistent with the ontology of field physics, the ontology of deterministic causality and of the physical continuum in four dimensions. The argument in this paper, often called the "EPR argument" after the three co-authors, includes a thought experiment, which is based on explicit criteria for completeness and for physical reality. The completeness criterion says that a physical theory is complete only if every element of the physical reality has a counterpart in the physical theory. The criterion for physical reality is that if without in any way disturbing a system, one can predict with certainty the value of a physical quantity, then there exists an element of

physical reality corresponding to this physical quantity. This criterion's reference to independence of any act of observation is repeated in a later statement of the programmatic aim of all physics in "Remarks" in Schilpp's *Albert Einstein.* The thought experiment in the EPR argument attempts to demonstrate that the quantum theory's satisfaction of the reality criterion does not result in satisfaction of the completeness criterion.

The stated criteria for completeness and for physical reality are defined such that field theory satisfies both criteria while quantum theory does not. The point of departure, the basic premises of the argument, is Einstein's ontological preferences. In an article with the same title also appearing in *Albert Einstein* Bohr argued that the phrase "without in any way disturbing a system" in Einstein's criterion for physical reality is ambiguous, because its meaning in classical physics is not the same as that in quantum physics. Bohr maintained that in quantum measurements the object measured and the observing apparatus form a single indivisible system that defies any further analysis at the quantum level. A large literature developed around the technicalities of the physical thought experiment, but in practice the physicists chose their ontological premises according to their preferences about the ontological conclusions, depending on whether one agreed or disagreed about Einstein's view that quantum theory must have the same ontology as field physics. And for most of the following half century the preferred conclusion was the Copenhagen interpretation of the quantum theory.

On the other hand Einstein's implicit ontological criterion was operative in his development of the special theory of relativity. This criterion (stated explicitly) is that the empirically adequate scientific theory must be interpreted realistically. Unlike Einstein's explicit criterion, which subordinates a scientific theory and its interpretation to a preconceived ontology, the implicit criterion subordinates ontological commitment to the outcome of empirical scientific criticism. And Heisenberg applied this same ontological criterion to the mathematical expressions of the quantum theory to defend the Copenhagen dualistic ontology against Einstein's criticism based on the latter's explicit ontological criterion for physical reality. In this defense based on the mathematical language of the quantum theory instead of the everyday language of the microphysical experiments, Heisenberg referenced Einstein's realistic interpretation of the Lorentz transformation equation. In his discussions about Einstein's special theory of relativity in *Physics and Philosophy* and in *Across the Frontiers* Heisenberg describes as the "decisive" step in the development of special relativity, Einstein's

rejection of Lorentz's distinction between "apparent time" and "actual time" in the interpretation of the Lorentz transformation equation, and Einstein's taking "apparent time" to be physically real time, while rejecting the Newtonian concept of absolute time as real time. In other words this decisive step consisted of taking the Lorentz transformation equation realistically, and of letting it define the ontology of the physically real due to its empirical adequacy.

Nowhere does Heisenberg write that he was consciously imitating Einstein at the time Heisenberg developed the uncertainty relations. But in "History of Quantum Theory" in *Physics and Philosophy* he describes his use of the same strategy. In this description of his thought processes Heisenberg does not refer to his conversation with Einstein in Berlin in 1926. He states that his thinking in the discovery experience of the uncertainty principle consisted of turning around a question. Instead of asking himself how one can express in the Newtonian mathematical scheme a given experimental situation, notably the Wilson cloud chamber experiment, he asked whether only such experimental situations can arise in nature as can be described in the formalism of the matrix mechanics. The new question is a question about what can arise or exist in reality. Later in "Remarks on the Origin of the Relations of Uncertainty" in *The Uncertainty Principle and Foundations of Quantum Mechanics* (p. 42.) he explicitly states that this meant that there was not a Newtonian path of the electron in the cloud chamber. Heisenberg's strategic answer to the new question, the uncertainty relation, resulted from this realistic interpretation of the quantum theory. Similar remarks are to be found in "The Development of the Interpretation of the Quantum Theory" in Pauli's *Niels Bohr and the Development of Physics* (1955, p. 15) where Heisenberg says that he inverted the question of how to pass from an experimentally given situation to its mathematical representation, by using the hypothesis that only those states which can be represented as vectors in Hilbert space can occur in nature and be realized experimentally. And he immediately adds that this method of solution had its prototype in Einstein's special theory of relativity, when Einstein had removed the difficulties of electrodynamics by saying that the apparent time of the Lorentz transformation was the real time, that similarly it is now assumed in quantum mechanics that real states can always be represented as vectors in Hilbert space (or as mixtures of such vectors), and that the uncertainty principle is the simple expression for this assumption.

If at the time that he developed the uncertainty principle, Heisenberg was not consciously imitating the discovery strategy that Einstein used for development of special relativity, it is nevertheless not difficult to imagine how Heisenberg hit upon it independently. For the realist it is a small step from Einstein's semantical thesis that it is the theory that decides what can be observed, to the ontological thesis that it is the theory that decides what is physically real, where the theory in question is empirically warranted, as was his matrix mechanics. This strategy in which the empirical adequacy of a scientific theory as revealed by scientific criticism decides the ontology to be accepted, is a reversal of the more traditional relation in which currently accepted ontological and metaphysical views are included among the criteria for scientific criticism, and operate prior to empirical criticism. Heisenberg's approach is similar to the contemporary Pragmatist thesis of scientific realism. Heisenberg explicitly compares his realistic interpretation of the statistical quantum theory to Einstein's realistic interpretation of the Lorentz transformation equation, when he defends the ontology of his Copenhagen interpretation against Einstein's explicit ontological criterion for physical reality. In his "Criticism and Counter-proposals to the Copenhagen Interpretation of Quantum Theory" in *Physics and Philosophy* he characterizes the ontology advanced explicitly by Einstein as the ontology of "materialism", which he says rests upon the "illusion" that the kind of existence familiar to us, the direct actuality of the world around us, can be extrapolated into the atomic order of magnitude. In the closing paragraphs of this chapter of his book he states that all counterproposals offered in opposition to the Copenhagen interpretation must sacrifice what he calls the symmetry properties of the quantum theory, namely the wave-particle symmetry and the position-momentum symmetry. He explicitly states that like Lorentz invariance in the theory of relativity, the Copenhagen interpretation cannot be avoided, if these symmetries are held to be genuine features of nature.

However, there is an ambiguity in Heisenberg's practice of scientific realism. The position-momentum symmetry that he construes realistically is clearly expressed by the indeterminacy relations. But the wave-particle symmetry of the Copenhagen interpretation is that the wave and particle are dual alternative manifestations of the same entity. This thesis cannot be affirmed on the basis of the mathematically expressed quantum theory, because descriptive language having mathematics for its grammar does not have the syntactical categories for expressing reference to entities. Statements referencing entities, such as Aristotelian or Russellian logic or

ordinary thing-language (as Caranp would say) must be added to the mathematically expressed quantum theory in order for a realistic version of the Copenhagen duality thesis to be either affirmed or denied. A better example of Heisenberg's practice of scientific realism is his *potentia* ontology given in his summary of the Copenhagen interpretation of the statistical nature of the quantum theory in "The Copenhagen Interpretation of Quantum Theory" in his *Physics and Philosophy* (1958).   Heisenberg invokes Aristotle's idea of *potentia* to express thew thesis that wave and particle do not appear simultaneously, and are always wave or particle manifestations of the same entity.   His interpretation of the probability function is that it has both a subjective and an objective aspect.   The subjective aspect makes statements about the observer's incomplete knowledge, while the objective aspect makes statements about what Heisenberg calls "tendencies" and "possibilities", and it is in this latter aspect he refers to the idea of *potentia*. The probability function in the quantum theory is subjective and represents incomplete knowledge, because the observer's measurements are always inaccurate. The subjective reason that they are inaccurate is the ordinary errors of measurement that occur both in classical physics and in quantum physics. But the objective reason is distinctive to quantum physics, and it is the inaccuracy caused by a disturbance introduced by the measurement apparatus in the measurement process.   Heisenberg illustrates this by means of an ideal experiment involving a gamma-ray microscope used to observe an electron. In the act of observation at least one light quantum of the gamma ray must have passed the microscope, and must first have been deflected by the electron. Therefore the electron must have been impacted by the light quantum and must have changed its momentum.   The uncertainty relations give the uncertainty of this change. When the probability function is written down, it includes both these inaccuracies, and there must be at least two such disturbing observations in an atomic experiment. The probability function also contains an objective element, but it is not like the description of motion in classical physics. The classical physicist would like to say that between the initial and the second observation the electron has described an unknown path in a cloud chamber.   But Heisenberg says that between the two observations the electron has not described any path in space and time, since the electron has not been anywhere.   The probability function does not represent a course of events in the course of time, but rather represents statistical possibilities or tendencies, which are actualized by the second act of observation.   The transition from the possible to the actual takes place

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with the act of observation involving the interaction of the electron with the measuring device. Heisenberg notes that the transition applies to the physical and not to the psychological act of observation, and that certainly quantum theory does not contain "genuine subjective features" in the sense that it introduces the mind of the physicist as a part of the atomic event.

The objective aspect of the statistical quantum theory is described in terms of the transition from the possible to the actual is due to the wave-particle duality, which Heisenberg illustrates by another experimental set up, the historic interference experiment firstly performed by Thomas Young in 1801. It involves passing monochromatic light through a screen with two holes or slits in it, and then registering the light on a photographic plate. Viewed as a wave phenomenon there are primary waves entering the slits, and there are secondary spherical waves starting from the slits, that interfere with each other to produce a pattern on the photographic plate. But the registration on the plate is a quantum process, a chemical reaction. If the quantum passes through either slit, the other one is irrelevant. But the existence of the other slit is in fact relevant, because the photographic plate registers an interference pattern. Therefore the statement that any light quantum must have gone through either just one or just he other slit is problematic. Heisenberg maintains that this problematic outcome shows that the concept of the probability function does not allow a description in space and time of what happens between the two observations. The description of what "happens" is restricted to the observation process in which there occurs the transition from the possible or *potentia* to the actual. As it happens, the idea of construing the indeterminacy realistically as potentiality had been proposed several years earlier by David Bohm in his *Quantum Theory* (1951), written while he accepted the Copenhagen interpretation and before proposing his hidden-variables thesis. But Heisenberg does not reference Bohm in his own thesis of *potentia*, and seems to have derived the idea independently from his knowledge of Aristotle's philosophy.

Contemporary philosophers and historians of science have learned to recognize in the history of science the occurrence of the scientific realism, the realistic interpretation of empirically successful theories. As new and empirically superior theories are developed, their realistic interpretations produce new ontologies with new ideas and beliefs about what is real, including ideas of the nature of causality. Hanson describes the scientists' gradual acceptance of scientific realism with his metaphor of the black box, the gray box, and the glass box, where a new theory is seen to reveal reality as the transparent glass box reveals its contents. As Feyerabend notes, when

the new theory with its new ontology is attacked by the establishment, the so-called authorities of the particular scientific profession, it is invariably attacked with the ontological beliefs defined by a preceding and less empirically adequate theory. Einstein seems not to have been unaware of this historical of phenomenon. In his "Reply to Criticisms" he stated that the scientist cannot afford to carry his striving for epistemological systemic as far as will the philosopher, and that while the scientist gratefully accepts the epistemologist's analysis, nonetheless the facts of experience, by which he presumably means scientific evidence, do not let the scientist be too much restricted in the construction of his conceptual world by the adherence to an epistemological system. Einstein was faithful to this insight to the extent that he rejected the Positivist philosophy, but he did not follow through with it, when he functioned as his own epistemologist and attempted to impose the deterministic ontology of field theory upon quantum theory.

Bohr's Influence on Heisenberg and Issues with Einstein

Niels Bohr was one of the leading atomic physicists of the first half of the twentieth century. He had studied in England under J.J. Thompson and Lord Rutherford, and received the Nobel Memorial Prize for Physics in 1922 for his theory of the structure of the atom. He founded the Copenhagen Institute for Theoretical Physics in 1920, and as its director was actively recruiting talented staff members, when he accepted an invitation to deliver a series of lectures on atomic physics at the University of Gottingen in the summer of 1922. In "Quantum Theory and its Interpretation" in *Niels Bohr* (1963) Heisenberg reports that he first met Bohr at the Gottingen lectures, which he attended with his teacher, Arnold Sommerfeld. At the time Heisenberg was a twenty-two year old, fourth semester student at the University of Munich. Heisenberg came to Bohr's attention, because in the discussions following one of the lectures, he dissented from Bohr's optimistic assessment of a theory developed by Kramers at Copenhagen. Heisenberg relates that Bohr was sufficiently worried about the objection, that after the discussion he asked Heisenberg to take a walk with him for a conversation. During the walk Bohr talked about the fundamental physical and philosophical problems of modern atomic theory, and the encounter resulted in an invitation for Heisenberg to visit the Institute at Copenhagen for a few weeks, and later to hold a position. Heisenberg describes his impressions of Bohr as primarily a philosopher rather than a physicist, and he states that he found Bohr's philosophy to be fascinating, although he also

states that he and Bohr had different views on the role of mathematics in physics.

Bohr's philosophy of atomic physics is set forth in his *Atomic Physics and the Description of Nature* (1934), "Discussions with Einstein" in *Albert Einstein* (ed. Schilpp, 1949), *Atomic Physics and Human Knowledge* (1958), and *Essays 1958/1962 on Atomic Physics and Human Knowledge* (1963). Bohr's philosophical views may have been influenced by some casual reading of the philosophical literature, but he never references any philosopher in his writings. His views seem largely to be the product of his own reflections on his research in atomic physics and on the work of the staff at Copenhagen. In "Quantum Theory and Its Interpretation" Heisenberg states that Bohr had developed views on the semantics of language and scientific theory many years before he had met Bohr and before he developed his matrix mechanics. Bohr's mature philosophy of science included two theses: Firstly that the mathematical formalisms of microphysics cannot describe the microphysical domain that lies beyond ordinary experience. Secondly that the only language that is capable of a descriptive semantics is the language of ordinary discourse and its refinement in classical Newtonian physics. Heisenberg did not accept the first thesis, and had a different concept about the abstract nature of mathematics. But Bohr's second thesis had a lifelong influence on him, an influence that had a retarding effect on his development of his own philosophy.

Bohr gives various reasons why in his view the mathematical formalisms of microphysics have no descriptive semantics and are only symbolic instruments for making calculations and predictions. One reason given in "Discussions with Einstein" is the occurrence of a complex number in the formalism. Apparently he believed that reality could be described only by equations having variables and parameters that admit only real numbers for values. Another reason given in "The Solvay Meetings and the Development of Quantum Theory" (1962) in his *Essays 1958/1962* is the interpretation of the statistical wave function in a configuration space of more than four dimensions. Like Einstein, Bohr believed that real physical space-time has no more than four dimensions. But the basic reason why Bohr interpreted the mathematical formalism of quantum theory instrumentally is his belief that only the language of everyday discourse and its refinement in classical physics can have a descriptive semantics. He maintained that ordinary language and classical physics must be used to describe any experimental set up in physics, while at the same time he

believed that classical physics is too limited to describe the microphysical domain beyond ordinary experience. It is limited not only because Newtonian physics is inadequate as a microphysical theory, but also due to the inherent nature of human cognitive perception. This is a philosophy of the semantics of language that is a variation on the naturalistic thesis. Due to Bohr's philosophy of perception, Einstein as well as many philosophers of science were led to conclude that Bohr's philosophy of science is Positivist.

If Bohr's philosophy of science is a Positivist philosophy, it is a peculiar one. His statements of his philosophy that are most often referenced in this connection by philosophers of science are those in *Atomic Physics and the Description of Nature*. In the opening "Introductory Survey (1929)" he states that both relativity theory and quantum theory are concerned with physical laws that lie beyond ordinary experience, and which therefore present difficulties to our "accustomed forms of perception". In quantum theory the limitations of these forms of perception are revealed by the need for the complementary description, the inconsistent description of the quantum phenomenon as both a wave and a particle. Both of these two forms based on classical physics are necessary for a complete description, even though they are inconsistent in classical physics. Yet these "customary" forms of perception cannot be dispensed with, since all human cognitive experience must be expressed in terms of them. The fundamental concepts of classical physics therefore will never become superfluous for the description of physical experience; they must be used to describe experiments and to relate the mathematical symbolisms to the data of experience.

In Einstein's attack on Bohr's philosophy of quantum theory the central issue is the ontology of the Copenhagen interpretation, which Einstein critiqued with his programmatic aim of all physics. The explicit criterion set forth in the programmatic aim of science is the "complete" description of any individual situation, as it supposedly exists irrespective of any act of observation or substantiation. Accordingly he characterized the Copenhagen interpretation as a version of Bishop Berkeley's idealist thesis "*esse est percipi*", a characterization that is not accurate, because Bohr did not maintain that the atomic phenomenon is produced by a cognitive process but rather by the physical processes of measurement in the experimental set up. In this matter Einstein seems to have confused an epistemological issue with a physical one. But Bohr is not blameless for the confusion. For example in "Introductory Survey (1929)" he opens with statements emphasizing the subjectivity of all experience and the difficulties in

distinguishing between phenomena and their observation; and he concludes the chapter with the statement that "to be" and "to know" lose their unambiguous meanings. From an epistemological viewpoint some of Bohr's statements are ambiguous as to whether he is advancing a realist or an idealist philosophy. Some of Heisenberg's earlier statements are also suggestive of an idealist position. For example he writes in the opening chapter of *The Physicist's Conception of Nature*, that since we can no longer speak of the behavior of the particle independently of the process of observation, the natural laws formulated in the quantum theory no longer deal with the elementary particles themselves, but only with our knowledge of them. But later Heisenberg is very clear about avoiding any metaphysical idealism. In "The Copenhagen Interpretation of Quantum Theory" in *Physics and Philosophy* he states explicitly that quantum theory does not contain genuinely subjective features, since it does not introduce the mind of the physicist as part of the atomic event, and that the transition from possible to actual in the act of observation is in the physical and not the psychical act of observation.

This metaphysical idealist/realist confusion notwithstanding, however, Einstein's central ontological thesis is that the statistical quantum theory is incomplete in the sense that further theoretical research is necessary, in order to develop a complete theory that would give Heisenberg's uncertainty relations a status in future physics, which he thought should be analogous to the status had by statistical mechanics. What is most noteworthy is that Einstein admits that the indeterminacy principle is not empirically incorrect, even as he rejects the Copenhagen ontology because it does not conform to his explicit ontological criterion. In the 1949 "Reply to Criticisms" Einstein conceded that his incompleteness thesis is the minority view among physicists; contemporary philosophers as well as physicists have accepted the indeterminacy thesis of the Copenhagen interpretation of the statistical quantum theory, and have rejected the deterministic ontology advocated by Einstein. When confronted with the dilemma of having to choose between an established ontological criterion and a new but empirically adequate quantum theory, both the contemporary physicists and the contemporary Pragmatist philosophers of science have opted for the latter, contrary to Einstein's arguments for the former.

In addition to the ontological issue between Bohr and Einstein about what is physically real, there is also a related epistemological issue about the relation between sense perception and intellectual concepts, which is also a semantical issue about what the Positivists called the relation between

observation language and theory language. Einstein had portrayed Bohr as a Positivist due to Bohr's views about perception and the semantics of language. This portrait is debatable, because Positivists do not usually speak of forms of perception, and particularly about the limitations of such forms of perception for physics. But in his 1934 book Bohr writes of the necessity of these forms of perception for science to reduce our "sense impressions" to order. Even though Einstein himself uses the phrase "sense impressions" in his statement of the aim of science in "Physics and Reality" in 1936, he seems to have taken Bohr's discussion referencing sense impressions to mean that these are no concepts or categories in perception. Einstein opposed this view, and states in 1949 in his "Reply to Criticisms" that thinking without positing categories and concepts is as impossible as breathing in a vacuum. He furthermore states that his philosophy differs from Kant's only by the fact that he does not view categories as unalterable and as conditioned by the understanding, but rather views them as "free conventions". The philosopher of science may ask whether Einstein's neo-Kantian views without Kant's idealism and *a priorism* is still recognizably Kantian. But the point to be emphasized is that Einstein's thesis that concepts are necessary for perception and that they are free conventions amounts to a restatement of what he told Heisenberg in 1926, when he said that it is the theory that decides what we can observe. In this earlier statement Einstein might consistently have told Heisenberg that observation without theory is as impossible as breathing in a vacuum. Perhaps it was in response to Einstein's criticisms in these matters that Bohr refrains in his later writings from using the phrase "sense impressions". Instead Bohr merely describes the concepts of classical physics as a refinement of the concepts of ordinary discourse, so that he is no longer mistakenly taken as saying that perception occurs without any concepts or forms.

Nonetheless there is still a fundamental difference between the semantical views of Bohr and Einstein. Einstein's thesis that concepts are free conventions is intended to mean that there are none of the inherent limitations in observation or in language that Bohr had maintained. In Bohr's phrase "customary forms of perception", the term "customary" does not mean the same thing as the term "convention" in Einstein's phrase "free conventions". The limitations that Bohr said these customary forms of perception impose on descriptive language are not temporary limitations, which will be removed with the change in language customs resulting from the further development of theory. Rather these limitations are inherent in the nature of the human cognitive processes of perception and consequently

in the semantics of descriptive language. They are therefore permanent. There is no such permanence according to Einstein's view; the free conventions of human thought, in the concepts and categories in language and scientific theory, are not only conventions that are free to change, but are destined to change with the advancement and further development of scientific theory. The difference between Bohr's and Einstein's semantical views is the difference between the naturalistic and the artifactual philosophies of the semantics of language.

A few more comments about the relation of the semantical issue to contemporary philosophy of science: Einstein's semantical views anticipated those of the contemporary Pragmatist philosophers of science in several respects, and his arguments against Positivism undoubtedly had an influence on the Pragmatists, even though he is seldom referenced in the philosophical literature. Einstein rejected the Positivist thesis that each individual concept in a theory requires specific justification of its meaningfulness, when the concept is indispensable for the theory, and when the theory in its entirety has been empirically validated. This is a rejection of the Logical Positivist problem of theoretical terms. He also rejected Bridgman's operationalist thesis, and its requirement that each of a theory's assertions must be independently interpreted and tested, because this procedure has never yet been accomplished for any scientific theory, and furthermore in Einstein's view, it cannot be accomplished. On Einstein's thesis a physical theory need only imply some empirically testable assertions; there exists no logical path from the empirically given to the conceptual world. Both the individual concept and the individual assertion in a theory confront the empirically given in connection with the entire system of assertions, because there is an element of arbitrary choice between the empirical and the conceptual world, that result in what Einstein calls an "embarrassment of riches" for the theorist. This element of the arbitrary in the relation between the empirical and the conceptual is the basis for the contemporary Pragmatist philosophers' thesis that the semantics of language is not predetermined by nature, as Bohr and the Positivists had maintained, but rather is a cultural artifact. Thus the meanings of individual terms and assertions are not determined by their relation to the empirical world individually, but by their relation to one another in the larger context of a discourse, such as a scientific theory. This Pragmatist thesis is at least consistent with Einstein's views. But many contemporary Pragmatists take a step that probably Einstein cannot be associated with. They equate the dependence of meanings upon context with a wholistic view of the semantics of language.

But one cannot be certain about what Einstein might have said. While Einstein affirmed an artifactual theory of the semantics of language, he did not develop a theory of meaning description.

On the other hand Einstein took his views a step in another direction than the Pragmatists, when he advanced his explicit ontological criterion of logical simplicity for the whole of physics. This is a nonempirical criterion for scientific criticism, which Einstein used to argue that the concepts that are successful in field theory must also be used in quantum theory. It is this requirement that macrophysical and microphysical theories use the same ontological categories that led Einstein to reject the ontology of the Copenhagen interpretation of the statistical quantum theory. Here the contemporary Pragmatist philosophers of science depart from Einstein's views. The element of arbitrariness that both they and Einstein admit in the relation between the empirical and the conceptual, leads the Pragmatists to admit pluralism in empirical science that both Einstein and most Positivist philosophers found scandalous. This pluralism is opposed to Einstein's explicit ontological criterion of simplicity for all of physics. The Pragmatists do not find the theorists' embarrassment of riches permitted by the artifactual character of the semantics of language embarrassing as Einstein's nonempirical criterion for scientific criticism, which seeks to constrain the development of scientific theory by imposing a uniform monolithic ontology. On the Pragmatist view pluralism is characteristic of the development of science, and some Pragmatist philosophers maintain that it is a condition for its advancement.

Semantical Revision and Heisenberg's Doctrine of Closed-off Theories

Heisenberg's response to the conflicting influences of Einstein and Bohr was his doctrine of closed-off theories. An earlier and a later version of his semantical doctrine may be distinguished. The earlier version is given in his "Questions of Principle in Modern Physics" originally given as a lecture at the University of Vienna in 1935 and since published in his *Philosophical Problems of Quantum Physics*, where he sets forth the central questions that are addressed by his philosophy of physics. He firstly asks how it is possible for there to have occurred the "strange" revision of the fundamental concepts of physics during the preceding thirty years. Then secondly he asks what is the truth content of classical physics and of modern physics in view of this conceptual revision. He notes that these are also the questions that were posed and discussed by Bohr, who approached them

from the fundamental premises of quantum theory. It is noteworthy that Heisenberg's philosophy of science addresses questions formulated by Bohr. The formulation of the questions in terms of how a conceptual revision is possible suggests a naturalistic philosophy of the semantics of language as a point of departure, since on the artifactual thesis the possibility of a fundamental semantical revision is not problematic. When concepts and meanings are understood to be cultural artifacts, then semantical change may be expected as a matter of course.

As it happens, Heisenberg did not depart very far from the naturalistic thesis. He developed a theory of semantical revision, but it is also a theory of semantical permanence. His mature philosophy of science is not Positivist, but he maintains that classical physics is permanently valid, and that its concepts are necessary for experimentation in physics. He states that classical physics is based on a system of mathematically concise axioms, whose physical content is fixed by the choice of words used in them. These words determine unequivocally the application of the system of axioms to nature. Wherever concepts like mass, velocity and force can be applied, there Newton's law, $F=ma$, will be true. The validity of the claim of this law is comparable to Archimedes law of the simple lever, which today forms the theoretical basis for all load-raising machines, and which will be true for all time. Therefore in spite of the fact that there has been a revision of classical mechanics, the axiomatic system developed by Newton is still valid. The revision pertains to the limits encountered in the application of the axiomatized system of concepts of classical physics; it is not the validity but only the applicability of classical laws, that has come to be restricted by relativity theory and quantum theory. The experiences that provide the basis of relativity theory have demonstrated that the simple concept of time in Newton's mechanics ceases to be of use, when dealing with bodies moving with a velocity approaching that of light. Similarly in microphysics classical mechanics can predict the correct track of the electron in the Wilson cloud chamber. But if without observation of its track the electron is reflected at a diffraction grating, the basis for an unambiguous application of the space-velocity concept disappears, and classical laws cannot be applied to such a process.

Having thus described how the axiomatized mathematical system of classical physics is permanently valid, Heisenberg then describes how revision is possible. The revision of classical physics is possible due to a "lack of precision" in the concepts used in the system. While the quantitative variables x, t, and m used in the Newtonian system are linked

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without ambiguity by the system of equations, which contain no degree of freedom apart from initial conditions, the words "space", "time", and "mass", which are attributed to the quantities are tainted with all the lack of precision that may be found in their everyday use. The validity of classical physics is limited by the lack of precision of the concepts contained in its axioms. As a result of this lack of precision science may be forced into a revision of its concepts as soon as it leaves the field of common experience; the concepts currently used may lose their value for the orderly presentation of new experience. But this revision cannot be known in advance. For example before the experiences of quantum theory the results of the Wilson cloud chamber experiments could unhesitatingly be expressed as "we see in the cloud chamber that the electron has described such and such a path", and this simple description could be accepted as an experimental fact. It was only later that physicists came to know from other experiments the problematic nature of the phrase "path of an electron". Scientific progress consists initially in the unhesitating use of existing terms for the description of experience, and then subsequently in the revision of those terms as demanded by new experience. The lack of precision contained in the systems of concepts of classical physics is necessary, and therefore even the mathematically exact sections of physics represent only tentative efforts to find our way among a wealth of phenomena.

Classical concepts must be retained for experimentation in physics. So far as the concepts of space, velocity and mass can be applied unhesitatingly, as in everyday experiences, Newtonian principles still apply. The Newtonian laws represent an "idealization" achieved by taking into account only those parts of experience that can be ordered by the concepts of space, time and mass on the assumption of objective events in time and space. Therefore they always remain the basis for any exact and objective science. Since we demand of the results of science that they can be objectively demonstrated, we are forced to express these results in the language of classical physics. For example for an understanding of relativity theory, it is necessary to stress that the validity of Euclidian geometry is presupposed in the instruments that are used to show the deviation from Euclidian geometry, i.e. the measure of the deviation of sunlight [an apparent reference to Eddington's 1919 eclipse experiment to test relativity theory]. Furthermore the very methods used for the manufacture of these instruments enforce the validity of Euclid's geometry for these instruments within the range of their accuracy. Similarly we must be able to speak without hesitation of objective events in time and space in any discussion of

experiments in atomic physics. Heisenberg concludes that while the laws of classical physics seen in the light of modern physics appear only as limiting cases of more general and abstract connections, the concepts associated with these laws remain an indispensable part of the language of science, without which it is not possible even to speak of scientific results. Therefore, while mathematically exact sections of physics are tentative, the classical concepts must nevertheless be used for the description of experiments.

Heisenberg offers a later version of his doctrine of closed-off theories in several later articles and chapters in his books. In the earlier version meanings found in ordinary-language words, which are associated with variables in mathematically expressed axiomatic systems of physical theories, retain their vagueness in Newtonian physical theory. In his latter version association of the vague meanings with the terms in the axiomatic system resolves the vagueness, because the axiomatic systems have a definitional function. This development represents his transition to a context-determined semantics, where the relevant context is the axiomatic system of a physical theory. Consider firstly Heisenberg's earlier version of his semantical metatheory: In "The Notion of a 'Closed Theory' in Modern Science" in *Across the Frontiers* he discusses the criteria for scientific criticism and the evolution of the aim of science. When Einstein developed his special theory of relativity, it was evident that Maxwell's theory of electromagnetic phenomena could not be traced back to mechanical processes that obey Newton's laws, and the inference seemed unavoidable that either Newtonian mechanics or Maxwell's theory must be false. Physicists concluded that Newton's theory is strictly speaking false. This misleads many scientists into unwittingly attempting to describe the phenomena of the world exclusively by means of the concepts of field theory. This represented an aim of science that is commonly accepted from Newton's theory that science should proceed by means of a unitary conceptual scheme, except that now the concepts should be those of field theory instead of classical mechanics. But in both cases the concepts supplied an objective and causal description of the process involved, and were therefore thought to be universal. These common concepts were rejected by quantum theory for the description of the atom, although they must still be used to describe the results of an observation while standing in a complementary relation to one another. Thus physicists no longer say that Newton mechanics is false and must be replaced by quantum mechanics which is correct. Instead it is said that classical mechanics is a consistent self enclosed scientific theory, and that it is a strictly true and correct

description of nature, whenever its concepts can be applied. Quantum theory has only restricted the applicability of Newtonian mechanics, and has made classical physics a "closed-off" theory. Heisenberg says that in contemporary physics there are four great disciplines that are closed-off theories. They are firstly Newtonian mechanics, secondly Maxwell's theory and special relativity, thirdly the theory of heat and statistical mechanics, and fourthly nonrelativistic quantum mechanics, atomic physics and chemistry. General relativity is not yet closed off.

Heisenberg then turns to a discussion of the properties of a closed-off theory and of its truth content. He says that a closed-off theory is consistent as an axiomatized mathematical system. The most celebrated example is Newton's *Principia Mathematica*. And the concepts of the theory must be directly anchored in experience. Before the axiomatic system is developed, concepts describing everyday life remain firmly linked to the phenomena and change with them; they are compliant toward nature. But when they are axiomatized, they become rigid, and they "detach" themselves from experience. This is the distinctive aspect of his later version of the doctrine of closed-off theories. The system of concepts rendered precise by axioms is still very well adapted to a wide range of experiences, but axiomitization of concepts sets a decisive limit to their field of application. The discovery of these limits is part of the development of physics. Yet even when the boundaries of the closed theory have been encountered and overstepped, and new areas of experience are ordered by means of new concepts, the conceptual scheme of the closed theory still forms an indispensable part of the language in which the physicist speaks of nature. The closed theory is among the presuppositions of the wider inquiry; we can express the result of an experiment only in the concepts of earlier closed theories.

A comment may be interjected here about the later version of closed-off theories: Heisenberg's purported historical and operational continuity between everyday and classical concepts seems motivated by his agenda of making classical physics a permanently valid observation language, and so explains why he does not distinguish everyday concepts from classical concepts in most other passages in his literary corpus. But in his exposition of his later version of his doctrine of closed-off theories, he distinguishes the everyday and the classical types of concepts. The classical concepts are those defined by the context of the classical axiomatic system, while the everyday concepts are not defined by this context, but instead have a vagueness and "lack of precision" that makes their semantics silent about other concepts to which they could be but are not related in an axiomatic

system such as Newtonian or quantum physics, a vagueness that enables them to be "compliant toward nature". On the Pragmatist thesis of the contextual determination of meaning, both the everyday and classical concepts have both vagueness and defining contexts. But these concepts differ in that the former's context lacks the higher degree of clarity supplied by the context constituting the axiomatic deductive system of a physical theory that is had by the latter. The continuity between classical and quantum concepts, such that on Heisenberg's view the latter presuppose the former for observation, is more problematic since the two types of concepts are not in logically consistent axiomatic systems, but rather are on opposite sides of the schism in physics. But on the Pragmatist view the everyday, classical, and quantum concepts share component parts that make them relevant to the same subject. The everyday concepts are simply those that are vague, because they do not contain any of the mutually exclusive and inconsistent parts not shared by the classical and quantum concepts in their respective axiomatic systems.

Heisenberg summarizes the properties of closed-off theories as follows: Firstly the closed-off theory holds true for all time. Whenever experience can be described by the concepts of the closed-off theory, even in the most distant future, the laws of this theory will always be correct. Secondly the closed-off theory contains no perfectly certain statements about the world of experiences; its successes are contingent. Thirdly even with the uncertainty of its contingency, the closed-off theory remains a part of scientific language, and therefore is an integrating constituent of our current understanding of the world.   Heisenberg sees the evolution of modern science differently than Einstein's description in "Physics and Reality". The historical processes that have given rise to the whole of modern physics since the conclusion of the Middle Ages, is a developmental process consisting of a succession of intellectual constructs, which take shape as if from a "crystal nucleus", out of individual queries raised out of experience, and which eventually once the complete crystal has developed, again detach themselves from experience as purely intellectual structures that forever illuminate the world for us as closed-off theories. Thus the history of science is like the history of art, where the goal is to illuminate the world by means of intellectual constructs. In his "The End of Physics" in *Across the Frontiers* he adds that while physics consist of many closed-off systems, it is not possible to close off physics as a whole. Today it is necessary to seek out new and still more comprehensive closed-off theories, or "idealizations"

as he also calls them, which will include both relativity theory and quantum theory as limiting cases.

Closely related to his thesis of closed-off theories is Heisenberg's theory of abstraction. In "Abstraction in Modern Science" in *Across the Frontiers* he defines abstraction as the consideration of an object or a group of objects under one viewpoint while disregarding all other properties of the object. All concept formation depends on abstraction, since it presupposes the ability to recognize similarities. Primitive mathematics developed from abstraction, e.g. the concept of the number three. Mathematics has formed new and more comprehensive concepts, and thereby ascended to ever higher levels of abstraction. The realm of numbers was extended to include the irrational and complex numbers. This view is quite different from Bohr's, who believed that the mathematical formalisms used in physics have no descriptive semantical value but are merely symbolic, i.e. semantically vacuous, instruments for calculation and prediction, particularly if they contain complex numbers or represent more than four dimensions as in quantum theory. In Heisenberg's philosophy abstraction, the consideration of the real from a selective viewpoint, produces idealizations of reality which are axiomatic mathematical structures that become closed-off, as the historical development of science reveals the limitations of their applicability and occasions the creation of new theories.

In expounding his semantical doctrine of closed-off theories Heisenberg departed from Bohr. Comparison of their views reveals essential similarities, but it also reveals differences. Bohr's semantical views are stated in "Discussions with Einstein" where he says that Planck's discovery of the quantum of action makes classical physics an "idealization" that can be unambiguously applied only in the limit, where all actions involved are large in comparison with the quantum. A more elaborate statement is given in "The Solvay Meetings" in *Essays 1958/1962*. There he firstly says that unambiguous communication of physical evidence demands that the experimental arrangement and the reading of observations be expressed in common language suitably refined by the vocabulary of classical physics. Then secondly he states that in all experimentation this demand is fulfilled by using as measuring instruments bodies like diaphragms, lenses, and photographic plates, which are so large and heavy that notwithstanding the decisive role of the quantum for stability and properties of such bodies, all quantum effects can be disregarded in the account of their position and motion. Finally and thirdly he says that in classical physics we are dealing with an idealization according to which all phenomena can be arbitrarily

subdivided, and all interaction between measuring instruments and the object under investigation can be neglected or compensated for. Bohr seems to be using the term "idealization" as Heisenberg does, but he reserves it for the classical physics. He does not admit a separate set of distinctively quantum concepts, because he maintains an instrumentalist interpretation of the quantum theory formalism. In his view there are no quantum concepts defined by the equations of the quantum theory, but rather there are only classical concepts and the semantically uninterpreted mathematical formalism used for generating predictions expressed in classical terms.

Bohr's "Forms of Perception" and Neo-Kantianism

Having based his doctrine of closed-off theories on Bohr's philosophy of observation, Heisenberg attempted to relate Bohr's philosophy to the history of philosophy, and specifically to that of Kant. Heisenberg's statements are found in his "Recent Changes in the Foundations of Exact Science" (1934) in *Philosophical Problems of Quantum Physics*, in his "The Development of Philosophical Ideas Since Descartes in Comparison with the New Situation in Quantum Theory" in *Physics and Philosophy*, in his "Quantum Physics and Kantian Philosophy (1930-1932)" in *Physics and Beyond*, and in his "Planck's Discovery and the Philosophical Problems of Atomic Theory" in *Across the Frontiers*. Like Einstein, Heisenberg rejects the Positivist phenomenalism and advocates realism; he was never a metaphysical Idealist. In "Planck's Discovery" he states that quantum theory does not consider sense impressions to be the primary given, and that if anything is the primary given in quantum theory, it is the reality described with the concepts of classical physics. And in "Development of Philosophical Ideas Since Descartes" he describes his realistic variation on Kant's views with the phrase "practical realism", since in Heisenberg's view things rather than perceptions are the given for the human mind.

But while Heisenberg is opposed to Positivism as much as Einstein, his referencing the philosophy of Kant is not motivated by his anti-Positivism. Heisenberg is interested merely in relating Kantianism to the philosophy of observation he took from Bohr and incorporated in his doctrine of closed-off theories. In "Recent Changes in the Foundations of Exact Science" he says that in the field of philosophy of perception, Kant's philosophy has been put into a new light as a result of the critique of absolute time and Euclidian space by relativity theory and by the critique of

the law of causality by quantum theory, and that the question of the priority of the forms of perception and of the categories of the understanding must be reconsidered. He states that there are two apparently contradictory propositions that must be reconciled: On the one hand relativity theory and quantum theory have shown that our space-time forms of perception and the category of causality are not independent of all experience in the sense that they must for all time remain essential constituents of every physical theory. On the other hand, as Bohr taught, the applicability of the classical (i.e. Kantian) forms of perception and the law of causality are the premises of every objective experience even for modern physics. The physicist can only communicate the course of an experiment and the result of a measurement by describing the necessary manual operations and instrument readings as objective events taking place in the space and time known to our intuition. And he could not infer the properties of the observed object from the result of measurement, unless the law of causality guaranteed an unambiguous connection between measurement and object. Heisenberg resolves the contradiction between the two statements as follows: Physical theories can have a structure differing from classical physics, only when their aims are no longer those of immediate sense perception; that is to say, only when they leave the field of common experience dominated by classical physics. In "Quantum Physics and Kantian Philosophy" Heisenberg views Kant's philosophy of perception as a closed-off theory, as he elsewhere describes closed-off theories in physics. He compares the validity of Kant's philosophy to the validity of Archimedes' theory of the lever, and he states that Kant's theory is eternally true, just as Archimedes' theory is eternally true. Kant's analysis of perception represents true knowledge that applies wherever thinking beings enter into the kind of contact with their environment called "experience". Relativity theory and quantum theory have defined the limits of the *a priori* in the exact sciences in ways that could not have been known to Kant. The *a priori* has not been eliminated from physics, and Kant's analysis of how we come by our experiences is essentially correct. But the *a priori* has become "relativised" in the sense that classical concepts are *a priori* conditions for relativity and quantum theory, since classical concepts are necessary for experiments. Remarkably Heisenberg says that the progress of science has changed the structure of human thought, and has taught us the meaning of "understanding". In the closing paragraph of his "Quantum Physics and Kantian Philosophy" Heisenberg states that he has described the relationship between Kant's philosophy and modern physics from the perspective of Bohr's teachings.

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The importance of Heisenberg's discussion of Kant is that it treats the philosophy of perception. And the philosophy of perception in turn is important because it often serves as a philosophy of observation in philosophy of science and epistemology. In the context of the philosophy of modern physics the central problem is the semantics of modern physical theory. On the one hand where the semantics of the language of physics is said to be supplied by observation and perception, the traditional assumption is that perception is a natural cognitive process that predetermines the semantical content of language in an invariant, objective, and atemporal manner. On the other hand the history of science is a history of change of theory, which involves semantical change that is not easily reconciled with such a view of perception. Traditional philosophy of perception suggests a naturalistic philosophy of the semantics of language, while history of science suggests an artifactual philosophy of semantics. In anticipation of the discussions to follow about the views of other philosophers, it may be said that contemporary philosophers have addressed this semantical issue and specifically the topic of perception in a different manner than did Heisenberg or Bohr, or for that matter most earlier philosophers. Unquestionably there are limitations to what can be perceived; there are objects that are too small to be seen with the human eye, there are sounds that are pitched too high to be heard by the human ear, etc. Yet observation is permeated with learned ideas, permeated with interpretation, such that it has variability, subjectivity, and historicity that are not invariably associated with the outcomes of the functioning of our natural faculties for perception. The Pragmatist philosopher of science, Hanson, addressed this difficult mixture of nature and culture in knowledge by reconsidering the concept of perception in observation. Hanson's answer is the same as Einstein's admonition to Heisenberg, that theory decides what the physicist can observe. But Heisenberg does not consider this view in his discussion of his doctrine of closed-off theories or in his discussion of Kant, notwithstanding that he used it for his development of the indeterminacy relations. On the other hand a theory of knowledge based on perception is sometimes called a "psychologistic" theory of knowledge, and some philosophers object to psychologism, even when the particular psychologistic position advocated does not assert that the laws of logic are "laws of thought" in the sense of psychological laws. The notable contemporary example is the philosopher of science Karl Popper, who rejects psychologism, and separates observation from perception with his distinction between "world two", the domain of subjective psychology including perception, and "world three', the domain of

objective knowledge including observation. In this way Popper separates the roles of nature and culture, so that following Einstein, who said that theory decides what we can observe, Popper says that there is no observation without theory. The contemporary philosophers of science agree that knowledge is not predetermined by nature. Thus they depart not only from Kant and the Positivists but also from Bohr, Heisenberg and the other Copenhagen physicists.

On Scientific Revolutions

Heisenberg considers the development of modern quantum theory to be one of the two great scientific revolutions of twentieth century physics; the other in his view is relativity theory. Few would disagree. The complete title of his 1958 book is *Physics and Philosophy: The Revolution in Modern Science*. But by the 1960's the term "revolution" as used in connection with the development of science had become what Heisenberg calls a "vogue word" due to some similarities between scientific revolutions and social revolutions. Possibly the vogue status of the term is due to the popular monograph, *Structure of Scientific Revolutions*, written by Thomas Kuhn in the United States in 1962, but Heisenberg never references Kuhn, and their views are not the same. Heisenberg discusses his idea of revolution in science in a lecture delivered to the Association of German Scientists in Munich in 1969, which was published in English in 1974 as "Changes of Thought Pattern in the Progress of Science" in his *Across the Frontiers*. Heisenberg recognizes the operation of sociological forces in the scientific professions, but his views are different from those of Kuhn.

Heisenberg defines a "revolution" in science as a change in thought pattern, which is to say a semantical change. He states that a change in thought pattern becomes apparent, when words acquire meanings that are different from those they had formerly, and when new questions are asked. He does not reference his semantical thesis of closed-off theories in this context, although the episodes in the history of post-Newtonian physics that he cites as examples of scientific revolutions are the same as those that he also says resulted in new closed-off theories in the history of physics. And the semantical change that occurs in the transition to a new axiomatic theory and the closing off of the old one, is the change involved in the transition to a new thought pattern. The central question that Heisenberg brings to the phenomenon of revolution in science understood as a change in thought pattern, is how the revolution is able to come about. The occurrence of the

revolution is problematic due to resistance to the change in thought pattern offered by the cognizant profession. Heisenberg also expresses the question in more sociological terms, when he asks how a small group of physicists are able to "constrain" other physicists to make the change in thought pattern in spite of the latter's resistance to do so. Firstly he discusses the reasons for resistance. Then he discusses various proposed explanations about how the resistance is overcome.

In his discussion of the reasons for resistance he states that there have always arisen strong resistances to every change in the pattern of thought. The progress of science proceeds as a rule without much resistance or dispute; the scientist has by training been put in readiness to fill his mind with new ideas. But the case is altered when new groups of phenomena compel changes in the pattern of thought. Here even the most eminent of physicists find immense difficulties, because a demand for change in thought pattern may create the perception that the ground is to be pulled from under one's feet. A researcher who has achieved great success in his science with a pattern of thinking he has accepted from his young days, cannot be ready to change this pattern simply on the basis of a few novel experiments. Heisenberg states that once one has observed the desperation with which clever and conciliatory men of science react to the demand for a change in the pattern of thought, one can only be amazed that such revolutions in science have actually been possible at all. Undoubtedly the case in Heisenberg's experience is the desperation that he saw in Schrödinger's and especially Einstein's opposition to the new thought pattern represented by the Copenhagen interpretation of the quantum mechanics.

He then considers several possible answers to the question of how scientific revolutions can come about in spite of the resistances, of how the resistances are overcome. One answer that he rejects is that the revolution is due to a strong revolutionary personality. He maintains that no such strong personality could overcome the profession's resistance. Another answer that he rejects might be described as a variation on the conspiracy thesis, the view that a small group of physicists intended from the outset to overthrow the existing state of the science. He states that never in its history has there ever been a desire for any radical reconstruction of the edifice of physics; this is because at the onset of a revolution there is a very special, narrowly restricted problem, which can find no solution within the traditional framework. The revolution is brought about by researchers who are genuinely trying to resolve the special problem, but who otherwise wish to change as little as possible in the previously existing physics. It is precisely

the wish to change things as little as possible, which demonstrates in Heisenberg's opinion, that the introduction of novelty is a matter of being compelled by the facts. The change of thought pattern is enforced by the phenomena. He concludes therefore that the way to make a scientific revolution is to try to change as little as possible: it is an error to demand the overthrow of everything existing due to the risk of attempting a change that nature makes impossible. Small changes on the other hand show what is compelled by the facts, and in the course of years or decades enforce a change in thought pattern and shift the foundation of the science. An example of such a small change is Planck's quantum of action, which years later resulted in the modern quantum theory.

Having rejected the view that scientific revolution occurs due to a conspiracy either with or without a strong revolutionary personality, Heisenberg then considers the answer that the resistances to revolution are overcome simply because there is a "right" and a "wrong" in physics, and the new theory is right while the old theory is wrong. It is noteworthy that Heisenberg does not reject the thesis that there is a right and a wrong in the sense of a correct and an incorrect, and in view of his thesis of closed-off theories, it would be remarkable if he did. Furthermore he had explicitly rejected historical relativism in his "Quantum Physics and Kantian Philosophy". Still he finds that there is a problem with this answer as an explanation for overcoming resistances, namely that historically the right theory has not always prevailed. He cites as an example the dominance of the geocentric theory of Ptolemy over the heliocentric theory of Aristarchus. Therefore, while there are absolute standards for criticism of scientific theories, there still remains the question of why some correct theories succeed in gaining acceptance over the strong forces of resistance, while others do not, even though the rejected theories may be correct. Heisenberg then proposes his own answer. Scientists perceive that with the new pattern of thought, they can achieve greater success in their science than with the old; the new system proves to be more fruitful. Heisenberg states that once anyone has decided to be a scientist, he wants above all to get ahead, to be on hand when the new roads open up; it does not satisfy him merely to repeat what is old and has often been said before. Consequently the scientist will be interested in the kind of problems where there is something to be done, where he has the prospect of successful work. That is how relativity theory and quantum theory came to prevail according to Heisenberg. He describes this as a "pragmatic criterion of value", and he states that while one cannot always be certain that the right theory will always prevail,

nevertheless these are forces that are strong enough to overcome the resistances to a change in thought pattern.

Since Heisenberg is a principal participant in one of the great scientific revolutions in modern physics, his views based on his personal experience deserve singular consideration. He was undoubtedly impressed by the resistances offered to the Copenhagen interpretation by Schrödinger and especially by Einstein. While few contemporary philosophers of science accept Heisenberg's doctrine of closed-off theories with its naturalistic view of observation, which he uses to interpret his experience of scientific revolution, they recognize the operation of sociological forces including the thrust of opportunistic careerism. And they also recognize that semantical change occurs in scientific revolutions, and that the adjustment it imposes on the affected profession operates as a cause of resistance within it, even though they do not accept Heisenberg's theory of semantical change and permanence. Unlike others such as Kuhn, Heisenberg does not identify the institutionalized criteria for scientific criticism with the existing thought pattern, and he does not maintain that the revolution is a change with no institutional framework controlling it. Heisenberg avoids the historical relativism found by many in Kuhn's thesis, and which is explicitly embraced by Feyerabend. And one would not expect the proponent of the doctrine of closed-off theories and the advocate of Bohr's theory of observation to find the process of scientific criticism very problematic. The scientist is simply compelled by the facts, and the semantics of the statements of fact are not a problematic matter. Failure of the correct theory to overcome the forces of resistance, and indeed the very existence of those resistances, is due to the professional failure of those who cannot adjust to new thought patterns when the facts compel, and not to any inherently problematic character in the process of scientific criticism itself.

The contemporary Pragmatist philosopher of science can only wonder what Heisenberg might have said, were he to have followed through with Einstein's thesis that it is the theory that decides what the physicist can observe; how he would have addressed the consequent problem that the concepts used to describe the facts are supplied by the choice of thought patterns expressed in the theory. Yet the semantics of the statement of fact is not unproblematic, as Hanson attempted to demonstrate in his *Patterns of Discovery* (1958), where he considers at length the interpreted nature of the observations relevant in the choice between the geocentric and the heliocentric theories of the planetary motions. If what Heisenberg calls "the pragmatic criterion of value" determines the choice between the old and the

new theories, and each theory determines the facts, then a problem arises as to how facts can have any independently compelling force.

Heisenberg's Philosophy of Science

Heisenberg's rich and extensive philosophical writings can be related to the four basic questions addressed by contemporary professional philosophers of science.

Aim of Science

The question of the aim of science has a special importance in Heisenberg's philosophy, because it was explicitly developed to defend the Copenhagen interpretation of quantum theory against Einstein's explicitly formulated programmatic aim of all physics. Heisenberg's views are expressed in his "Notion of 'Closed Theory' in Modern Science" and in his "On the Unity of the Scientific Outlook on Nature" (1941) in *Philosophical Problems of Quantum Physics*. Einstein used his programmatic aim of physics to claim that the statistical quantum theory is "incomplete" in the sense that it does not represent an adequate explanation for the problem that it addresses, and that further research work is still needed. The reason it is still incomplete is that it is not consistent with the ontology of field physics, which describes physical reality as continuous in four dimensions and deterministic. Heisenberg denied Einstein's thesis that the microphysical theory must employ the same ontological concepts as those used in macrophysical field theory, and his doctrine of closed-off theories was motivated in part by his desire to show how multiple ontologies can co-exist in physics. This is Heisenberg's thesis of pluralism in science. The Copenhagen interpretation of quantum theory is complete in Heisenberg's view, because it is a closed-off theory, and like all closed-off theories it is not only a complete solution to the problem that it addresses, but it is also a permanently true solution. In Heisenberg's philosophy of science the aim of science is to progress through a sequence of closed-off theories, and it is not, as Einstein maintained, to progress toward a single and all-inclusive ontology. The result of physics pursuing its aim as Heisenberg views it, has been the architectonic scheme for physics, a scheme of closed-off theories which he delineates in his "Relation of Quantum Theory to Other Parts of Natural Science" in *Physics and Philosophy*.

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Discovery

In Heisenberg's treatment of the question of scientific discovery, two aspects may be distinguished: One is the syntactical or structural aspect, and the other is the semantical or the interpretative aspect, which also includes ontological considerations. The structural aspect pertains to the development of the new formal axiomatic system, the new mathematical theory. These new formal structures are the result of repeated failures of the conservative attempts by the researchers to extend a currently acceptable theory to explain phenomena in a new domain of experience, and eventually may result in a revolutionary development. Closely related to the first aspect is the second, the interpretative problem. When extension of Newtonian physics could not solve the problem of microphysics, and after the matrix mechanics was eventually developed by Heisenberg, the interpretation of the new matrix mechanics still remained problematic. Using Einstein's thesis that the theory decides what the physicist observes, Heisenberg reinterpreted the relevant observations in the Wilson cloud chamber experiment, and developed the uncertainty relation and its nondeterministic ontology. The new interpretation was accomplished by taking the new quantum theory realistically, as a description of the ontology of the microphysical world. When Einstein attacked the statistical quantum theory, he attacked only the second aspect, the Copenhagen interpretation with its nondeterministic ontological claim; he rejected the indeterminacy claim as a true description of the real world.

Explanation

Heisenberg's views on the question of scientific explanation are implicit in his position against Einstein's objections to the Copenhagen interpretation. Einstein's objection to the Copenhagen interpretation is that it is incomplete as a scientific explanation. This objection is a very traditional type of objection, because historically the concept of scientific explanation has been defined in terms of one or another ontology, and Einstein demanded conformity to the ontology defined by the concepts of field physics. Bohr placed himself and his Copenhagen colleagues at a disadvantage, when he employed the vocabulary of their critics by referring to the statistical quantum theory as "noncausal"; he accepted the definition of causality in terms of the ontology of classical physics and field theory. But Heisenberg also maintained that the revolutionary developments in physics

consisted in part of interpreting the new mathematical formalism realistically, such as accepting the field as a reality, accepting relativistic time as real time, abandoning the concept of absolute time, and most notably accepting the uncertainty relation as describing the real microphysical world as nondeterministic. This amounts to separating the concept of scientific explanation from any preconceived ontology, a view of scientific explanation that was quite radical in its time, even though it is now the common property of the contemporary Pragmatist philosophers of science.

Criticism

In striking contrast to his radical concept of scientific explanation, Heisenberg's treatment of the fourth question, the question of scientific criticism, is very conservative: he believed that his doctrine of closed-off theories enables him to explain how scientific theories can be permanently true. His views of explanation and of criticism represent a very unusual combination of views; historically philosophers and scientists have maintained that scientific explanations are permanently true, because as explanations, they describe correctly the one and only true ontology. Heisenberg's philosophy of scientific criticism includes a semantical thesis, which is a thesis of both semantical change and semantical permanence. Whether or not this semantical thesis is a sustainable one is certainly questionable, particularly when it depends on such curious processes as the semantics of words becoming "detached" from the variables occurring in the closed-off axiomatic theories, when the theories encounter the limits of their applicability. A philosopher of science such as Popper would dismiss such a thesis as a "content-decreasing" stratagem. If when a theory is criticized by an experimental test, the words expressing the test outcome describe something contrary to what the theory had predicted, then the attempt to save its truth claim by equivocation, by the "detachment" of the meanings describing the experimental outcome from the terms in the theory, only makes the theory tautological. In other words Heisenberg's doctrine in effect says a theory is true where it is true, and that where it is not true, it is not falsified, because it becomes silent, i.e. inapplicable.

Comment and Conclusion

A new philosophy does not spring forth as from the brow of Zeus - coherent, complete, and fully formed. It struggles to emerge from the

confusion produced by the inevitable conflict between new seminal insights and old conventional concepts. It is not surprising, therefore, that there should exist an inconsistency between the seminal insights in Heisenberg's philosophical reflections on his pioneering findings described in his autobiographical accounts and the conventional concepts in his systematic philosophy of science set forth as his doctrine of closed-off theories. In "Bohm and the 'Inevitability' of Acausality" in *Bohmian Mechanics and Quantum Theory: An Appraisal* (1996) Mara Beler takes a cynical perspective to Heisenberg's inconsistency, arguing that he had neither belief nor commitment, but only a selective and opportunistic use of Bohrian doctrine for the finality of the Copenhagen orthodoxy. Human motives are seldom unmixed, but there is likely more to the story. Clearly the principal source of this inconsistency in Heisenberg's philosophy is the conflicting influences of Bohr and Einstein, and the conflict has its basis in two fundamentally different philosophies of the semantics of language, particularly where the relevant language is the vocabulary used to describe observations. The emerging new philosophy of language in philosophy of science is the artifactual thesis of semantics and the prevailing old one is the naturalistic thesis. Bohr's philosophy of language is that the semantics of language is the natural product of perception, such that concepts used for observation are what he calls the "forms of perception" that have their information content determined by nature and the natural processes of perception. Einstein's philosophy of language on the other hand is that the semantics of language is an artifact, a "free convention", a cultural product instead of a natural product, such that concepts and categories used for observation in physics do not have their information content specifically determined by the natural processes of perception.

It was evident to Heisenberg as well as to every other physicist at the time that revolutionary revisions had been made in twentieth-century physics. Heisenberg wanted to explain how such developments in the history of science could produce correspondingly revolutionary revisions in the semantics of the language of physical theory. Heisenberg's response was his doctrine of closed-off theories, and the philosophy of language that he used for his semantical theory was greatly influenced by Bohr. This doctrine restricts semantical revision to the description of phenomena that lie beyond ordinary perception, and thereby retains semantical permanence for the description of phenomena accessible to everyday observation and described by the language and concepts of classical physics. According to Heisenberg's doctrine of closed-off theories Newtonian physics is

permanently valid and serves as the observation language for physics, because it is necessary for reporting experimental measurements and other observations. This is similar to the Positivist philosophy of science, which also assumes a naturalistic philosophy of the semantics of language and the semantical permanence of observation language.

In Heisenberg's semantical theory all observation must be with concepts either of classical physics or of "everyday" language. In his mature version of his doctrine of closed-off theories these concepts are not the same. The everyday concepts have a "lack of precision" or vagueness, while the concepts of classical physics have their content rigidly and precisely fixed by their occurrence in the context consisting of the laws constituting the axiom system of Newtonian physics. The concepts of quantum physics also have their content fixed by their occurrence in the context consisting of the laws constituting the axiom system of quantum physics. What is significant is not just that the laws may be expressed in axiomatized systems, but that the quantum concepts are contextually determined in contexts that are alternative concepts relative to classical concepts, because the laws of classical and quantum physics are mutually inconsistent. And most notably in Heisenberg's view the quantum concepts are not merely alternative resolutions of the vagueness in everyday concepts, because according to the doctrine of closed-off theories the quantum concepts cannot be used for observation. The fact that classical and quantum concepts occur in mutually inconsistent axiom systems of laws implies that, when these concepts are associated with the same descriptive term or variable, they are alternative meanings making that common term equivocal.

The equivocal relation between classical and quantum concepts is illustrated in the cases of the terms "position" and "momentum", which occur in both classical and quantum physics. On the one hand the advocates of the Copenhagen interpretation of the quantum theory argue that in practice the concepts of classical physics must operate in descriptions of the macrophysical experimental apparatus and observation measurement. This classical semantics includes the idea that nature is fundamentally continuous, and the idea that in principle the measurements can be indefinitely accurate, notwithstanding the fact that in practice the degree of accuracy is limited. On the other hand there are also meanings for these terms that are distinctive of quantum physics, and this semantics which is defined in the context of the indeterminacy relations, includes the ideas that nature is fundamentally discontinuous and that the accuracy of the joint measurement of momentum and position is limited by Planck's constant. Therefore, in order for

observation to be possible in quantum physics there must exist an equivocation for every term common to classical and quantum physics, such that for every quantum concept determined by the context of quantum laws there must be a corresponding classical concept for observation determined by the context of classical physics. Such is Heisenberg's doctrine of closed-off theories, his explicit and systematic philosophy of science. Yet Heisenberg's use of Einstein's admonition for describing the tracks in the Wilson cloud chamber, which led to his subsequent development of the indeterminacy relations, does not agree with his doctrine of closed-off theories. Einstein's admonition consists of the semantical thesis that it is the theory that decides what the physicist can observe, and for microphysical experiments this thesis implies that the quantum theory supplies the concepts for observation.

Contrary to Heisenberg's semantical doctrine of closed-off theories, classical concepts are not necessary for observation, variables in the quantum laws are not equivocal, and all the concepts in the quantum theory are quantum concepts including the concepts used for observation. It is possible with a metatheory of semantical description to follow through with Einstein's admonition and to say that theory decides what the physicist can observe, because the concepts used for observation are quantum concepts. Such a new semantical theory is needed, because Heisenberg had premised his doctrine of closed-off theories on the naturalistic philosophy of language. Attempts to preserve a permanent semantics for observation, while at the same time to explain the semantical revisions produced by the revolutionary developments in theory, results in attributing equivocation to language that in practice physicists are routinely able to use unambiguously. The historic twentieth-century scientific revolutions motivated post-Positivist professional philosophers of science to reject the naturalistic philosophy of language, and to accept the artifactual philosophy of language. It is necessary to consider further how to describe the semantics both of quantum theory and of experimental observation, in order to exhibit how concepts are culturally determined as linguistic artifacts instead of predetermined as products of nature, and to explain why semantical change does not involve complete equivocation.

Heisenberg's doctrine of closed-off theories contains certain basic assumptions that are in need of reconsideration. One is the tacit assumption that all concepts are indivisible or simple wholes, that must be either completely different or completely the same, such that classical and quantum concepts are simply and wholly equivocal. The other basic assumption is

that observation language must be exclusively associated with macroscopic phenomena. Both of these basic ideas contain errors. Firstly it is incorrect to assume that concepts in physics or in any other discourse are simple wholes that cannot be analyzed into component parts. And secondly it is necessary to reconsider the Copenhagen school's basis for dividing the relevant language into statements of experiment and statements of theory. Specifically rejection of the naturalistic philosophy of language implies rejecting two mental associations that occur in the doctrine of closed-off theories. The first is the classical-macroscopic-observation association, and the second is the quantum-microscopic-theoretical association. Consider firstly an alternative to the wholistic view, and how it affects Heisenberg's thesis of equivocation.

Reflection on the common occurrence of looking up a word in a unilingual dictionary or thesaurus reveals that the meanings of words are not simple wholes, but rather have component parts that are identified by the defining words occurring in the dictionary definition or lexical entry. These dictionary definitions give semantical descriptions of the meanings they define, and in order to function in this way they always must have the force of universally quantified statements accepted as true. Dictionary definitions are often viewed as describing the complete meaning of the term, but dictionary definitions are minimal statements, and by no means give complete meaning. Usually an understanding of the meaning of a technical term requires a larger context consisting of a discourse having many statements containing the term. Such larger context may be examined with the aid if a key-word-in-context computer program. Since Hempel rejected the separation of the meaning-specification and descriptive functions in analytic statements and since Quine rejected the analytic-synthetic distinction, all universal empirical or "synthetic" statements may be viewed as also definitional or "analytic". Thus if one were to make a list of logically consistent universally quantified affirmative categorical statements containing the common term as the subject term with each statement accepted as true, then the predicates in each of the mutually consistent statements constituting the list would describe part of the meaning of the common subject term, and the entire list as well as each statement in it may be called a "semantical description" of the common subject term's univocal meaning. A semantical description consists of the language context in which the descriptive term's meaning is determined and described by a set of universal affirmations believed to be true.

This contextual determination of the semantics of language is the essence of the artifactual thesis. Quine calls this context the "web of beliefs". A term is equivocal if any of the universal affirmations in the semantical description are mutually inconsistent. This equivocation is made explicit if the predicates of the inconsistent universal affirmations can be related to one another by universal negations accepted as true. The two meanings in the equivocation have separate semantical descriptions which can be exhibited when the original list is subdivided into mutually exclusive subsets with each containing only mutually consistent universal affirmations, such that each subset is a semantical description of one of the two different meanings of the equivocal term instead of each functioning as a description of different parts of the one meaning of a univocal term. The equivocations postulated by Heisenberg's doctrine of closed-off theories applied to microphysics are the result of the logical inconsistency between the axiomatic systems of classical and quantum physics. Thus there exists equivocation with each axiomatic system, a separate semantical description list for any common subject terms such as "position" or "momentum".

In addition to the properties of equivocation and univocation there is another aspect of language called vagueness. Equivocation and univocation are properties of terms, while vagueness and clarity are properties of meanings. Thus terms are univocal or equivocal relative to meaning, but meanings are clear or vague relative to one another, and thus indirectly relative to the extensions they reference. Two concepts are clear in relation to one another, if they can be related to each other by universal affirmations or negations accepted as true, and they are vague in relation to each other if they cannot be so related by any universal statements. Adding to a univocal term's semantical description list any universal affirmations or negations believed to be true has the effect of resolving the vagueness in the concept associated with the term by explicitly adding or excluding meaning. Every meaning is vague and admits to further resolution or clarification, because its semantical description can always be increased by additional universal statements believed to be true, although responsible addition in science usually requires additional research. Waismann has called this inexhaustible residual vagueness the "open texture" of concepts.

Some comments are in order about mathematics in empirical science. Firstly mathematics supplies the grammar for much of the language of science, and it is a distinctive language. Just as statements in logic and ordinary discourse may be said to constitute what Carnap called the "thing language" and what Whorf called the language of substantives, so too the

equations and inequalities constitute what may be called the "measurement language". Statements in substantive language, which describe the measured phenomenon, the measurement procedures, and the design and operation of any apparatus employed, must be used to relate substantive language to measurement language. The controversy about the interpretation of the quantum theory equations, which are in measurement language, is about statements in substantive language that describes what is a real entity in the quantum domain. The Copenhagen physicists including Heisenberg maintained that the wave and particle are two alternative aspects of what is really one entity. Lande said that only the particle is a real physical entity, Schrödinger believed that only the wave is physically real, and Bohm said that wave and particle are both co-existing and separate real entities. These issues become very philosophical, when they are about explicitly expressed criteria for identifying an entity. Heisenberg was not explicit in his criterion, but Lande and Born had their different yet explicit philosophical criteria. Both brilliant physicists and bull-headed philosophers have spent most of the twentieth century arguing about this issue, and they continue to do so, because even with the practice of scientific realism, the language of mathematics is silent about the ontological category of physical *entity*. Any future resolution will be the result of new and superior experimental and observational techniques, but mathematics alone cannot express the findings that will resolve the issue.

A second comment about mathematics in physics is that in the empirical sciences equations and inequalities express universal logical quantification, which is changed to particular logical quantification when any of their constituent variables are given numerical values by measurement, or by calculation with the equation whose initial conditions have been satisfied by measurement. Each measurement performance is an individual measurement instance, which is not the same as an individual entity. Different measurement performances will likely result in different measurement values, either due to measurement errors in the execution of the measurement procedures described in the statements of an experimental design, or due to different initial conditions in the ranges of the variables. An equation is not given particular logical quantification merely by associated inequalities expressing limits on the range of possible values of its variables, as in the indeterminacy relations in quantum theory. Such conditions may severely restrict the range of applicable values. But there is always a universal claim made by the equation, because there are an indefinitely large number of possible measurement performances in

repeatable experiments. Also equations are not given particular quantification, when only their parameters such as constant coefficients are given specific values. Furthermore no mathematically expressed theory need be a more "general theory" relative to any other theory, in order to be a logically universal statement. In the case of quantum physics the microphysical quantum theory need not also be a macrophysical theory, in order to be a universal theory, just as it need not be a biological or a sociological theory, in order to be a logically universal expression. Logical reductionism is not a condition for logical universality. Therefore equations can express universality like substantive statements in logic, and they may be included in semantical description lists to add to the semantics of a meaning complex associated with a term and to reduce the meaning's vagueness, if the term is a mathematical variable.

To summarize this alternative to the wholistic view: the meanings associated with descriptive terms are not simple wholes, but are complexes having component parts, which in turn are meaning complexes associated with other descriptive terms to which the former are related in universal affirmations believed to be true. The composite meaning complex is exhibited in a semantical description, which consists of a list of one or several universal affirmations having a common subject term with its component parts exhibited as predicates. The universal affirmations may also include mathematical expressions with descriptive terms appearing as variables. Vagueness is a property of concepts, and exists to the extent that descriptive terms cannot be related to one another in universal statements believed to be true. Vagueness limits the size of a semantical description list, and thus also limits the propagation of semantical change through the web of beliefs.

Consider next the relation between the language of observation and the language of theory, the second basic assumption in the doctrine of closed-off theories. The word "theory" is still used conventionally to refer to Newton's "theory" of gravitation, to Einstein's "theory" of relativity, and to the quantum "theory", even though the physics profession had decided many years ago either to accept or to reject these expressions as physical explanations. In this conventional usage the term "theory" does not have the same meaning as it did when these expressions were firstly advanced for testing as proposed explanations of problematic phenomena. When they were firstly proposed, these expressions represented statements that had a much more hypothetical status in the judgment of the profession than they do today, and they were typically topics of controversy. There is, therefore,

an ambiguity between "theory" understood as an accepted or rejected explanation, and "theory" understood as a tentative proposal submitted for empirical testing. Unlike the former archival understanding, which Hanson calls "almanac" science, the latter Pragmatist understanding of "theory", which Hanson calls "research science", pertains to the function of language at the frontier of the development of a science, where the function under consideration is empirical testing. Only this latter understanding is strategic in the Pragmatist philosophy of science, even though the former meaning and still conventional usage may occur in its expository discourse. From this functional or Pragmatist view theories may be defined as universal statements that are proposed for testing, and explanations are former theories that have been tested and not falsified. Theory that is tested and not falsified by a competent test ceases to be a theory and is given the status of an explanation, even though there may be other tested and nonfalsified former theories addressing the same problem also having the status of explanations accepted by some scientists in the same profession. Some scientists are uncomfortable with this pluralism, but the contemporary Pragmatist philosophers recognize such pluralism as historically characteristic of science.

In an empirical test the semantics of the vocabulary in all the relevant discourse is controlled by a strategic decision that is antecedent to the performance of the test. This is the decision as to what statements are presumed for testing and what statements are proposed for testing. The former language is the explicit statements of test design together with usually many tacit assumptions, and the latter is the explicit statements of the theory. This decision is entirely pragmatic, since it is not based on the syntactical or the semantical characteristics of language, but rather is based on the use or function of the language, namely empirical testing. The test design statements are those that by prior decision and agreement among cognizant members of the profession have the status of definitions. These statements are presumed to be true regardless of the outcome of the test, and serve to identify the subject of investigation throughout the test. The theory is the language that by prior decision and agreement among the cognizant members of the profession has the less certain status of a hypothesis. The hypothesis is believed to be true to the extent that it is considered worthy of testing, although the proponent and his entourage of cheering advocates may be quite firmly convinced. But if the test outcome is a falsification, then it is the statements of theory and not the statements of test design, that are judged to have been falsified. However, a falsification may lead some interested

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scientists, such as the theory's proponent and advocates, to reconsider the beliefs underlying the test design, even while admitting that the test was executed in accordance with its design. This role reversal between test design and theory may result in productive research. When the falsified theory is made a test design statement characterizing the problematic phenomenon, the problem has become reconceptualized. As Conant discovered to his dismay, the history of science is replete with such prejudicial responses to scientific evidence that have been productive and strategic to the advancement of basic science in historically important episodes.

The decision distinguishing test design and theory language made prior to the experiment may but need not result in identifying mathematical equations as the statements of theory and of identifying colloquial discourse or substantive language as the statements of test design. The decision is not based on syntactical characteristics of the language, and the test design statements often include mathematically expressed statements together with statements in substantive language describing the measured phenomenon, the measurement procedures, and the design and operation of the measurement apparatus. Even more relevantly the decision is not based on semantical criteria, as advocates of the naturalistic philosophy of the semantics of language believe. The decision is not based on any purportedly inherent distinction between observation and theory, whether or not, as in the case of quantum theory, the observation concepts are called "classical" or "macroscopic", and the theoretical concepts are called "quantum" or "microscopic". The distinction between statements of test design and statements of theory is neither syntactical nor semantical; it is distinctively and entirely pragmatic.

Consider the language of an empirical test before the test is executed. In order for the test design statements to characterize evidence independently of the theory proposed for testing, the test design statements and the theory statements must be logically independent. Neither set of statements may be merely a transformation of the other, and the test design statements may neither deductively imply nor contradict the theory or any of its alternatives. Furthermore the statements of the theory are too hypothetical to function as definitions, except perhaps for the proponent and other advocates of the theory, who may believe in the theory as strongly as they believe in the truth of the test design statements. But for all those critical researchers for whom the test is a decision procedure, the semantical consequence of the logical independence and hypothetical status of a theory relative to the universal

statements of test design, is that each of the terms common to both the test design statements and theory statements have their semantics defined in relation to the meanings of the other terms in the test design statements, such that they characterize the subject matter of the experiment, but not defined in relation to the meanings of the other terms of the theory. In other words the theory statements are not included in the same semantical description list as the test design statements, even though both sets of statements are mutually consistent and contain the same common subject term. The meaning of each term common to the test design and theory statements is therefore vague with respect to the meanings of the other terms of the theory. And on the artifactual thesis of the semantics of language the observation language in turn is merely the test design statements with their logical quantification changed from universal to particular, to enable their use to describe the particular ongoing or historical experiment performance. The test design statements similarly supply the vocabulary that describes the observed test outcome, even if the outcome contradicts the claims of the tested theory, thus falsifying the theory.

Consider next the language of the empirical test after the test is executed. When the test is executed, a falsifying test outcome produces no semantical change except for the proponents and advocates of the theory, who had been convinced of the theory's truth, and then choose to reconsider their belief in the theory due to the test outcome. But a nonfalsifying outcome produces a semantical change, especially for the critics of the theory for whom the test is a decision procedure. After the test the theory no longer has the hypothetical status that it formerly had merely as a proposal, but assumes the status of an explanation, which is neither more nor less contingent than other accepted universal empirical statements including the test design statements. The semantical outcome is that both the test design statements and the theory statements (now elevated to the status of an explanation) are semantical rules exhibiting the composition of the meanings of the univocal terms common to both sets of statements. Those component parts defined by the test design statements remain unchanged. But the semantical descriptions for these terms now include not only the test design statements but also the statements constituting the tested and nonfalsified theory. These theory statements are additional information learned from the test outcome that resolves some of the vagueness in the vocabulary terms common to both the theory and the test design statements. In summary: the descriptive terms common to both test design and theory statements have part of their semantics defined by the test design statements throughout the

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test, both before, during, and after the test is executed. And these common terms have part of their semantics augmented and thus defined by the statements of the tested and nonfalsified theory added after the test.

In Heisenberg's doctrine of closed-off theories the naturalistic philosophy of language requires retention of the Newtonian concepts for observation in the context of the quantum theory. But the resulting equivocation is unnecessary, if it is remembered that the Newtonian concepts are never involved, since the Newtonian theory is a falsified microphysical theory. It is sufficient to use a less precise vocabulary that Heisenberg calls "everyday" words used by physicists in order to describe the experimental set up, which is a macrophysical phenomenon. The meanings of these everyday concepts are vague about the fundamental constitution of matter. After the quantum theory was recognized as exper-imentally adequate, the vagueness in these everyday concepts was resolved by the equations constituting the statements of the quantum theory, because the quantum theory is the tested and nonfalsified theory, which after the test became a semantical rule contributing meaning parts to the complex meanings of these univocal terms. For this resolution of vagueness to occur it is not necessary for the Newtonian macrophysical laws to be made logical extensions of the quantum theory by logical reduction procedures, because the Newtonian theory is falsified as a microphysical theory. Nor is it necessary for the Newtonian macrophysical laws to be replaced by a macrophysical theory that is an extension of the quantum laws. The univocal semantical thesis neither implies nor requires Hugh Everett's "many worlds" interpretation (which furthermore is in principle empirically untestable), nor does it imply or require any other reductionist development of a macrophysical quantum theory, i.e. a macrophysical theory which is deductively or reductively a logical extension of the microphysical quantum theory. It is sufficient merely that the scientist realize that the nonfalsifying test outcome has made the quantum theory and not classical physics an empirically warranted microphysical theory.

Heisenberg's doctrine of closed-off theories is incorrect, and Einstein's semantical thesis expressed in his admonition to Heisenberg is correct, because the vocabulary used for observation after the quantum theory's acceptance is a univocal vocabulary with meaning parts from the quantum theory. The descriptive terms in the equations of the quantum theory contribute to, and thereby resolve some of the vagueness in, the meaning complex associated with the descriptive terms used for observation. Thus the quantum theory decides what the scientist observes in the Wilson cloud

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chamber. The macrophysical description is not in contradiction to the microphysical quantum theory including the indeterminacy relations. The quantum concepts are included in the univocal meaning complexes associated with the observation description. The Newtonian concepts were never included, because the macrophysical description never affirmed a Newtonian microphysical theory.

In his "Remarks on the Origin of the Relations of Uncertainty" in a memorial volume dedicated to him titled *The Uncertainty Principle and Foundations of Quantum Mechanics* (1977), which was in press at the time of his death in 1976, Heisenberg concludes the brief four-page article by saying that there have been attempts to replace the traditional language with its classical concepts by a new language which should be better adapted to the mathematical formalism of quantum theory. But he adds that during the preceding fifty years, physicists have preferred to use the traditional language in describing their experiments with the precaution that the limitations given by the uncertainty relations should "always be kept in mind". He concludes that a "more precise" language has not been developed and in fact it is not needed, since there seems to be general agreement about the conclusions and predictions drawn from any given experiment in the field. Regrettably Heisenberg never repudiated his doctrine of closed-off theories. But contrary to his doctrine of closed-off theories, Heisenberg's statement that the contemporary physicist must keep quantum effects "in mind" when the physicist is describing macrophysical objects, even while not explicitly accounting for quantum effects that are experimentally undetectable in the circumstances, is *prima facie* evidence of a semantical change in the univocal macrophysical vocabulary used to describe experiments due to the development of quantum theory. In other words a "more precise" language with a less vague semantics has in fact evolved. This semantical evolution consists in the fact that the concepts employed for description contain component parts from the quantum theory. That is how the limitations of the uncertainty relations are "always kept in mind": they have become built into the semantics of those terms, even when those terms are used to describe observations.

Heisenberg's semantical theory of equivocation is the result of the acceptance of the naturalistic philosophy of the semantics of language together with the assumption that meanings are simple, indivisible wholes. However, all such views are untenable, because they imply what can only be called "double think". The equivocation thesis demands that the modern physicist indulge in a contrived cognitive duplicity with himself, a pretext of

simultaneously both knowing and not knowing the modern quantum theory. But concepts are not known like physical objects to which one may simply close one's eyes; they *are* knowledge. Scientists never did in practice carry on the kind of cognitive duplicity that the equivocation semantical theses require, and since the ascendancy of the contemporary Pragmatism, philosophers no longer expect that they should. Heisenberg might have obtained greater utility from his insightful idea of "everyday" concepts, had he rejected Bohr's philosophy of observation language, and realized that neither these everyday concepts nor the Newtonian concepts nor any other concepts are inherently observational. In the Pragmatist perspective "everyday" concepts are distinctive only because they are vague in a very strategic fashion: they are the concepts used in test design statements, and are vague relative to the concepts in the theories proposed for testing prior to execution of the test and prior to the production of a nonfalsifying test outcome. In the case of the quantum theory experiments, everyday concepts are vague because they are not defined by the axiomatic systems of either the Newtonian or the quantum theories or of any other proposed microphysical theory prior to the execution of the tests.

In "On the Methods of Theoretical Physics" in *Ideas and Opinions* (1933) Einstein said that if you want to find out anything from the theoretical physicists about the methods they use, stick closely to one principle: don't listen to their words, but rather fix your attention on their deeds. This might be construed as good advice for anyone attempting to understand Heisenberg's philosophy of science. The philosophy of science that Heisenberg practiced as a result of Einstein's influence and chronicled in his autobiographical works, is historically more important than the one he expounded as a result of Bohr's influence and set forth as his doctrine of closed-off theories. In the philosophy he practiced, he anticipated the contemporary Pragmatist philosophy of science by at least a quarter of a century. Because contemporary Pragmatism is based on an artifactual view of the semantics of language, it implies the interdependence of belief and semantics. On the naturalistic view of semantics, the truth of empirical statements is dependent on meanings which in turn are determined independently by the natural processes of perception which are thought to capture some presumed or preferred ontology. On the artifactual view truth and meaning are mutually determining in empirical statements believed to be true. Thus on the artifactual thesis, when Heisenberg used Einstein's admonition for developing the uncertainty relations, he had made a prior commitment to the empirical truth of the equations expressing them, and he

then used their concepts for his theory-dependent observation of the Wilson cloud chamber tracks made by the free electron. Years later Feyerabend will call this counterinduction even though he seems never to have recognized its occurrence in Heisenberg's practice. Had he done so, he might have spoken of the Galileo-Einstein-Heisenberg tradition, and produced a philosophy of quantum theory different from than his relativist philosophy.

Furthermore, given the Pragmatist thesis of scientific realism, the thesis that empirically warranted discourse has a semantics describing an ontology, the artifactual philosophy of the semantics of language implies the mutual determination of truth and ontological claims. In scientific realism, however, the truth is the empirical warrant earned by testing with no falsification. Heisenberg too practiced this, when unlike Bohr, he reported that he construed the quantum mechanics realistically when he imitated Einstein's realism. He said the decisive step in the development of special relativity was Einstein's rejection of the distinction between apparent time and actual time in the interpretation of the Lorentz transformation equation, and his taking Lorentz's apparent time to be physically real time while rejecting the Newtonian absolute time as real time. He said he took the same kind of decisive step, when he inverted the question of how to pass from an experimentally given situation to its mathematical representation, by affirming that only those states represented as vectors in Hilbert space can occur in nature and be realized experimentally. This is not a positivist claim about mere phenomenal appearances; it is an ontological claim backed by the theory's empirical warrant as tested and not falsified. Unlike Bohm's hidden variable ontology Heisenberg's realist step did not involve creating "interpretations" by supplementing the quantum theory with additional characterizations that are not affirmed by the tested and nonfalsified theory. Nor is it like Bohm's informal characterizations that are separable from the quantum theory, and which enable no new empirical tests or new predictions, and therefore offer no empirically warranted ontological claims.

Similarly for the *potentia* ontology, if contrary to Heisenberg *potentia* is taken to refer merely to the fact that position and momentum measurement instances cannot both occur simultaneously, such that they may be taken realistically as a manifestation of reality - rather than supplementary claims about entities, which cannot even be referenced in mathematical measurement language. Initially the quantum theory's stark and spartan realism was strange, and "wave" was a metaphor. But metaphor is new and unconventional meaning that convention converts to dead metaphor, i.e. new literal meaning. Contemporary realists can let the theory and experiments

change the old literal meanings to new literal meanings, as continuing experimental research will further enrich the theory's descriptive semantics and ontology. Today's pragmatic scientific realism is the thesis that a theory's ontology - its view of reality - is described by the semantics of language that is defined by the context of universal discourse accepted as empirically true. This language characterizing manifestations of mind-independent reality includes the empirically tested and nonfalsified theory, all its associated test-design language needed for measurement and/or other observation, and descriptions of additionally related experimental findings. Unlike both the Bohm and the Bohr interpretations this language characterizing manifestations of reality *neither affirms nor denies* supplemental ontological claims that lack of warranting empirical evidence.

Heisenberg's practice of scientific realism anticipates Quine's doctrine of ontological relativity and also Quine's rejection of all first philosophy, namely the use of prior ontological commitments such as Einstein's commitment to determinism as criteria for scientific criticism. Contemporary Pragmatists like Quine reject ontological criteria in scientific criticism. They admit only empirical criteria for scientific criticism, and let the statement of the empirically tested and nonfalsified theory describe both the semantics and the ontology of the theory's domain.

KARL POPPER AND
FALSIFICATIONIST CRITICISM

Karl Popper (1902-1995) was born in Vienna, Austria. He enrolled in the University of Vienna in 1918, where he studied physics, mathematics, and philosophy. In 1928 he received his Ph.D. for a dissertation titled *On the Problem of Method in the Psychology of Thinking*. He never returned to the subject of psychology again during his professional career, because he became convinced that methodology of science is exclusively a matter of logic and objective knowledge instead of psychology. Popper was personally acquainted with Rudolf Carnap and other members of the Vienna Circle, and although he had been invited to address the group at a meeting in which he set forth his philosophy of science, he was never a member of the Circle. In 1937 he was appointed a senior lecturer to Canterbury University College in Christchurch, New Zealand, and then in 1945 he was appointed to a readership at the London School of Economics, University of London. In 1949 he was made professor of logic and scientific method at the London School. He was knighted in 1964.

Einstein's Influence and the Falsificationist Thesis of Criticism

In his intellectual autobiography in Schilpp's *The Philosophy of Karl Popper* (1974) Popper states that Einstein was the most important influence on his thinking. The influence was not a personal one, since Popper and Einstein did not actually meet until 1950; the influence was through Einstein's published works. The year 1919 was the fateful year in Popper's intellectual life. At that time he was interested in the views of several thinkers including Marx's theory of history, Freud's theory of psychoanalysis, and Alfred Adler's theory called "individual psychology."

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Popper relates in his "Science: Conjectures and Refutations" (1957) in *Conjectures and Refutations* (1963), that he had come into personal contact with Alfred Adler and cooperated with Adler in the latter's social work with children and young people in the working class districts of Vienna during the last years of the Austrian Empire and the subsequent revolution. In the summer of 1919 Popper became dissatisfied with the views of Marx, Freud and Adler, because the persons who accepted and advocated these theories were strongly impressed by the theories' purported explanatory power, and because study of these theories had the effect of an intellectual conversion or revelation. Most objectionable to Popper was the fact that once the reader's eyes were opened to the theory, he found that the theory was verified everywhere one might think of applying it. Unbelievers were dismissed as persons who could not see the verifications. In Popper's view the apparent strength of these theories' purported "explanatory" power is their principal weakness.

Popper saw in Einstein's theory a striking contrast to the situation he found in the views of Marx, Freud and Adler. Eddington's solar eclipse observations in 1919 brought the first important test to bear upon Einstein's relativity theory of gravitation. This test was distinctive, because in the test there was a risk involved in the theory's prediction. Had Eddington's observations showed that the predicted effect is definitely absent, then Einstein's theory would simply have been refuted. And the risk in Einstein's case was very great, since the predicted effect was different from what was expected from Newton's theory, which had long demonstrated great success culminating with the discovery of the planet Neptune. In his autobiography Popper said that what impressed him most was Einstein's own clear statement that he should regard his theory of relativity as untenable, if it should fail certain tests. This was an attitude that was very different from the dogmatic attitude of the Marxians, Freudians, and Adlerians. Einstein was looking for crucial experiments where agreement with his predictions would by no means establish his theory, but where disagreement with his predictions, as Einstein was the first to say, would show his theory to be untenable. Thus in 1919 Popper concluded that the critical attitude, which does not look for verifications but rather looks for crucial tests that can refute the tested theory, is the correct attitude for science, even though the crucial tests can never establish the theory. This is Popper's falsificationist philosophy of scientific criticism, the central thesis of his philosophy of science.

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Explanation, Information, and the Growth of Science

Popper's philosophy recognizes the dynamic character of science that is not recognized in the philosophy of the Positivists. His statements on the dynamics of science are found in appendices to the 1968 edition of his *Logic of Scientific Discovery*, in his "Truth, Rationality, and the Growth of Scientific Knowledge" in his *Conjectures and Refutations*, and in "The Rationality of Scientific Revolutions" in *Problems of Scientific Revolutions* (ed. Harre, 1975), as well as elsewhere in his literary corpus. His falsificationist thesis is not only a philosophy of scientific criticism but also a philosophy of scientific explanation and of the growth of scientific knowledge. As a philosophy of scientific criticism, it says that the empirical test outcome can never establish or "verify" a scientific theory, but can only refute or "falsify" the theory. And even before a theory's claims are considered for testing, it is possible to determine whether or not it is a scientific explanation: it is not a scientific explanation if it is not empirically testable. Another way that Popper describes this condition is that what makes a theory scientific is its power to exclude the occurrence of some possible events, and he calls the singular statements that describe these excluded events "potential falsifiers." This way of speaking introduces his idea of various degrees of explanatory power: the more that a theory forbids or excludes and therefore the larger the class of potential falsifiers, then the more the theory tells us about the world. Popper calls the variability of degree of explanatory power the "amount of information content" of a theory or explanation. The idea of the amount of information content may be illustrated by reflection on the logical conjunction of two statements α and ß. It is intuitively evident that the conjunction αß has no lesser amount of information content than do the component statements taken separately, and it usually has more information content than its components. This is because there are more potential falsifiers for the conjunction than for the component statements taken separately; the conjunction is false if either component is false. In some contexts Popper calls information content "empirical content", and he calls the falsifiability of the theory its "testability." All of these terms refer to a logical relation between a theory or a hypothesis and its class of potential falsifiers.

Popper relates the idea of information content to probability theory. He says that the amount of information content is inversely related to the degree of probability that may be associated with a hypothesis. This view can be illustrated also by the logical conjunction: if the probability value

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P(α) is associated with the statement α and the probability value P(β) is associated with the statement β, then by the probability calculus the probability P($\alpha\beta$) associated with the conjunction $\alpha\beta$ must be less than the probability values P(α) and P(β). Therefore as the information content of a theory increases, the associated probability must decrease. Popper maintains that the whole problem of the probability of hypotheses as viewed by Carnap is misconceived, because on Carnap's idea of degree of confirmation, scientists should prefer statements having higher associated probabilities, while on Popper's view scientists should prefer theories with higher information content. Therefore in contrast to Carnap's idea of degree of confirmation Popper advances the idea of "degree of corroboration", although in some contexts Popper also uses the phrase "degree of confirmation" in a sense that is synonymous with his idea of degree of corroboration. On the corroboration thesis a scientific theory that has greater information content (because it is more universal, or because it is more accurate than an alternative theory) also has a higher degree of corroboration, if when it is tested it is not falsified. Like the idea of information content, the idea of corroboration is based on the idea of falsifiability, but a theory would not be said to have been corroborated until it had been tested and found to have no falsifying test outcome; the degree of corroboration actually attained does not depend only on the degree of falsifiability. A statement may be falsifiable to a high degree yet it may be only slightly corroborated or it may be falsified.

The measures for corroboration, *C(h,e)*, and probability, *P(h,e)*, for hypothesis *h* and for basic statement *e* of evidence describing a test outcome, are related by certain equations. The inverse relation between the measures of corroboration and probability is related as follows:

$$C(h,e) = 1 - P(h,e)$$

and he is willing to admit a proposal by Kemery in *The Journal of Symbolic Logic* (1954) that the relation may also be expressed in terms of information science concepts as:

$$C(h,e) = 1 - \log P(h,e).$$

Popper states that the measure of the degree of corroboration, *C(h,e)*, may be interpreted as a measure of the rationality of belief in the statistical hypothesis, *h*, in the light of test outcomes, *e*, only if *e* consists of reports of the outcome of sincere attempts to refute the hypothesis by the severest test

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that can be devised, rather than attempts to verify *h*. But the degree of corroboration does not measure the degree of rationality in our belief in the truth of *h*, since $C(h,e) = 0$ whenever *h* is logically true. Rather, it is the measure of accepting tentatively a problematic guess. On the other hand the measure of explanatory power, *E(h,e)*, may be interpreted as the measure of the explanatory power of *h* with respect to *e*, even though *e* is not a report of any genuine and sincere attempts to refute *h*. The measure *E(h,e)* is a purely logical relation to the infinite class of potential falsifiers, and in an appendix to his *Logic of Scientific Discovery* (1959) Popper relates *E(h,e)* positively to *C(h,e)* as follows:

$$E(h,e) = C(h,e)/(1+ P(h)\ P(h,e)).$$

The concepts of relatively greater or lesser degrees of information content and falsifiability provide the basis for Popper's ideas on scientific progress, the growth of scientific knowledge, and the aim of science. He advances a "metascientific" criterion of progress, that enables the scientist and methodologist to know in advance of any empirical test, whether or not a new theory would be an improvement over existing theories, were the new theory able to pass crucial tests, in which its performance is compared to older existing alternatives. He calls this criterion the "potential satisfactoriness" of the theory, and it is measured by the degree or amount of information content. Simply stated, his thesis is that the theory that tells us more is preferable to one that tells us less, and the theory that tells us more is also one which is most falsifiable. From this it follows that the aim of science is high empirical information content as well as successful performance in tests. It is the criterion of high information content that makes the growth of science rational. The aim of science is not high probability, and the rationality of science does not consist of constructing deductive axiomatic systems, since there is little merit in formalizing a theory beyond the requirements for testing it. Nor does the growth of science consist of the accumulation of observations. Rather it consists of the repeated overthrow of scientific theories and their replacement by more satisfactory theories. The continued growth and progress of science is essential to the rational and empirical character of scientific knowledge. The growth is continuous, because criticism of theories, which are proposed solutions, in turn generates new problems. Scientific problems occur when expectations are disappointed. Science starts from problems, not from observations, although unexpected observations give rise to new problems.

Popper views science as progressing from old problems to new problems, to new problems having increased depth as it progresses from old theories to new theories having increased information content. He also views progress in science as approaching more and more closely to the truth, where truth is understood as a correspondence with the facts and as a regulative idea. Just as there are degrees of information content, so too there are degrees of approach to the truth that he calls "verisimilitude."

In his "Rationality of Scientific Revolutions" Popper therefore sets forth two criteria for the rationality of scientific revolutions, which are also two logical properties that enable the scientist to evaluate any new theory even before it is tested. The first criterion may be called a criterion of discontinuity: the new theory must conflict with the old one in the sense that it leads to conflicting results. Popper says that in this sense scientific progress is always revolutionary, and that the Marxian refrain "revolution in permanence" is applicable to science. The second criterion may be called a criterion of continuity: the new theory must be able to explain fully the success of its predecessor in the sense that either there are applications in which the old theory must appear to be a good approximation to the results of the new theory, or there are cases where the new theory yields different and better results than the old one. Scientific revolutions are rational because unlike ideological revolutions, which are sociological, the former cannot simply break with tradition.

Against Psychologism, Induction, and Naturalistic Semantics

Popper's philosophy of knowledge is a critique of psychologism and a defense of the objectivity of knowledge. In the opening chapter of *Logic of Scientific Discovery,* which is titled "A Survey of Some Fundamental Problems", he devotes a section to the elimination of psychologism. This section follows the opening section on the problem of induction, which he views as a fallacy resulting from the psychologistic philosophy of knowledge. He sets forth his own theory of knowledge in the fifth chapter titled "The Problem of the Empirical Basis" and the opening section is a critique of the psychologistic view that perceptual experiences are the empirical basis for science. In his "Demarcation Between Science and Metaphysics" (1955) in *Conjectures and Refutations* he criticizes Carnap's theory of meaninglessness, which he describes as a "naturalistic theory of meaningfulness" of linguistic expressions. The linguistic expressions of particular relevance are those singular statements that are used for describing

observations in science. All of these ideas are interrelated according to Popper: induction as the logic for making generalizations and hypotheses, psychologism which proposes perception as the empirical basis of observation in science, and the naturalistic theory of the semantics of language. Popper rejects all of them together. In his "Epistemology Without a Knowing Subject" (1967) and his "On the Theory of the Objective Mind" (1968) published as chapters three and four in his *Objective Knowledge* (1972), in Part I of *The Self and Its Brain* (1977), and also in an appendix to *The Open Universe* (1982), Popper sets forth his own philosophy of the three "worlds" of reality which locates subjective psychology and objective knowledge in different worlds.

The development of Popper's own philosophy of science began with the objective of demarcating empirical science and pseudoscience (e.g. Astrology, Marxism, Freudianism, and Adlerian psychology). His solution to the problem of demarcation is the criterion of empirical falsifiability, which he also uses to demarcate empirical science from metaphysics, and he contrasts this criterion with the criterion of meaningfulness that Carnap and other Logical Positivists used for distinguishing science from metaphysics. Carnap's criterion of meaningfulness is based on the naturalistic philosophy of language. Popper argues that the Positivists have never succeeded in distinguishing science from metaphysics or in distinguishing theory from observation, that metaphysics need not be meaningless even though it is not a science, and that Positivism excludes scientific theories as meaningless while failing to exclude metaphysics as meaningless. Popper maintains that there is no observation without theory, and that the observation terms occurring in observation language are "theory impregnated", such that observation terms are a type of theoretical term that Carnap calls disposition terms. The reason that the Positivists have not succeeded in distinguishing science from metaphysics, is that they cannot define meaningfulness, and they cannot define meaningfulness because they interpret the problem in a naturalistic way, as though it were a problem in natural science or in psychology. Popper maintains that the Positivists have confused the psychology of knowledge with the logic of knowledge, which is to say that they have adopted a psychologistic philosophy of knowledge. Popper rejects both behaviorism and psychologism, and maintains that the content of thought, the meanings of words, the semantics of language, are not determined either by the natural laws of the physical world or by the natural laws of psychology. The world of objective knowledge, which is governed by the laws of logic, is a third world that is autonomous from the world of

objective physical nature and also from the world of subjective psychology. In *The Self and Its Brain* he argues against behaviorism and physicalist reductionism by the display of ambiguous drawings that he emphasizes may be interpreted in different ways by voluntary action, in order to demonstrate the existence of world 2, the world of the mind and of subjective mental experiences. He argues against the psychologistic view by stating that the objects of world 3 are intersubjectively testable. Hence there are the three separate worlds which cannot be reduced to one another: world 1 is the world of objective physical nature, world 2 is the subjective world of psychological experience, and world 3 is the objective world of human artifacts or creations including knowledge. Popper emphasizes that while the three worlds interact through world 2, nevertheless the world of objective knowledge is autonomous of the world of subjective psychological experience including perceptual experiences. Advocates of psychologism and the naturalistic theory of the semantics of language fail to recognize the autonomy of world 3 from the other two worlds. More recently in his "The Foundations of Information Science: Philosophical Aspects" in *The Journal of Information Science* (1980) the information scientist Bertram C. Brookes proposed that the task of information science as a discipline can be defined as the exploration of the world of objective knowledge understood as Popper's world 3, and that this discipline is distinct from documentation and library science.

Popper's rejection of inductive logic is based on his thesis that world 3 is autonomous from worlds 1 and 2. He references Einstein's stated view that there is no logical path leading to the universal laws that scientists search for, and that these laws can only be reached by intuition. Popper accepts Hume's thesis that universal statements cannot be justified by the singular statements describing observations, and he rejects the early Wittgenstein's verifiability criterion of meaningfulness adopted by Carnap and the other Logical Positivists of the Vienna Circle. He also rejects the probabilistic inductive logic developed by Carnap and set forth in the latter's *The Logical Foundations of Probability*, and he wonders why anyone would ever write such a book. In Popper's view there is no logic of scientific discovery; there is only a psychology of scientific discovery. He explains that the title of his own book, *The Logic of Scientific Discovery* in not about the psychological processes involved in inventing new scientific theories, but rather is about the growth of scientific knowledge by conjectures and refutations, the proposal and criticism of new theories.

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Popper's philosophy of scientific knowledge is a sustained attack on Positivism, but it is not just a critical rejection; he has his own alternative philosophy of observation. The Positivists maintained that there is a clear distinction between theory and observation, such that one could separate the language of theory from the language of observation with each containing its own distinctive vocabulary and its own class of universal of statements. The universal statements containing only observation terms are produced by inductive generalization, while those containing theoretical terms are invented by the scientist's creative imagination. However, with the recognition that theory determines what is observed, the separation between theory language and observation language can no longer be sustained, and the ideas of theory and observation must be reconceptualized. And since the existence of an observation language was thought to be the empirical basis for science, the empirical basis for science also must be reconsidered.

The Positivists had attempted to base empirical science on "atomic statements", "protocol statements" and "judgments of perception" stated in the observation language. Popper rejects these ideas with his rejection of the naturalistic philosophy of meaning. Instead he proposes the idea of the "basic statement", which he defines as a singular statement which together with the universal statements of theory can serve as a premise in an empirical falsification of a theory. The basic statement is fundamentally different in concept from Carnap's protocol statement. The protocol statement is thought to be justified by perceptual experiences and thereby to constitute a foundation for science. But Popper maintains that this is a confusion between the subjective psychological aspect of knowledge and the objective logical aspect. Perceptual experiences are subjective and psychological; they can motivate a decision and hence an acceptance or a rejection of a statement, but a basic statement cannot be justified by them any more than it can be justified by thumping on a table. Basic statements are objective in the sense that they can be intersubjectively tested by repetition of the conditions that occasioned them. And they can be falsified, since they operate as premises from which other statements can be deduced, which in turn can be tested. As a result there can be no ultimate statements in science, as the Positivists believed; all statements in empirical science can be refuted by falsifying some of the conclusions that may be deduced from them.

But it is not necessary that a basic statement should be tested in order for it to be accepted; it is only necessary that the basic statement be testable. The function of basic statements is to test theories. Every test of a theory

must stop at some basic statement, which the scientists have agreed to accept at least for the present time. To the extent that the basic statements are accepted on the basis of agreement, they are conventional. But the agreement is not arbitrary or capricious; the decision is made by reference to a theory and the problem that the theory is proposed to address. Theory dominates experimental work from its initial planning to its completion in the laboratory. Popper summarizes his views on the empirical basis of science by means of a memorable metaphor: There is nothing absolute about science; it does not rest upon solid bedrock, as it were. The bold structure of its theories rises as it were above a swamp like a building erected on piles, which in turn are driven down to whatever depth is found to be satisfactory to carry the structure for the time being.

Popper's reconceptualization of the empirical basis of science is also a reconceptualization of the concept of theory in science. Unlike the Positivists, Popper does not define the concept of scientific theory in terms of theoretical terms. Instead he views theories as universal statements, and rejects any distinction between empirical laws and theories, since there is no longer any distinction between theory language and observation language based on a distinction between theoretical terms and observation terms. All the universal statements in science are conjectures that are testable and falsifiable, and these conjectures are invented by the human mind; none of them are produced by inductive generalization. To give a causal explanation of an event means to deduce a statement which describes the event using as premises of the deduction one or more universal laws as theories together with singular basic statements that describe the initial conditions. Popper's ideas for such terms as "theory", "law", and "cause" are fundamentally different from the Positivists' ideas for these terms, because Popper's ideas are separated from the subject matter or ontologies described by the sciences.

Empirical science is not purely formal like mathematics or logic, but neither is it defined in terms of certain substantive concepts about reality as it is described by science today. Future science may have revised the substantive content of today's science and yet science will still be science as Popper has defined it. As Popper says in reply to Kuhn's concept of science in "Normal Science and Its Dangers" in *Criticism and the Growth of Knowledge* (1970), science is "subjectless." Such could not be said of science by the Positivists, for whom the naturalistic philosophy of the semantics of language requires that certain substantive concepts permanently established by observation must always be retained as definitive of the

empirical character of science. The rejection of the naturalistic philosophy of the semantics of language implies the reconceptualization of such metascientific terms as "theory", "law", "explanation", and "cause" in a manner that disassociates these ideas from any particular ontology that the semantics of science may describe at any point in history. Empirical science becomes a sequence of alternative ontologies instead of a specific ontology. And with his criterion of increasing information content Popper believes that the sequence of ontologies is not a disconnected random sequence, but rather is one that reveals objective and rational scientific progress. Curiously Popper himself did not follow through on these ideas when he supported Einstein's criticism of the Copenhagen interpretation of quantum theory, and advanced his own "commonsense realism" ontology.

On Computers, Induction Machines, and Scientific Discovery

In his *Logical Foundations of Probability* and elsewhere Carnap proposed using a computer to make empirical generalizations with inductive logic. Throughout his career Popper has rejected the idea of inductive logic, but in *Realism and the Aim of Science* (1982) he admits to induction machines of a certain type. For such a machine he postulates a simple universe containing individuals and a limited number of properties that the individuals can have. This universe furthermore operates with a number of so-called "natural laws." Popper says that for this universe a machine can be created, such that in some reasonable period of time it will discover the laws that are valid in the postulated universe during the time period. If the laws of its universe are modified, the machine will show its capacity for finding a new set of laws. It would be capable of drawing up statistics about various distinguishable occurrences and of calculating averages. If the postulated universe is complicated further to include among its natural laws, the laws of succession, the general or conditional frequencies having a certain degree of stability, etc., then the machine can be enhanced to be able to formulate hypotheses, to test the hypotheses, and to eliminate those that should be eliminated. Such a machine can learn from experience.

But this inductive machine is limited to the universe that its architect has created for it. The architect of the universe decides what are to be individual events, and what constitutes a property or a relation. In general it is the architect of the machine who decides what the machine can recognize as a repetition. And even more fundamentally it is the architect of the

machine who decides what kinds of questions the machine is to answer. All these considerations mean that the more important and difficult problems are already solved by the human designer, when he constructs the machine and the universe it can recognize. Things that Positivists such as Carnap had thought to be simply given by nature, the meanings that according to the naturalistic theory of the semantics of language are delivered by the natural operation of human perception, are in Popper's view the product of the creative and imaginative powers of the human designer. These powers enjoy a freedom that is permitted by the artifactual character of objective knowledge, and that is necessary for the creation of the hypotheses and theories that have characterized the growth of knowledge by science. The basis of this freedom is the nondeterministic relation between world 3 on the one hand and worlds 1 and 2 on the other. Carnap had admitted that an induction machine cannot create hypotheses, and that theories are inventions created by the human mind. But Popper does not admit to the Positivists' separation between empirical generalizations on the one hand and theories on the other; he maintains that there is no observation without theory. He also argues that no human or computer can predict the future growth of scientific knowledge by scientific methods without committing the fallacy of historicism. In his *Poverty of Historicism* (1975) as well as in *Realism and the Aim of Science* he maintains that historicism involves unconditional predictions, and he says that such predictions are impossible, because prediction in science requires universal laws, which are always conditional.

As it happens, the computerized development of hypotheses and conjectures is precisely what information scientists attempt to accomplish by their artificial-intelligence computer systems, which Herbert Simon calls "discovery systems." These computer systems are instrumental to the scientist's development of hypotheses. They are not historicist, but are conditioned upon inputs that require the same kind of preparation or initial conditions that Popper says are needed for what he calls an "induction machine."

The Schism in Physics and Metaphysical Research Programmes

The term "schism" in the context of the philosophical discussions of the quantum theory did not originate with Popper; Heisenberg introduced it. In his "Recent Changes in the Foundations of Exact Sciences" (1934) in *Philosophical Problems of Quantum Mechanics* Heisenberg notes a "peculiar schism", that he says is inescapable in the investigation of atomic

processes. He is not referring to a sociological phenomenon in the physics profession or to an issue that must be resolved; he views the schism positively as a development in physics. As Heisenberg uses the term "schism", it refers to the different concepts used by classical physics and quantum physics and to the different ontologies they describe. On the one hand there is the need for macrophysical classical concepts of space and time, which are used in quantum physics for the description of experiments and of the apparatus of measurement in experiments. On the other hand there is the mathematical expression suitable for the representation of microphysical reality, the wave function in multidimensional configuration spaces, that allow of no easily comprehensible interpretation. Heisenberg says that the dividing line between the classical and the quantum physics is the statistical relation.

Popper's earlier views on quantum theory are set forth in his *Logic of Scientific Discovery* and his more mature statement is set forth in his *Postscript to the Logic of Scientific Discovery* (1982). The latter work is a collection of three volumes: *Realism and the Aim of Science, The Open Universe: An Argument for Indeterminism,* and *Quantum Theory and the Schism in Physics.* Popper brings to statistical quantum theory a prior ontological commitment, which he calls "commonsense realism." In Popper's view physics has historically developed out of one or another metaphysical view which he calls a "metaphysical research programme." A metaphysical research programme is a set of ideas that are currently untestable, and therefore are called "metaphysical." In Popper's philosophy the demarcation between science and metaphysics is testability thus giving metaphysics a residual status relative to science. The metaphysical research programme supplies the physicist both with a metaphysical view or ontology about the general structure of the world and with a metascientific view about such things as the criteria for a satisfactory scientific explanation based on the ontology contained in the metaphysical research programme. Science needs metaphysical research programmes, because they largely determine its problem situations. Popper cites Einstein's way of looking at the Lorentz transformation as an example of how a metaphysical research programme can supply a new way of looking at things, that may change science completely. Metaphysical research programmes change and are replaced as some parts become testable and are incorporated into science. The relation between the testable theory and the research programme is part of the history of problem situations of the science, along with the problems arising from inconsistency among theories and empirical falsifications of theories.

POPPER

Unlike Heisenberg, Popper views the schism in physics in more sociological terms and in terms of the issues that have given rise to the schism. And unlike Heisenberg, he does not view the current schism in physics favorably. In his opinion the acceptance of the Copenhagen interpretation and the rejection of what he calls the Faraday-Einstein-Schrödinger metaphysical research programme have left physics without any unifying picture of the world, without any theory of change, and without any general cosmology. The current schism in physics is a clash between two metaphysical research programmes, neither of which in his view seems to be doing its job. In *Quantum Theory and the Schism in Physics* he summarizes the current schism in terms of three issues: (1) indeterminism vs. determinism, (2) realism vs. instrumentalism, and (3) objectivism vs. subjectivism. All three issues are closely related to one another and to the interpretation of the probability function in the statistical quantum theory. The schism has its orthodox group, and it has a variety of dissenters. On the dissenting side of the schism he locates the views of Einstein, de Broglie, Schrödinger and Bohm, which he characterizes together as determinist, realist and subjectivist. On the orthodox side of the schism he locates the Copenhagen school including Bohr, Heisenberg, Pauli and Born, which he characterizes together as indeterminist, instrumentalist and objectivist. He does not consider Heisenberg's views to be realist, and he effectively lumps Heisenberg together with Bohr, who was explicitly instrumentalist in his view of the formalism of quantum theory. This amounts to a misrepresentation of Heisenberg. Popper proposes a new and unifying metaphysical research programme that he says offers a consistent ontology for both macrophysics and microphysics. Such an ontology has been the Holy Grail of nearly every critic of the Copenhagen school. In his autobiography he states that his views on quantum theory were greatly influenced by those of the physicist Alfred Lande, and he states in the *Postscript* that Lande anticipated his own interpretation of the quantum theory. Therefore, a brief examination of Lande's interpretation of the statistical quantum theory is in order before proceeding further in the discussion of Popper's particle-propensity interpretation.

Lande's New Foundations of Quantum Physics

A brief biography of Alfred Lande (1888-1975) can be found in an obituary published in *Physics Today* (May 1976). Lande was a German-born American physicist, who received a doctorate in physics in 1914 from

the University of Munich, where he studied under Sommerfeld. In 1918 he co-authored a paper with Born, that refuted Bohr's model of coplanar electronic orbits. In 1931 he immigrated to the United States, where he taught theoretical physics at Ohio State University until his retirement in 1960. Lande originally advocated the Copenhagen interpretation of quantum theory, but publicly disassociated himself from it with the publication of his *Foundations of Quantum Theory* (1955). His most mature statement of his views is his *New Foundations of Quantum Mechanics* (1965), which includes ideas published in his previous papers.

As a physicist Lande had his own agenda: the solution of what he calls "The Quantum Riddle", which is the derivation of the laws of quantum mechanics from a nonquantal and nondeterministic basis without the *ad hoc* assumptions that he finds in the Copenhagen interpretation. In his deductive explanation of quantum laws from three nonquantal postulates, he maintains that uncertainty is a physical principle for both classical and quantum physics, and he advances and defends a particle interpretation of both Heisenberg's uncertainty relations and Schrödinger's wave function. Both of these views were central to Popper's philosophy of science twenty years before Lande rejected the Copenhagen interpretation of quantum theory, and Lande references Popper's views in his own literary corpus. However, Lande maintains a contrary ontology with respect to the reality of the waves associated with the Schrödinger wave function.

In "Probability in Classical and Quantum Theory" in *Scientific Papers Presented to Max Born* (1953) Lande argues that classical thermodynamics cannot be reduced to deterministic mechanics, and that it is futile to search for hidden causes behind any distribution that satisfies the rules of probability either in classical or quantum physics. To illustrate his thesis he describes an experiment in which ivory balls are dropped through a tube onto the center of a steel blade, resulting in an observed 50:50 average ratio of balls falling to the left or right. On the determinist view the 50:50 ratio is possible only if it is already contained in the initial conditions, which in turn either implies an infinite regress to still prior conditions, or is left unexplained. Lande rejects both these options. Instead he concludes that random distribution is a physical reality, and that determinism is a purely academic construction, because a program of giving a deterministic theory of statistically distributed events leads nowhere. Statistical theory can only reduce one probability distribution to another, and when there are ensembles of events conforming to error theory, these events are not reducible to deterministic mechanics.

In *New Foundations* he states that the belief in determinism is as much beyond the domain of physics as the belief in indeterminism, because both ideas are metaphysical theses. Observation only shows that equal preparation, as far as equality can be achieved, always leads to unpredictably different results. Lande elevates this general insight to the physical principle of uncertainty. In contrast to ordinary experience, classical mechanics was deterministic, while on the other hand ordinary experience and quantum mechanics agree. Unpredictability understood as the acausality of individual events must be seen as an irreducible feature of natural science. Statistical mechanics can describe predictable averages for unpredictable individual events. In quantum mechanics it is Heisenberg's great merit that he established quantitative limits for the uncertainty of prediction, but Lande also states that unpredictability of future events does not preclude the reconstruction of past individual cases using a deterministic theory.

Lande rejects Heisenberg's thesis that between two observations in atomic physics the electron is nowhere. In his discussions of uncertainty and measurement in *New Foundations* he admits that while in classical physics a measurement value can be attributed to the object immediately before, during, and after the measurement, in quantum physics there is an active, unpredictable, and unavoidable participation of the instrument or "meter" in producing the result, in which the microphysical object is thrown from its previous state into a new state. Therefore in quantum physics the measured value can be ascribed to the atomic object only immediately after the measurement is completed, and any subsequent measurement erases all traces of the first state and produces an entirely new situation. Nevertheless Lande maintains that it is always possible to reconstruct one and only one path between the two space-time positions according to the laws of classical mechanics *post factum*, even though the path cannot be predicted. He distinguishes between direct and indirect measurements; the former are coincidences in space and time, and are the basis for all other measurements, which are indirect measurements. Energy, momentum, and velocity are relevant examples of indirect measurements; velocity by definition requires measuring two adjacent positions at two adjacent times.

Lande rejects the Copenhagen thesis that effectively equates "indirectly observed" with "not observed", and then with "not observable", and finally with "nonexistent" and "meaningless." The Copenhagen school wrongly maintains that only direct measures count as observation. To say as they do, that position and momentum cannot be measured simultaneously is only a half-truth. If one includes "directly", then it is trivial because

momentum can never be measured directly.   And without the word "directly" the statement is wrong, because the momentum value acquired within a given position increment can be determined by reconstruction of space-time data with the help of theory.   The root of the difficulty with reconstructing values of indirect observables is the ambiguity of their definition, which always requires theory.   Lande maintains that classical theory can be used to make the indirect measurements needed to describe the path of an electron.   The controversy about the meaning of an atomic measurement is due to an erroneous connecting of the first measurement with a set of possible future measurements. When the wave function is used as a mathematical representation of just one physical state, there is no confusion.   But when it is used to connect one measurement with a set of future possible measurements, misunderstanding occurs which results in different interpretations of the wave function, including the Copenhagen dualistic thesis that the wave function describes a physical state of matter which is spread out in space and time, and which suddenly contracts to one point when the particle is measured.

Lande rejects subjective interpretations, and states that quantum physics deals with records of instruments rather than any observer's consciousness, with physical objects rather than mental pictures, and with statistical distributions rather than lack of knowledge by human observers. Knowledge and conscious reading by observers are as irrelevant in atomic physics as they are in any other branch of physical science.   Echoing Einstein's programmatic aim of all physics (but without referencing Einstein), Lande says that the object of natural science is to suppose that the real world exists without human advice and consent, and then to search for general regularities which may help to manipulate things and events. The significance of all that quantum theory stands for, is to provide formulas, tables, and other rules of correlation between events, and in particular between probabilities of transition. To speak of the contraction of the wave packet upon an observation is as senseless in Lande's opinion as to speak of a sudden contraction of a statistical mortality table upon an individual fatality.   A probability wave does not guide actual events any more than a mortality table guides actual mortalities, and it shrinks no more than a mortality table shrinks when an actual death occurs. In Lande's view the subjectivist confusion begins when the material body used as a measuring instrument is regarded as a subject, and when it is then said that quantum theory has changed the relation between subject and object. This makes a great impression on those who mistakenly identify statistical distributions

recorded by instruments with knowledge or lack of knowledge of observing subjects.

Lande advances a particle interpretation of the Heisenberg uncertainty relations and the Schrödinger wave function, and he criticizes the Copenhagen dualistic interpretation. A central part of his criticism is his alternative interpretation of the two-slit diffraction experiment, in which the diffraction pattern is construed by the Copenhagen school as an interference pattern, that must be taken as evidence for the wave nature of the electron, which in turn must also be construed as a particle before its entry into the slit and then again upon its impact on the photographic plate. Lande references the Stern-Gerlach experiment, the theory of William Duane (1923), and the work of Paul Ehrenfest and Paul S. Epstein (1924). He explains that Duane's quantum theory was not immediately recognized as a way out of the Copenhagen duality paradox, because Duane's proposed statistical particle theory of diffraction pertains to X-rays in support of the photon theory of light, and also because in 1923 diffraction of electrons was not yet discovered. Lande references a letter written to him by Born stating that Duane's 1923 paper on the particle theory of X-ray diffraction was well appreciated at the time of its publication, and stating that it is a riddle as to why its significance was overlooked when the diffraction of matter was discovered a few years later. Lande remarks that he could not find any hint of recognition in any of the works of Bohr, Born, de Broglie, Dirac, Einstein, Heisenberg, Pauli, or Schrödinger, that Duane's quantum rule is relevant to the alleged dilemma of matter diffraction and duality.

According to Duane's quantum rule for linear momentum, the incident matter particles do not spread out as continuous matter waves or manifest themselves as though they do. It is the crystal slit with its parallel lattice planes, which is already spread out in space, and which reacts as one rigid mechanical body to the incident particles, that produces the diffraction pattern. Duane's rule yields the same observed diffraction directly without appealing to any wave interlude. Therefore, the idea of a dualistic change from matter particles to waves and then back to particles is a quite unnecessary and fantastic invention in Lande's opinion. According to his criteria for scientific criticism the scientific value of a theory is measured not only by its power to account for observed data, but also by criteria of simplicity, freedom from *ad hoc* assumptions, and reducibility to more general postulates. As a result of Duane's theory, quantum physics has discovered that even such wave-like phenomena as matter diffraction through crystals can be understood in a consistent unitary way as produced

exclusively by matter particles obeying the conservation laws of mechanics under special restrictions known as quantum rules, matter particles which react to bodies containing periodicies in time and space. Lande thus states that electrons always behave as particles, and never misbehave as waves; he calls Duane's quantum rule the "missing link" between wave-like appearances and particle reality. To the two recognized general quantum postulates, Planck's rule for energy exchange and Sommerfeld-Wilson's rule for angular momentum exchange, Lande adds Duane's quantum rule for linear impulse changes as the third postulate for quantum physics. Lande thus answers the problem of the two-slit diffraction experiment, the problem of which of the two slits did the particle pass through: he states that for its contribution to the diffraction pattern, it does not make any difference where exactly the diffraction takes place. The electron changes its momentum in reaction to the harmonic components of the matter distribution of the crystal screen with two slits as a whole. All that matters is the conservation of charge and of total momentum in the reaction between electron and diffractor.

For these reasons Lande maintains that the Copenhagen school starts from "wrong physics", when they maintain that wave-like appearances of matter diffraction are due to the periodic wave action of the electron. The correct view is that the appearances are due to the periodic structure of the bodies in space (the crystal) and in time (the oscillators) via the three corresponding quantum rules for the momentum and energy activity of the periodic bodies. He calls his particle interpretation "practical realism", and offers reinterpretations of Heisenberg's and Schrödinger's equations. The Heisenberg uncertainty relations describe objective statistical dispersion. Heisenberg's claim, that simultaneous exact position and momentum measurement pairs is meaningless and nonexistent, is incorrect because it confuses lack of predictability (which is true) with lack of measurability (which is false). Unpredictable data including position and momentum measurement pairs can be reconstructed which are more accurate than Planck's constant. And what can be measured, exists. The doctrine of the indeterminacy of existence is a "semantic artifice" rather than legitimate physics. Nor is denying that a particle always is somewhere, warranted by diffraction experiments, because each particle reacts to a space-extended periodic component in the matter distribution of the diffractor. To say that the particle is nowhere is a "linguistic extravaganza" and not a philosophical innovation.

As for Schrödinger's equation, Lande says that it does not deal with matter waves, but with probability amplitudes; it is a probability table not essentially different from any mortality table. The real constituents of matter are discrete particles, which occasionally give the appearance of wave action, and the real constituent of light is a continuous electromagnetic field, which sometimes gives the appearance of photonic particles. The Schrödinger wave function is a probability curve describing betting odds for future events; it is not a real thing even when the curve looks wave-like. Lande uses the phraseology of Dr. Samuel Johnson (a critic of Bishop Berkeley's *esse est percipi* philosophy, who kicked a great stone and exclaimed "I refute him thus") saying that you can kick a stone, and you can kick an electron and even a water wave and an electromagnetic wave, and be hurt by them, thus proving their reality. But you cannot kick or be hurt by a wave-like curve representing probabilities of events. For Lande, physical interaction is the only correct ontological criterion for physical reality. He also takes exception to Born, his former colleague, who had initially developed the statistical interpretation of the Schrödinger wave function as a probability amplitude for particles, but who later made what Lande calls "belated concessions" to the Copenhagen dualistic interpretation. He references Born's "Physical Reality" appearing in *Philosophical Quarterly* (1953) in which Born sets forth his own ontological criterion, the criterion of invariance. In this article Born is not explicitly opposing Lande, but rather is opposing the Idealist metaphysics and the Logical Positivist philosophy of phenomenalism.

Born explains his criterion of invariance as follows: Most measurements in physics are not concerned with things that interest us, but are concerned with some kind of projection which is defined in relation to a system of reference. In every physical theory there is a rule which connects the projections of the same object on different reference systems. The rule is called a law of transformation, and all transformations have the property of forming a "group", where the sequence of two consecutive transformations is a transformation of the same kind. Invariants are quantities having the same value for any system of reference, and therefore are independent of the transformations. The main advances in the conceptual structure of physics consist in the discovery that some quantity which was formerly regarded as the property of a thing, is in fact only the property of a projection. The historical development of the theory of gravitation from pre-Newtonian physics to relativity theory is one example. Another example is the development of quantum physics. An observation or measurement in

quantum physics does not refer to a natural phenomenon as such, but to its projection on a system of reference which is the whole apparatus used in the experiment. Using instruments the physicist can obtain certain restricted but well described information, which is independent of the observer and of his apparatus, namely the invariant features of a number of properly devised experiments. Bohr's complementarity principle means that the maximum knowledge of the quantum can only be obtained by a sufficient number of independent projections of the same physical entity. The final result of complementary experiments is a set of invariants characteristic of the entity, and these invariants are called "charge", "rest mass", "spin", etc. In every instance, when we are able to determine these quantities, we decide we are dealing with a definite particle. The words "photon", "electron", etc. signify definite invariants, that can be constructed by combining a number of observations.

Born maintains that the idea of invariance is the clue to a rational concept of reality, not only in physics but also in every aspect of the world. The power of the mind to neglect the differences of sense impressions and to be aware only of their invariant features is the most impressive fact of man's mental structure. He proposes translating the term "*gestalt*" not as "shape" or "form" but as "invariant." And he proposes speaking of invariants of perception instead of sense impressions as the elements of our mental world. In the closing paragraph of his article Born considers the reality of waves according to his ontological criterion of invariance. He says that we regard waves on a lake as real, though they are nothing material but are only a certain shape of the surface of the water. The justification for this view is that they can be characterized by certain invariant quantities like frequency and wavelength, or as a spectrum of these. Born says that the same thing holds for light waves, and he asks rhetorically why the physicist should withhold the epithet "real" even if the waves represent in quantum theory only a distribution of probability.

In his *New Foundations* Lande replies to Born's rhetorical question from the viewpoint of his own criterion of interaction: Particles are real while Schrödinger waves are not real, for the same reason that sick people are real things while the wave-like curve which symbolizes the probability distribution during a fluctuating epidemic is not a real thing. Lande says that a given formalism can always be interpreted in a variety of ways. At the conclusion of his *New Foundations* he gives seven alternative interpretations of the Schrödinger wave function including Schrödinger's, de Broglie's, Bohm's, Heisenberg's subjective interpretation, Heisenberg's objective

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interpretation together with Bohr's instrumentalist interpretation, and Lande's own interpretation. He does not include Popper's propensity interpretation. He states that this list is indicative of the present confusion regarding the wave function, and paraphrases Mao Tse Tung saying that while it may be good politics to let a hundred flowers bloom and let a hundred schools contend, it is not good enough for science. He asserts that only his interpretation stands up to realistic criticism in accordance with "monolithic" quantum mechanics, i.e. quantum theory with an ontology that is consistent with the rest of physics.

Popper's Particle-Propensity Interpretation of Quantum Theory

Popper explains the basis for the schism in physics as follows: On the one hand Einstein was a determinist, who believed that the statistical nature of quantum theory is due to the physicist's ignorance of the underlying deterministic laws, which have not yet been discovered. Therefore Einstein chose a subjective interpretation of probability based on the scientist's ignorance. On the other hand Heisenberg was an indeterminist, but because the only objective interpretation of probability available at the time was the frequency interpretation, Heisenberg's introduction of the observer's disturbance of the quantum phenomenon by the measurement apparatus resulted in the combination of both the objective and subjective interpretations of the probability function in the Copenhagen interpretation of the quantum theory. The frequency interpretation is applicable only to mass phenomena, while the quantum theory pertains to singular events. Therefore in order to describe the single quantum event, it seemed necessary to view probability as describing the scientist's ignorance resulting from the disturbance. For this reason according to Popper the Copenhagen interpretation also relies on the subjective interpretation of probability. Popper's propensity hypothesis advances an objective interpretation of the probability calculus and of probabilistic theories in physics, and it is an objective interpretation that is applicable to singular events. Popper has arguments for probability interpretations that are exclusively objective, but any objective interpretation requires a realistic philosophy with an indeterministic ontology. Therefore he also advances arguments for realism and indeterminacy, as well as for objectivism.

Popper has several arguments against the subjective interpretation of probability and for the objective interpretation. Some quantum theorists

such as Pauli introduce the idea of induction into discussions about the statistical nature of quantum theory. Popper rejects this application of inductivism for the same reasons that he rejects all applications of the idea of induction; induction is psychologistic and confuses world 2 with world 3. He also argues that the idea of explaining the statistical outcomes of experiments and predictions in terms of the ignorance of the physicist is absurd. Empirical science absolutely never explains anything in terms of the researcher's ignorance; it always explains phenomena in terms of other phenomena. While this argument of Popper's is true and may apply to some subjective interpretations of the quantum theory, it does not apply to interpretations such as Heisenberg's, which invoke the subjective interpretation of probability only to address the problem of measurement errors, thus giving the subjective interpretation a metalanguage status instead of the object-language status of an explanation in physics.

Popper's argument for realism is based on his falsificationist thesis of scientific criticism. Simply stated, he argues that the possibility of falsification is evidence of the existence of the real world that is independent of human knowledge. He furthermore argues that the fact that theories are conjectures does not imply that they do not describe the real world. Rational criticism results in better theories that have greater verisimilitude. Popper argues against instrumentalism, which he associates with both Bohr and Heisenberg. In "Three Views Concerning Human Understanding" in *Conjectures and Refutations* he references Heisenberg's thesis that physical theories such as Newton's are not falsified, but rather have had their applicability restricted by later theories such as relativity and quantum mechanics. This view is an aspect of Heisenberg's doctrine of closed-off theories, although Heisenberg did not set forth his doctrine of closed-off theories as an instrumentalist thesis. In a footnote in this paper Popper states that Heisenberg's instrumentalism is far from consistent, and that he has many anti-instrumentalist remarks to his credit, but that Heisenberg's view of quantum theory necessarily leads to an instrumentalist philosophy by neglecting falsification and stressing application. A mere instrument cannot be falsified, and the instrumentalist view may be used *ad hoc* to rescue a theory threatened by falsifications. Popper maintains that such an evasion was the reason that Bohr advanced his principle of complementarity, the renunciation of the attempt to interpret atomic theory as a description of anything; the self-consistent formalism need not be reconciled with its inconsistent applications, if it is left uninterpreted. On Popper's view the unfalsifiability thesis of the instrumentalist view makes instrumentalism

incapable of explaining scientific criticism and scientific progress. Only by reaching for refutations can science hope to learn and to advance.

Popper argues against determinism, and in this respect he takes exception to Einstein, although he says that he may have changed Einstein's mind about determinism in a conversation at Princeton in 1950. Popper distinguishes between metaphysical determinism, which is a thesis about the whole world, and scientific determinism, which is a thesis about the part of the world described by a scientific theory. He classifies Einstein as a metaphysical determinist, and reports that in his discussions with Einstein he referred to him by the name Parmenidies, because like the ancient philosopher Parmenidies, Einstein's metaphysical determinism implies that the future is entirely contained in the past, and that change is not real but is merely an appearance. Popper also argues against scientific determinism, and specifically he denies that Newtonian mechanics implies a deterministic ontology. He describes the theories of classical physics as *prima facie* deterministic, by which he means that the deterministic character is a property of the theory and not of the real world. He maintains that classical physics does not imply determinism any more than quantum physics does, because there is always an irreducible and stable statistical element in any predictions made with a *prima facie* deterministic theory; and it is always necessary to add to the deterministic theory a probability assumption to explain the statistical component in the prediction, because statistical conclusions require statistical premises. Popper quotes at length Lande's description of the experiment with the ivory balls and steel blade, which Lande uses to argue that statistical results require statistical assumptions about the initial conditions. Therefore Popper rejects attempts to explain the statistical outcomes subjectively by reference to lack of knowledge of the experimenter for the reasons given above, and he maintains that the law-like behavior of statistical sequences is for the determinist ultimately inexplicable.

Popper developed his propensity interpretation of probability in 1950 specifically to address the interpretation problem arising from statistical quantum theory, but it is also intended to be applicable to all physics. While it is but one of many interpretations for the probability calculus, it is the best for physics in Popper's view. Popper distinguishes three objective interpretations of the probability calculus: the classical interpretation, the frequency interpretation, and his propensity interpretation. The classical interpretation is that the probability measure $P(\alpha, \beta)$ is the proportion of equally possible cases compatible with the event β, that are also favorable to

the event α. The frequency interpretation is that P(α,ß) is the relative frequency of the events α among the events ß. The propensity interpretation is a refinement of the classical interpretation. In the classical interpretation experimentation is not needed, because it deals with equally possible cases, such as the two sides of a coin or the six faces of a die.

The propensity interpretation substitutes weights for equally possible cases, where the weights are experimentally determined measures of the propensity or tendency of a possibility to realize itself upon repetition. Thus in the propensity interpretation the measure P(α,ß) is the propensity of α given experimental conditions ß. It is the sum of the weights of the possible cases that satisfy the condition ß which are also favorable to α, divided by the sum of the weights of the possible cases that satisfy ß. The propensity interpretation is closely related to the frequency interpretation; the latter is about frequencies in actual finite sequences of experiments, while the former is about virtual finite sequences. In the propensity interpretation probability statements are about some measure of a physical property of the whole repeatable experimental arrangement, a measure of a virtual frequency, and the probability distribution is taken to be a property of the single experiment. The fact that the probability distribution in the propensity interpretation is a property of a single experiment is the strategic characteristic of this interpretation for quantum theory. Previously in *Logic of Scientific Discovery* Popper had attempted to modify the frequency interpretation so that it could address single events by means of what he called "formally singular statements." He abandoned this idea, when he developed the propensity interpretation. Now he says that the frequency measurements function to test the conjectured virtual frequency, which is a conjecture like any other scientific hypothesis.

The propensity interpretation is consistent with Popper's particle interpretation of the quantum theory, that he had advanced years before in *Logic of Scientific Discovery*. According to Popper`s particle interpretation the Heisenberg uncertainty relations are statistical scatter relations that describe the lower limits of the dispersion of particles; they are not the upper limits of the accuracy of measurements, as Heisenberg maintains. The uncertainty relations apply only to the magnitudes that belong to the particle after the disturbing measurement has been made. The particle always has position and momentum, and both position and momentum up to the instant of measurement can be ascertained in principle with unlimited accuracy. It is not the impossibility of precise measurement, but the statistical scatter that makes it impossible to predict the path of the particle after the disturbing

measurement operation. The scatter relations are statistical predictions about paths, and the paths must be measurable in order to test the statistical theory. For these reasons Popper rejects Heisenberg's view, as Popper sees it, that the uncertainty relations express limits to our subjective knowledge instead of expressing objective statistical scatter relations, and that measurements are impossible due to the nonexistence of the entities measured. What is impossible is producing scatter-free, dispersion-free quantum states. The statistical laws add to our knowledge. They do not set limits to our knowledge; they set limits to the scatter relations and tell us that the scatter is an objective reality that cannot be suppressed.

The propensity interpretation solves the problem of the relationship between particles and their statistics, and between particles and waves. Popper calls the Copenhagen wave-particle dualistic interpretation the "great quantum muddle." The great quantum muddle results from the mistake of taking the probability distribution function as a physical property of the elements of the population. Popper believes that this mistake is historically due to the fact that the works of de Broglie and Schrödinger led physicists to view the wave as the structure of the particle, and thus to view the particle as a "wave packet" or a "wavicle." Popper maintains that the statistical wave function is a property characterizing a sample space and not a property of the elements of the sample space. The elements have the properties of a particle. The propensity interpretation achieves the application of probability theory to single cases, but it does not do this by speaking about single electrons or protons; it speaks about propensities, which are properties of each instance of the whole repeatable experimental situation involving a single particle.

Propensity statements in physics describe properties of the situation, and are testable if the situation is typical.   Popper accepts Lande's explanation of the two-slit experiment, and he references what he calls the Duane-Lande space periodicity formula. The two-slit experiment is a space periodicity experiment, in which the particle interacts with the whole experimental situation including the crystal. More specifically from the viewpoint of the propensity interpretation, it is the whole experimental arrangement that determines the propensities. The possible results of any one experiment are different in the case of both slits being open from the case where only one is open; propensities are dependent on possibilities, such that the results will differ with different experimental arrangements, one slit or two. Thus in the two-slit arrangement the particle will pass through only one of the slits and in a sense will remain unaffected by the other slit. What the other slit influences are the propensities of the particle

relative to the entire experimental arrangement and not relative to the particle itself: the propensities for reaching the one point or the other point on the screen with the two slits. The Schrödinger wave equation enables the physicist to determine the propensities, and it entails the Heisenberg scatter relations, which limit the possible predictions.

Popper states that Schrödinger had anticipated one of the most important aspects of the propensity interpretation, namely the objectivity and reality of the waves in configuration space. One of the features of Popper's propensity interpretation is his thesis that the propensities are real, just as forces are real, and he speaks of propensity fields, just as contemporary physicists are accustomed to speak of force fields. The propensities are dispositional relational properties of the experimental set up. The waves are propensities of the particles to take up certain states under the conditions of the experimental set up, and the propensity waves are therefore no less real than electromagnetic waves. Lande believes that if he admitted to the reality of the Schrödinger wave, then like Born he would have to make what he called "concessions" to the Copenhagen dualistic thesis. Therefore Lande maintains that the Schrödinger wave function interpreted as a probability wave is merely a statistical function that is no more real than a mortality table, which Lande did not view as real. But Popper uses Lande's criterion of interaction, and argues that because the probability waves can interact to produce interference, they must be real, and are not merely mathematical tables. Popper supports Lande's rejection of the Copenhagen dualism, but contrary to Lande, Popper says that he prefers to speak of the particle and its associated propensity fields, instead of speaking of the particle and its associated mathematical probability function.

In the 1982 introduction to *Quantum Theory and The Schism in Physics* Popper proposes a crucial experiment for deciding between the Copenhagen interpretation and his propensity interpretation of the uncertainty relations. At issue is the subjective interpretation in the Copenhagen view: whether knowledge alone is sufficient to create uncertainty, or whether it is the physical situation that is responsible for the statistical scatter. The experimental situation involves two slits in two opposing screens, through which electrons pass, one electron through each screen. The sizes of the two slits are greatly different, while the degree of inaccuracy in the knowledge of the size of a slit is presumably small relative to the difference in the slit sizes. Popper expects different statistical scatters from each of the slits, while the accuracy with which the size of the slits is known, and is the same for each slit.

On Crucial Experiments and Scientific Revolutions

Unlike the Positivist philosophy of science, which has been interred to its resting place in the history of philosophy, Popper's philosophy of science is still a living philosophy in the sense that it is still accepted and debated in the professional literature. Popper has addressed more than one generation of philosophers during his lifetime. Initially his philosophy was a critique of the Positivists, who viewed his philosophy as an unconventional novation, while today his philosophy is criticized by the contemporary Pragmatists, who view his philosophy as the conventional wisdom. The central issue in which Popper represents the conservative position is the problem of the decidability of scientific criticism including most notably the decidability of crucial experiments. The origin of the problem is the thesis shared by both Popper and the Pragmatists, and also enunciated by Einstein, that theory determines what is observed. To the Pragmatists this thesis implies that the description of the observed results from an experimental test cannot be understood in the same way by different scientists who maintain alternative theories in an experimental test that is crucial in the sense that it purportedly decides between the alternative theories. If theory determines what is observed, then scientists maintaining different theories do not observe the same thing, and the observed outcome from the crucial experiment cannot decide between the alternative theories. To Popper on the other hand, Eddington's 1919 eclipse experiment, which is widely regarded as the historic crucial experiment deciding on behalf of Einstein's theory of relativity, demonstrates conclusively that crucial experiments are decisive.

It should be noted at the outset that even in his earliest writings Popper maintained that falsification is never finally and permanently conclusive, because the singular basic statements that are potential falsifiers may be revised, thus occasioning the revision of a falsifying test outcome. The empirical test may be said to be conclusive only to the extent that interested scientists agree to accept certain basic statements. Popper states that in some cases it has taken scientists a long time before a falsification is accepted, and that it is usually not accepted until a falsified theory is replaced by the proposal of a new and more adequate theory. But Popper does not find this historical fact to be problematic, even though in his view it is responsible for having led the Pragmatists to accept irrationalism and relativism in philosophy of science. In his introduction to *Realism and the Aim of Science* he gives several examples of successful falsifications, that furthermore have led to important scientific revolutions.

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If the development of Einstein's relativity theory can be said to be the formative influence in Popper's philosophy of science, then the development of the quantum theory can be said to be the formative influence in the contemporary Pragmatist philosophy of science. The topic of crucial experiments has assumed its controversial status in the professional literature due to the Copenhagen interpretation of quantum theory. The Copenhagen interpretation denies that a crucial experiment can decide between the wave and particle interpretations of microphysics, because the electron has the properties of both wave and particle. Quine invoked Duhem's philosophy of physical theory not only due to Duhem rejection of the decidability of crucial experiments, but also more fundamentally due to Duhem's thesis of the organic character of the semantics of theory language in physics. In his "Two Dogmas of Empiricism" Quine extended Duhem's thesis of the organic or wholistic character of the semantics of physical theory, to make it a general theory of the semantics of language as such, including the language used by physicists to describe observed experimental test outcomes. As a result of this extended thesis, which is now conventionally called the Duhem-Quine thesis, the wholistic character of the semantics of language explains why crucial experiments are undecidable not only in the wave-particle issue in quantum theory, but also more generally for all scientific criticism. Even where one of the alternatives is the Copenhagen dualistic interpretation, as in Lande's list of seven interpretations, the crucial experiment cannot effectively decide among them, according to the contemporary Pragmatist philosophy. The issue of crucial experiments has become a focal point in philosophy of science for the larger issues of the decidability of scientific criticism and of the nature of the semantics of language in general. The historic transition from the Positivists' naturalistic philosophy of the semantics of language to the contemporary artifactual philosophy of the semantics of language has thus resulted in two alternative artifactual philosophies of the semantics of language: The one is the organic or wholistic thesis advocated by the Pragmatists, which they use to attack the decidability of crucial experiments and of scientific criticism in general. The other is the logical or mechanistic thesis advocated by Popper, which he uses to defend the decidability of crucial experiments and the rationality of scientific criticism in general.

In his "Three Views Concerning Human Knowledge" (1956) reprinted in his *Conjectures and Refutations* Popper discusses Duhem's views on crucial experiments. He notes that Duhem shows that crucial experiments cannot establish a theory by refuting its alternatives, and emphasizes that

Duhem does not say that theories cannot be refuted in crucial experiments. Popper maintains that crucial experiments can be used to decide between alternative theories, as occurs when a new theory in proposed as a superior alternative to an older theory. The new theory is tested by applying it to cases for which it yields results that are different from what is expected from the older theory. He says that such cases are "crucial" in the Baconian sense that they indicate the crossroads between two or more theories, but not in the Baconian sense that any theory can be established. Popper then turns to Duhem's thesis that in every test it is not only the theory under investigation that is tested, but also the whole system of assumptions made by the theory, such that it is never possible to be certain which of the assumptions is refuted by the test. Popper states that if the scientists consider each of the two theories in the crucial test together with all the background knowledge assumed by both theories, then the scientists decide between the two systems, which differ only over the two alternative theories in the test. Popper adds that scientists do not assert the refutation only of one of the theories by the test, but rather the theory together with the background assumptions. By this he does not mean that every statement in the theory and its assumed background is refuted, but only that there is at least one statement that is erroneous, and that it may be in either the theory or the assumed common background. Thus he also says that in future tests parts of the background knowledge may be rejected as responsible for the falsification of the theory in the current crucial test.

Popper then proposes to "characterize the theory under investigation" in the crucial test precisely as that part of the vast system of knowledge for which the scientist has an alternative in mind, and for which he has therefore designed the crucial test. This may be taken as Popper's basis for individuating theories: theory α is distinguished from theory ß, because α makes a claim or statement than is an alternative to that made by ß, and because α consists in the language that makes it an alternative to ß. Thus it may be said that Popper individuates theories by reference to the theories' semantical properties as manifested in the crucial test situation. However, Popper does not define theory language by reference to the crucial test situation as such; he often states that the background knowledge includes theories other than the tested one. In his philosophy, therefore, theory language is any testable general statement regardless of whether or not it is being tested, which is to say that he defines theory by reference to its syntactical property of universal quantification and not by reference to its pragmatic properties. Furthermore Popper's concept of theory language may

be contrasted with that of the Positivists, who believed that it is possible to define theory in terms of its semantical properties by means of their distinction between theoretical and observation terms; Popper rejects this distinction.

In his "Truth, Rationality, and the Growth of Knowledge" (1961) reprinted in *Conjectures and Refutations* Popper turns to Quine's use of Duhem's philosophy. Quine maintains a wholistic view of empirical testing, and in his "Two Dogmas of Empiricism" in *From A Logical Point of View* he states that our statements about the external world face the tribunal of experience not individually but as a corporate body. Popper replies that this wholistic view of tests, even if it were true, would not create a serious problem for the falsificationist philosopher of science. He repeats his thesis that the fact that scientists take a vast amount of background knowledge for granted, is not to say that the scientist must uncritically accept it; the background knowledge too may be challenged and tested. Even though all of the background assumptions may be challenged, it is quite impossible to challenge all of the assumptions at the same time. All criticism must be "piecemeal", which Popper says is only another way of saying that the fundamental maxim of every critical discussion is that one should "stick to the problem", because the misguided attempt to question all background assumptions merely leads to a breakdown of critical debate. Critics such as Feyerabend will view this thesis as the Achilles heel of Popper's philosophy of science, its parallel postulate to be replaced with the new Pragmatist philosophy of language.

Furthermore even though the falsification of a theory does not reveal where the error is, nevertheless it is still possible to find the hypothesis that is responsible for the refutation, i.e. to find which hypothesis is responsible for the refuted prediction. The fact that such logical dependencies may be discovered is established by the existence of independence proofs for axiomatized systems; these are proofs that show that certain axioms of a system cannot be derived from the rest. Popper argues that the existence of such proofs shows that Quine's wholistic view of the global character of all empirical tests is untenable, and that it explains why even without axiomatized physical theories, the scientist may still have an inkling of what has gone wrong with the theory. In *Realism and the Aim of Science* Popper affirms as historical fact, that scientists are sometimes highly successful in attributing to a single hypothesis the responsibility for the falsification of a complex theory or of a system of theories, and he argues that this success remains to be explained if one adopts the wholistic view of empirical testing.

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In 1962 Thomas Kuhn wrote *Structure of Scientific Revolutions* in which he used the wholistic thesis to interpret the history of science. And in 1970 he defended his wholistic interpretation against critics in *Criticism and the Growth of Knowledge*. The leading critic in this later book was Popper, who contributed "Normal Science and its Dangers." In his earlier statements in defense of the decidability of crucial tests Popper did not explicitly address the basis of the wholistic view of testing, namely the thesis that the semantics of language is wholistic. The wholistic thesis of the semantics of language means that the meanings of terms are mutually determined in the context of the discourse in which they occur, such that alternative contexts consisting of alternative theories produce a semantic ambiguity or equivocation that is propagated through all of the related language. Therefore when considering the alternative theories investigated in a crucial test, all that constitutes the background assumptions is ambiguous. In other words there is really no common background, because one semantical interpretation is given to the language expressing the background assumptions by one of the theories in the test and another interpretation is given by the other theory in the test. Often the two alternative semantical interpretations are spoken of as two different languages, and there is said to arise a problem of translation from one to the other. This thesis is strategic to Kuhn's critique of the Positivists, because the lack of any common semantics for alternative theories that makes impossible a common background for crucial tests, also makes impossible a common observation language.

Kuhn maintains that the kind of scientific progress that Popper describes with its crucial experiments and falsifications can occur only within a linguistic framework, and he calls this type of scientific progress "normal science", which Kuhn opposes to another type which he calls "extraordinary science" or "revolutionary science." Revolutionary science is a transition from one language framework to another, where the term "framework" in the discussion refers to discourse having a univocal semantical interpretation and associated ontology. Popper rejects this theory of scientific revolution as irrational, when he criticizes Kuhn in *Criticism and the Growth of Knowledge*. While admitting that "normal science" in Kuhn's sense does exist, Popper argues that such normal science is dogmatic. He says that science is essentially critical, that it consists of bold conjectures controlled by criticism, and that it may be called revolutionary in this rational sense. He rejects Kuhn's relativism, the thesis that the linguistic framework cannot be critically discussed, and he calls this "the myth of the

framework." Comparison of different frameworks is always possible on Popper's view, and so is critical discussion therefore. Even totally different languages are not untranslatable. And it would be simply false to say that the transition from Newton's theory of gravitation to Einstein's theory is an irrational leap, and that the two are not rationally comparable; the transition to Einstein's theory was genuine progress in comparison with Newton's. Popper concludes that the myth of the framework is in our time the central bulwark of irrationalism, and that it exaggerates a difficulty with communication and criticism into an impossibility. In place of criticism as is found in Popper's falsification thesis, Kuhn proposes turning for enlightenment concerning the aim of science to psychology and sociology. But Popper rejects this proposal, and states that compared with physics, sociology and psychology are riddled with fashion and with uncontrolled dogmatism. He believes that such a proposal is a backward regression that cannot solve the difficulty.

In his "The Rationality of Scientific Revolutions" in *Problems of Scientific Revolution* Popper distinguishes between the sociological and the logical or rational dimensions in the history of science, when he distinguishes ideological from scientific revolutions. By an ideology he means any nonscientific theory, creed, or view of the world that is attractive or interesting to people including scientists. He cites the Copernican and Darwinian revolutions as examples of scientific revolutions that gave rise to ideological revolutions, because each changed man's view of his place in the universe. But these were also scientific revolutions in so far as each overthrew a dominant scientific theory, the one a dominant astronomical theory, the other a dominant biological theory. He also cites Einstein's relativity theory as a revolution, a truly scientific revolution, that gave rise to operationalism and supported Positivism, even though Einstein later rejected these ideologies. And Popper also refers to the subjectivist interpretation of quantum theory as an ideology, although in 1982 he proposed a crucial experiment that he thought could decide against it.

The wholistic thesis of the semantics of language is used by many Pragmatists to explain events that have been observed in the history of science: the impediment that language creates both to the development of new theories and to the communication of new theories within a profession. However, Popper relegates all semantical analysis to the status of a variation on the essentialist metaphysical thesis; in his autobiography in *Philosophy of Karl Popper* he admonishes the reader never to let himself be "goaded" into taking seriously problems about words and their meanings. He maintains

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that words "merely" play a technical or "pragmatic" role in the formulation of theories, just as the letters in written words play such a role in the formation of the words. Contemporary Pragmatists do not believe that language has so passive a role in concept formation and human cognitive processes, as Popper believes. And it may be noted that contemporary Pragmatists are as anti-essentialist as Popper; one need only recall Quine's rhetorical ridicule that an essence is merely a meaning wedded to a word. Popper's philosophy does not offer a theory of semantical description to reconcile the phenomenon of semantical change with his views on the decidability of criticism.

The Philosophy of Science

Popper`s philosophy comprehensively addresses the four basic topics of philosophy of science. His explicit rejection of the Positivist and essentialist naturalistic philosophies of the semantics of language represents a basic problem shift, a reconceptualization of science as viewed by philosophy of science.

Criticism

The central feature of Popper's philosophy of science is his falsificationist criterion, and its consequent rejection of the naturalistic thesis of the semantics of language and redefinition of the concept of theory. Theories are conjectures that are created by the human imagination, and similarly the meanings associated with the theories' constituent terms must also be created artifacts distinguished as world 3 objects. The theories do not originate by any natural process such as induction, and similarly the constituent meanings are not determined by any natural process such as perception. The theories are not permanently established by verification or confirmation, and similarly the meanings are not permanently established by virtue of any foundational ontology. Theories are routinely falsified as a part of the progress of science. The paradigmatic case for Popper is the transition from Newton's mechanics to Einstein's relativity theory. Einstein's theory does not include Newton's as a special case, but rather contradicts and corrects Newton's theory, and therefore describes an alternative ontology. And in such cases the new theory offers a higher degree of information content as indicated by the relative sizes of the classes of potential falsifiers,

such that even before empirical tests are attempted, it is possible to recognize that the new theory is preferable if it survives the test.

Crucial experiments are methodologically and historically important decision procedures in the progress of science. In the case of the transition from Newton's theory to Einstein's theory a crucial experiment was performed in 1919, in which Einstein's theory made the more accurate prediction within the range in which the deviation between the two theories was experimentally distinguishable. Crucial experiments are not only effective for deciding between theories, but are characteristic of the growth of science toward greater information content and verisimilitude. Popper rejects the wholistic variation on the artifactual theory of meaning, because it implies that crucial experiments are invalid because the alternative theories cannot share common background assumptions with univocal semantics, and because it implies in general that scientific criticism is undecidable. Both in his 1982 introduction to *Realism and the Aim of Science* and as early as his *Logic of Scientific Discovery* in 1934, Popper has maintained that the falsifying basic statements like all empirical statements cannot be verified, and that therefore it is impossible to prove conclusively that an empirical scientific theory is false. He also states that every falsification can be tested again for motivating an agreement among interested scientists about the test outcome. He maintains that there have historically been successful scientific revolutions, which were occasioned by successful falsifications, and he rejects the view that falsification plays no role in the history of science. But he offers no theory of meaning description that would enable him to reconcile the phenomenon of semantical change with his thesis of crucial experiments and the rational growth of science. Contrary to Kuhn, Popper maintains that communication problems are merely difficulties and not impossibilities. But without a metatheory of semantical description for analyzing semantical change, Popper cannot explain why communication is not impossible, because he cannot explain why it is merely difficult.

Explanation

Popper's theory of scientific explanation has been called the hypothetico-deductive thesis. In his chapter on theories in *Logic of Scientific Discovery* he states that to give a causal explanation of an event means to deduce a statement that describes the event, using as premises of the deduction one or more universal laws together with certain singular statements called initial conditions. Later in "Aim of Science" in *Ratio*

(1957), reprinted both in *Objective Knowledge* and in *Realism and the Aim of Science*, he defines a causal explanation as a set of statements by which one describes a state of affairs to be explained, statements which he calls the "*explicandum*", by deduction from a set of explanatory statements, which he calls the "*explicans*." The *explicans* must logically entail the *explicandum*, and it must not be known to be false. Furthermore, the *explicans* must be independently testable, so that it is not *ad hoc*. This means that the *explicandum* must not be the only evidence relevant to the *explicans*; the *explicandum* must have a variety of testable consequences, and especially consequences that are different from the *explicandum*.

The Logical Positivist concept of explanation is also described as hypothetico-deductive in the above sense. But there are also fundamental differences between Popper's and the Logical Positivists' views. Of central importance to Popper's concept of scientific explanation is the thesis that causal explanation need not describe certain things, or in other words that it need not have a certain semantics describing a certain ontology needed to supply science with foundations, such as the phenomenolist ontology. Popper's view therefore differs from the Positivist view that causal explanation must have a semantics with such ontological categories as sensations, elementary phenomena, or sense data. And it also differs from the Romantic view of causal explanation in social science, which requires a mentalistic ontology. In *Poverty of Historicism* Popper rejects the Romantic requirement of intuitive understanding of purpose and meaning produced by sympathetic imagination. In its *verstehen* version this mentalistic ontological requirement for causal explanation in social science becomes a theory of scientific criticism. He maintains that this requirement goes beyond causal explanation, and he proposes his doctrine of the unity of method in both natural and social science, the method that he describes in *Logic of Scientific Discovery*. In Popper's philosophy of science "causal explanation" is defined in terms of the function that theories perform in realizing the aim of science, and not in terms of some foundational ontology. His view of causal explanation is the result of his rejection of the naturalistic philosophy of meaning. Without the naturalistic theory of semantics there is no basis for requiring any particular ontology including the particular ontology's concept of causality, in order to be able to give a causal explanation. Rejection of the naturalistic thesis implies the rejection of all ontological criteria for causal explanation as well as the rejection of the distinction between observation language and theory language and of the idea of the existence of an ontological foundation for science. Thus Popper

says that explanation is of the known by the unknown in the sense of conjectural, instead of by the known in the sense of the permanently established foundation. In this respect Popper is in the company of the contemporary Pragmatists; Quine for example calls the view that there are ontological criteria for causal explanation the "genetic fallacy."

Popper's rejection of ontological criteria for causal explanation became complicated in later years by his idea of metaphysical research programmes. The metaphysical research programme is not atemporal and eternal like the ontological foundations of the essentialists and of the Positivists. It is part of the historical problem situation at a particular juncture in the history of a science, and it is also untestable at the point in time, and therefore "metaphysical" in Popper's sense. Most notably in Popper's view, at the given point in the history of the science the metaphysical research program functions as an ontological criterion for what constitutes a satisfactory explanation. This complication arises from Popper's way of demarcating between science and metaphysics, which appeared many years before he introduced the idea of metaphysical research programmes into his philosophy of science, as he did in his later discussions of quantum theory. As early as 1955 in "Demarcation Between Science and Metaphysics" in *Conjectures and Refutations* he states that all physical theories say much more than the physicist can test, and that whether this "more" belongs to physics or should be eliminated as a metaphysical element is not easy to say. And in 1958 in "On the Status of Science and of Metaphysics" reprinted in the same book he says that one can discuss irrefutable metaphysical theories rationally in the sense that one can discuss their ability to solve the problems that they purport to solve, that is, in relation to their problem situation.

This complication has its origin in the residual status of metaphysics in Popper's philosophy. Metaphysics for him contains a great heterogeneity of types of knowledge, which need have nothing in common, but their irrefutable character and therefore their nonscientific status. Historically philosophers have not treated metaphysics in so residual a manner, but instead have offered positive characterizations of metaphysics, which have sometimes been called "transcendental metaphysics", and which are not typically viewed merely as protoscience. For example issues such as realism versus idealism are viewed as transcendental and as incapable of empirical resolution at any time. In the concluding paragraph of the concluding section of the concluding volume of the *Postscript*, Popper states that there may be a criterion of demarcation within metaphysics between what he calls

"rationally worthless" metaphysical systems on the one hand, and metaphysical systems that are worthy of discussion and thought on the other hand. He does not characterize the basis for such a demarcation within metaphysics, but his motivation for recognizing the existence of protoscientific metaphysics within residual metaphysics seems clearly to have been the result of the influence of Kuhn. In the 1982 "Introductory Comments" in *Quantum Theory and the Schism in Physics* Popper compares metaphysical research programmes to Kuhn's concept of paradigm, while stressing that metaphysical research programmes must be seen in terms of a situation that can be rationally reconsidered, and that scientific revolutions viewed as changes of paradigms are due to rational criticism. In this context he references his 1975 "Rationality of Scientific Revolutions", where he distinguishes between scientific and ideological revolutions, and then sets forth criteria for rational criticism of scientific revolutions like Einstein's even before any experimental testing is attempted.

Aim of Science

Popper's concept of scientific explanation and the rejection of the naturalistic theory of meaning implied by the falsificationist thesis of scientific criticism, in turn imply a new concept of the aim of science, which is very different from the views of the Positivists. In his philosophical development two different types of statements of the aim of science may be distinguished, firstly the logical statement and then the later institutional statement. As early as 1934 in his discussion of the degrees of testability in the *Logic of Scientific Discovery* he states that theoretical science aims to obtain theories that are easily falsifiable, because the theories have a large information content, a large class of potential falsifiers. This concept of the aim of science is integral to Popper's view of the growth of scientific knowledge based on the idea of increasing empirical information content resulting from increasing falsifiability. Similarly in "Truth, Rationality, and the Growth of Knowledge" he states that the task of science is a search for interesting truth in the sense of truth that has a high degree of explanatory power or empirical information content. In his later statements Popper added to these ideas of the aim of science the role of the historical problem situation with his idea of the metaphysical research programme. In the introductory chapter to *Realism and the Aim of Science* he describes science as a social institution, that results from human actions that are unforeseen and unintended. He states that science grows through the institutionalized

cooperation and competition of scientists, who are not only motivated by their own subjective curiosity but also by their wish or aim to make a contribution to the growth of objective knowledge. The phrases "social" and "unforeseen and unintended" seem to refer to Popper's views on the nature of social science and to his rejection of all historical relativism. Popper defines social science as the study of the unintended consequences of social behavior. But what is unforeseen in the growth of science, is the new theories that result from conjectural scientific research. The content of theories in future science is in principle unpredictable in Popper's view, and he rejects all historicisms that purport to predict history including the history of science.

The strategic relevance of Popper's reference to the institutional character of science in the context of objective knowledge becomes evident when contrasted with Kuhn's view that in the history of science the ontology of a prevailing theory assumes an institutional status. It seems likely that Popper was led to think of the aim of science in institutional terms as a result of Kuhn's views. Kuhn's thesis that the prevailing theory or paradigm assumes institutional status, means that the ontology of the prevailing paradigm functions as the criterion for scientific criticism, and that therefore commonly recognized revolutionary developments in the history of science, which introduce a new theory and ontology into a science, must be viewed as institutional changes with no larger framework providing continuity. In Popper's view this radical discontinuity is historical-relativist and irrational. In his "Rationality of Scientific Revolutions" he paraphrases Marx, saying that the growth of science is "revolution in permanence", but Popper intends this phrase to mean that there exists criteria for scientific criticism that are invariant through even the most revolutionary developments, that make scientific change rational and meaningfully progressive. Thus the force of Popper's statement that the growth of objective scientific knowledge is a social institution, is that the objective nature of science makes revolutionary scientific change a change within an enduring set of institutional value standards, instead of a breakdown of the institution. The criteria for scientific criticism that operate as the institutional values of the scientific community are in Popper's view independent of the semantics and ontology of the prevailing theory or paradigm. As he says, science is "subjectless." In his 1982 "Introductory Comments" to *Quantum Theory and the Schism in Physics* Popper compares his idea of metaphysical research programmes to Kuhn's idea of paradigms, but nevertheless maintains that metaphysical

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research programmes can be rationally reconstructed and rationally criticized, even though they cannot yet be empirically tested.

Discovery

Popper's rejection of the naturalistic theory of meaning had the interesting consequence of leading him to exclude consideration of the topic of scientific discovery from philosophy of science, which he viewed as entirely a matter of logic and objective knowledge. He believes that the topic of scientific discovery is exclusively a psychological and therefore subjective matter. The conjectures resulting from the discovery process belong to world 3, but the discovery process itself belongs to world 2, and events in world 2 cannot determine the contents of world 3. While this view offers very adequate recognition to the freedom in the creative discovery process, it also relegates a whole area of interest for philosophers to the empirical studies of the psychologists. And as it happens, the topic of discovery has become a central concern of the emerging specialty of cognitive psychology, although Popper would reject the cognitive psychologists' explicit psychologism. Popper's exclusion of discovery is perhaps due partly to his identification of traditional discussions of discovery with the "logic" of induction. When he rejects inductive logic, he therefore rejects all logic from the discovery process. He later modified this view, when he explained what he would admit to be possible with an "induction machine." Considering the work done by contemporary information scientists working in artificial intelligence, Popper's later statements are more plausible. Up to the present time at least, these information scientists would find it difficult to deny that the system designer of a discovery model must decide on what Popper calls "a repetition" of an event, which is to say the system designer must firstly conceptualize the input to the system. The discovery systems are not unconditioned much less historicist, but must draw from the current state of the science under investigation for their input language.

Comment and Conclusion

Popper`s philosophy was occasioned by Einstein's development of relativity theory, a milestone episode in the history of science that Popper took to be paradigmatic of scientific progress. And Popper's philosophy is

also a milestone in the history of philosophy, because it represents a fundamental problem shift. While Carnap and other Positivists continued their efforts to establish theoretical science including Einstein's theory, on firm ontological foundations, Popper rejected the naturalistic theory of meaning that supposedly supplies such a foundation, and accepted the revision of scientific explanation as a matter of course. Positivist foundational problems, such as the problem of the meaningfulness of theoretical terms, became pseudo problems as a result of Popper's problem shift, while the problem addressed by Popper, the rational growth of science without foundations, has become central to philosophy of science.

Popper's philosophy was not occasioned by the development of the modern quantum theory, and he spent much of his professional career attempting to reconcile his philosophy and the modern quantum theory. It may be said that just as Carnap had attempted to reconcile Positivism and Einstein's relativity theory, so too Popper had attempted to reconcile his philosophy and the new quantum theory, except that Popper also presumed to revise the semantical interpretation of quantum theory. In the meanwhile the Pragmatist philosophers have taken up a role relative to quantum physics, accepting it as the paradigm of modern physics, that Popper had taken up relative to Einstein's relativity theory. As a result Popper's philosophy now represents the conservative position in the contemporary professional literature of philosophy, a position that casts him in the role of more the defensive rear guard than the aggressive *avant garde*.

One of the distinctive aspects of the historical development of quantum theory is the persistent plurality of semantical and ontological interpretations compatible with the same experimental measurements and mathematical formalism. This plurality has caused lengthy controversies among the physicists; Lande's list of seven alternative interpretations may be taken as indicative of this plurality. Popper's response to this situation in modern microphysics was to create still another interpretation for the quantum theory, his particle-propensity interpretation, because like Einstein, he rejects the schism in physics and believes that a uniform ontology for both microphysics and macrophysics is necessary. As it happens the history of physics has taken a different turn. In 1968 Gabriel Veneziano resurrected an old mathematical formula called Euler's beta function to develop what is now called string theory. In 1974 Schwarz and Joel Scherk applied the string theory to incorporate gravitation, and in 1984 Green and Schwarz united gravitation with quantum theory, thereby starting the superstring revolution in physics. However, the theory is not yet testable empirically.

The Pragmatists reacted differently than Popper. For them the quantum theory is the paradigmatic episode in the history of science, and their more accepting attitude has occasioned another problem shift in philosophy of science. While some Pragmatists express reason to advocate one or another particular interpretation of quantum theory as distinctively interesting from the viewpoint of philosophy of science, the reason is not the particular philosopher's prior ontological commitments. Rather it often proceeds from a belief in the importance of the particular interpretation for scientific discovery (discovery is the topic that Popper did not consider even to be a part of philosophy of science). The focus on the problem of scientific discovery has in turn occasioned the problem shift: philosophers have reconsidered the semantical and ontological pluralism represented by the different interpretations of quantum theory. They have concluded that the pluralism is an outcome of certain properties of language, and that it is therefore a strategic condition for continuing the growth of science (growth is the topic that Popper considered to be central to philosophy of science). In brief Popper's approach is to attempt to adjust the semantics and ontology of quantum physics to his philosophy of science, while the Pragmatists' approach has been to attempt to adjust philosophy of science to account for the phenomenon of semantical and ontological pluralism in science and to identify its function.

As it happens, Popper's rejection of the naturalistic theory of meaning supplied philosophers with the point of departure for addressing this phenomenon of semantical pluralism, and they did so in ways that Popper did not accept. The philosophical view that affirms an artifactual character of the semantics of language admits to a wholistic variation, that introduces an unresolvable cultural and historical relativism into science, which in turn makes problematic the intersubjective objectivity and rationality that Popper considers to be necessary for the growth of science. The affirmation of this wholistic variation and its consequent linguistic relativism, is occasioned by the thesis that scientific change involves semantical change. Popper's philosophy does not address the problem of semantical change, because he identifies all attempts at semantical description or "meaning analysis" with essentialism. As a result contemporary philosophers of science have moved on to new problems that Popper was unprepared and unwilling to address.

Finally some comments are in order about Popper and the Positivists' truth-functional logic. In addition to criticizing the Logical Positivists for their Positivism, Popper also refrained from using their favorite logic, the Russellian symbolic logic. This logic is called a truth-functional logic,

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because the truth value of any compound statement, such as a conditional "material implication", can be determined by reference to the truth values of its component elementary statements. Therefore in the truth-functional logic the truth tables for all compound statements are complete for all combinations of truth values of the component statements, thus enabling the symbolic logic to have the closure of an algebra, which is very desirable for a mathematical system including a logic. In contrast the nontruth-functional or Stoic conditional statement affirms the existence of a dependency connection between the truth values of the antecedent and consequent clauses, such that the truth of the compound statement is not determined by the truth values of the component clauses for most combinations of truth values. The affirmed connection might for example be a logical one, as obtains between the premises and conclusion of the categorical syllogism. The conditional statement expressing a syllogism would have an antecedent clause consisting of the conjunction of the major and minor premises and a consequent consisting of the conclusion. As is well known, the conditional connection is the logical inference, which may be valid independently of the truth of its constituent statements - either in the conjunction of the premises in the antecedent clause or in the conclusion in the consequent clause. The logical inference may be invalid such that the conditional statement is false, yet both of the premises in the antecedent and the conclusion may be true. Thus the conditional statement is not merely an oblique conjunction. Of greater interest in philosophy of science are those cases in which the nontruth-functional connection is an empirical hypothesis of a causal connection instead of a logical connection. If the antecedent clause is false, then the truth of the conditional statement is unknown, because the empirical test is not valid when it is not executed in accordance with its test design. If the antecedent is true, the test is valid, and the test outcome is not a falsification, then the theory can reasonably be believed for the time being, but its truth is not established. The truth of the nontruth-functional conditional statement is known from the truth of its component statements only in the event of falsification. The truth tables for truth-functional conditional and the corresponding nontruth-functional conditional logical forms are contrasted as follows, where T="True" and F="False":

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	Truth-Functional Truth Table			Nontruth-Functional Truth Table		
	A	B	A ⊃ B	A	B	If A, then B.
1.	T	T	T	T	T	Not Falsified
2.	T	F	F	T	F	Falsified
3.	F	T	T	F	T	Invalid Test
4.	F	F	T	F	F	Invalid Test

Consider the stereotypic universal "All ravens are black", which Popper would consider a theory, since he considers all descriptive terms to be a type of theoretical term which the Positivists called "disposition terms." Then re-express the universal categorical proposition as a conditional statement in the form of a material implication, A ⊃ B, of the Russellian symbolic logic:

$$(x) (xRaven \supset xBlack).$$

This is conventionally rendered in English as "For all *x*, if *x* is a raven, then *x* is black", or more colloquially as "For every thing, if a thing is a raven, then it is black." Popper's falsificationist thesis of scientific criticism requires a nontruth-functional logic in which only the falsehood of the universally quantified conditional can be determined from knowledge of the truth values of its component elementary statements. In this case the connection is a hypothesis or theory proposed for empirical testing.

The logic of the test resembles the *modus tollens* argument form, except that the truth of the conditional statement itself is in question rather than the truth of its component clauses. The antecedent clause expresses the initial conditions of the experiment, and the clause is actually a rather complex description of the experiment. When these conditions are realized in the execution of the experiment, then the antecedent clause is true. The consequent clause expresses the predicted outcome of the test. And the connection between the antecedent and consequent clauses is the universal hypothetical claim of the theory being tested. If the description of actual test outcome described in the same terms as the consequent contradicts the consequent, such that the prediction is false, then the conditional hypothesis is falsified. This is the set of truth-values represented in the second line of the truth table displayed above for the nontruth-functional conditional compound statement.

The third and fourth lines of the nontruth-functional truth table represent those cases in which the antecedent is false, which is to say that the

test was not executed correctly in accordance with its test design statements. Nothing can be concluded about the theory expressed by the conditional statement from an invalid test execution. The first line of the nontruth-functional truth table represents the case in which the test execution is valid, and in which the test outcome is as predicted by the expression constituting the consequent clause of the conditional hypothesis. Scientists in the cognizant scientific profession may accept the tested and nonfalsified conditional hypothesis as what might be called a "working hypothesis", which could be assumed as a test-design statement in a test of some other theory, but the conditional logic does not compel acceptance. Nor does nonfalsification prohibit any scientist from reconsidering the hypothesis later, even though it has been tested and not been falsified. Furthermore scientific theories are routinely more complex than the all-ravens-are-black stereotype. There may be many alternative tested and nonfalsified theories with alternative antecedent clauses expressing alternative test designs from which a scientist may choose a working hypothesis for future research, because there may be multiple alternative sufficient conditions to produce the same predicted consequence.

The Frege-Russellian "logistic" agenda to reduce mathematics to logic motivated the symbolic logicians to construct the truth-functional logic that firstly reduced logic to a closed mathematical algebra. And the result has been a disservice to philosophy of science. The Logical Positivist philosophers exercised themselves with their problem of so-called theoretical terms. They believed that they are being very sophisticated and impressively technical by using the Russellian mathematical logic. Curiously the truth-functional truth table dictates that the truth of their so-called observation sentences occurring in the antecedent and consequent atomic sentences, guarantees the truth of the material implication connecting them, an implication which is a universal statement. Yet even they did not maintain that material implications expressing empirical generalizations are eternal verities like the component observation sentences. Nonetheless *Philosophy of Science* and *British Journal of Philosophy of Science* still contain enough Russellian chicken tracks to suggest that their pages have been trampled in a hen house of panicked birds convinced that the sky is falling. Ironically for these philosophers of science who are still using the Russellian symbolic logic the sky has been falling for decades. In due course even the lesser lights of the profession will recognize that the technical pretenses of the Russellian logic can no longer supply the façade of

sophistication that had formerly masked its sophistic claim as the logic for science.

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 beliefs) 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's and Paul Feyerabend's variants of this artifactual thesis of the semantics of language led these two philosophers as well as others to propose new roles for the phenomenon of prejudicial belief in the history and dynamics of scientific development.

Thomas S. Kuhn (1922-1996) was born in Cincinnati, Ohio. He received a Bachelor of Science degree *summa cum laude* from Harvard University in 1943. His first exposure to history of science came as an assistant to James B. Conant in a course designed to present science to nonscientists. He received his Ph.D. from Harvard in 1949, and 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 taught at University of California at Berkeley. In 1993 he wrote a brief autobiography titled *Killing Time*. The story of the historical approach in twentieth-century philosophy of science, however, begins with Conant.

KUHN AND FEYERABEND

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 popular *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 the three above-mentioned 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 what he calls 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 the Positivist view that science is a quest for certainty, and he emphasizes that science is a speculative enterprise that is successful only to the degree that it is continuous.

On his skeptical 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 oxygen theory. 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 received truth is quite justified. He concludes that to suppose that a scientific theory stands or falls on the issue of one experiment is to misunderstand science entirely. Conant characterizes the first fifty years of the nineteenth century that culminated in the chemists' atomic theory of matter, as a period of the conflict of prejudices. He notes that one who is not familiar with this episode in the history of science will be amazed to discover that all the relevant ideas and all the basic data for the atomic theory were at hand almost from the outset of the nineteenth century. An analysis of the arguments, pro and con, shows that certain preconceived

ideas then current among scientists blocked its development. Still, Conant rejects the view that the scientific way of thinking requires the habit of facing reality quite unprejudiced by any earlier conceptions. In his *Science and Common Sense* he admits that prejudices are emotional and nonlogical reactions. Yet he also maintains that every scientist must carry with him the scientific prejudices of his day - the many vague, half-formulated assumptions which to him seem common sense. Apparently as a result of his acceptance of prejudice as an inevitable fact in the dynamics of science, Conant unabashedly declares that his dynamic view of science is his prejudice, and adds that he makes no attempt to conceal it.

It may be said that one of the differences between Kuhn and Conant is that the latter regards prejudice as merely an inescapable fact in the history of science, while the former regards it as having a 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 preparation for 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 therefore 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 that is the central feature of scientific revolutions is that old theories are replaced by new and incompatible theories. 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 as he sees in 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 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, 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 their specialties.

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 a historian of science, he discovered that much scientific behavior including that of the greatest scientists persistently violated accepted methodological canons, and that he wondered why these apparent failures to conform to the canons did not at all seem to inhibit the success of the scientific enterprise. The accepted methodological canons that Kuhn has in mind are not only those of the Positivists but also Popper's falsificationist thesis. He states that his altered view of the nature of science transforms what had previously seemed aberrant behavior into an essential part of an 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 metaphor, scientific revolutions.

KUHN AND FEYERABEND

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 that the paradigm seems sufficiently promising as a guide for future research and 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. In fact the paradigm is the source of the concepts needed for recognizing the new fact and for giving it anomaly 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.

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(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, because 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 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. 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.

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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 its 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 needed to explain how science is empirical and what difference observations make. He reports that he had agreed and that he replied he had not previously seen 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 offered no analysis of meaning to explain meaning change. Then in his third phase Kuhn attempted language analysis to explain his thesis of incommensurability with comparability. 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* 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 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. 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 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 "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.

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Popper's Criticism of Kuhn's Normal Science Thesis

Popper's criticism in reply to Kuhn is set forth in "Normal Science and its Dangers" in *Criticism and the Growth of Knowledge*. He criticizes the aim of normal science as set forth by Kuhn, and he rejects the historical relativism that he finds in Kuhn's thesis. 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. Consensus Paradigm

Feyerabend's criticism of Kuhn is given in his "Consolations for the Specialist" in *Criticism and the Growth of Knowledge.* He 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. 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

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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.

Feyerabend next 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.

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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 that 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?

Feyerabend then 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

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Lakatos, a professor of logic at the University of London, that the practices of tenacity and proliferation do not belong to successive periods in the history of science, but rather are always copresent. Feyerabend says that the interplay between tenacity and proliferation is an essential feature of the actual, historical development of science. It is not the puzzle-solving activity that is responsible for the growth of knowledge, but the active interplay of a plurality of tenaciously held views. It is the continuing intervention of new ideas and the attempts to secure for them a worthy place in the competition that leads to the overthrow of old and familiar paradigms. Feyerabend furthermore maintains that revolutions are basically matters of appearance, and that during a revolution there is actually no profound structural change such as a transition from normal to extraordinary science as described by Kuhn. Thus, instead of advocating conformity to a monolithic consensus paradigm, as Kuhn does, Feyerabend issues 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, University of Chicago philosopher of science, 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, which 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. He 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.

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 Kuhn's 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. In these replies he distances himself from the sociologists while still affirming the sociological character of his theory of science. He minimizes the differences between himself and Popper while still affirming the uncritical attitude in normal science. And he differentiates his views on incommensurability from those of Feyerabend,

and also explains how a multiplicity of theories emerges in the crisis period without Feyerabend's principle of proliferation.

Firstly Kuhn distances himself from the sociologists. He states that in this matter he agrees with Popper; he says the received theories of sociology and psychology are weak reeds from which to weave a philosophy of science, and he adds that his own work no more relies on current sociological theory than does Popper's. But he still maintains that his theory of science is intrinsically sociological, because whatever scientific progress may be, it is necessary to account for it by examining the nature of the scientific group, discovering what it values and what it disdains. Scientists must make decisions. They must decide what statements to make unfalsifiable by *fiat* and which ones will not be considered unfalsifiable. Using probability theory they must decide upon some 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 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.

Kuhn also 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 Romantic

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sociology, which aims at interpretative understanding). 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.

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 scientists' objectives, then, are 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 adds that the difference between him and Popper on this issue of criticism is only one of emphasis.

Kuhn then 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 retains an 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 functions as an example that enables the scientist to recognize similar cases without having to articulate or to characterize the similarity relations explicitly in a generalization. He states that the practice of normal science depends on a learned ability to group objects and situations into similarity classes, which are primitive in the sense that the grouping of objects is done without supplying an answer to the question, similar with respect to what? In scientific revolutions some of the similarity relations change, such that objects 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 special 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 euphoria by American academic sociologists. Monsieur Jourdain, the *parvenu* in Moliere's comedy, *Le Bourgeois Gentilhomme*, had aspired to write prose, and was delightedly surprised when he was told that he had been speaking prose for more than forty years without knowing anything about it. Moliere's play has its analogue in contemporary American academic sociology save for the absence of any comedy. The prevailing opinion among researchers in the more mature scientific professions is that sociology is merely a pretentious *parvenu* with a literature of platitudes expressed in jargon. 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.

A paradigmatic example of Kuhn's influence on sociologists is represented by Hagstrom's *The Scientific Community* (1965). 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 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)

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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, and she defines "universalist" 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 of functionalism. Even though his functionalist sociology is now passe, Parson's Romantic philosophy of science continues to haunt American academic sociology.

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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 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, as Kuhn struggles 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 that incommensurable magnitudes have no common integer divisor except the number one. In mathematics incommensurability means there is no common measure, and for his thesis of semantic incommensurability Kuhn substitutes "no common language" for "no common measure" for metaphorical use in his *Structure of Scientific Revolutions*.

Initially in *Structure of Scientific Revolutions* Kuhn's discussions of incommensurability were vague. He says that 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. 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 such 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, and 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. Thus 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 tagging and tracing of 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 noted 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 co-referencing 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 scientists 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. But 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. 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.

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Kuhn attempts to illustrate this kind of incommensurability in the transition from the phlogiston theory of combustion to the modern oxygen theory. In the phlogiston theory the phrase "dephlogisticated air" can mean either oxygen or oxygen-enriched air, while the phrase "phlogisticated air" means air from which oxygen has been removed. In the phrase "phlogiston is emitted during combustion", the term "phlogiston" refers to nothing, although in some cases it refers to hydrogen. Kuhn maintains that for the historian of science incommensurability in this case is dealt with by learning the meanings in the old texts by reference determination. He agrees that historians dealing with old scientific texts can and must use modern language to identify referents of out-of-date terms. Like the native's pointing out "gavagai" referents in Quine's radical translation situation, 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 or a 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. Co-referencing. This means that two users of the same language can employ different criteria for identifying the referents of its descriptive terms. Co-referencing 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 them 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 that like Kant's *Ding an sich* is ineffable, inscrutable, and indiscernible.

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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 the meanings of "male" and "horse" may overlap, but not those of "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. But 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 Feyerabend 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) his philosophy is almost exclusively concerned with the aim of science and its implications for 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 aim of science and 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 phase, which is incidental to the conscious aim of science, semantic incommensurability between old and new successive theories makes the revolutionary transition such that empirical 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 enables 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 in the decades following *Structure of Scientific Revolutions*. The result of his linguistic analysis is 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 mentions 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 of 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", what Hempel called "bridge principles", and what Nagel calls "coordinating definitions" and "correspondence rules." 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 know 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 the Boyle-Charles' law is made a logical consequence of the principles of mechanics, when these principles are supplemented by a hypothesis about the molecular constitution of a gas, a statistical assumption about the motions of molecules, and a postulate 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). 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. The coordinating definitions or correspondence rules, as Nagel also calls them, have the same functions as Carnap's reduction sentences and Hempel's 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. 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 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, where 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 meanings of the descriptive terms in the impetus theory are fixed by the procedures and assumptions of the theory. But Feyerabend maintains that the concept of impetus as fixed by the usage established in the Aristotelian theory of motion cannot be defined 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.

The Sapir-Whorf Hypothesis

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 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**, 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 as 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 1975 work, a summary of the Sapir-Whorf hypothesis is in order.

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. 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 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 thesis that language structure controls thought, which Whorf sets forth in his theory of covert categories, is central to his theory of linguistic relativity. He locates his development of linguistic relativity in the history of cultural anthropology in the lineage of Franz Boas and Edward Sapir.

KUHN AND FEYERABEND

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 on to 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, 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 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* (1978) Feyerabend says in a section containing some autobiographical notes that von Weizsacker (a former student of Heisenberg) 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 metaphor, "scientific anarchy", which he fully intends to be intellectually more radical than Kuhn's metaphor, "scientific revolution". Feyerabend's metaphor 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 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 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 Galileo 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 had in common by 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 worldview. It is necessary to retain the new cosmology, until it has been supplemented with the necessary auxiliary sciences, so that the language in which observations are expressed may be revised. Feyerabend finds what he illustrates with Galileo to be no less applicable today. He says that today's rational sciences survived, because irrational prejudices were permitted to have their way, and that it is advisable to let one's inclinations go against reason in any circumstances. Propaganda is of the essence.

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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 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

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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 "forms of perception". Some philosophers such as Heisenberg consider Bohr's forms of perception to be neo-Kantian. Feyerabend notes that Positivists do not customarily consider phenomena to have any forms, and he 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 Feyerabend's 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 that 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 the aid of his 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 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 interpreting 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 microphysics 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 Schrödinger were taken less and less seriously. 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.

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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 from Bohr's Copenhagen interpretation.

Feyerabend on Relativism, Historicism, and Realism

The consequential outcome of the lengthy debate between Hanson and Feyerabend resulted 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 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 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 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 he maintains that 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 not only historicist and relativist, but 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 really just 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!" As it happens, this is exactly what Feyerabend had previously said in response to quantum 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. Contrary 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 the 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 posses 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. 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 Kuhn calls 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 his 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 leads 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 his view is the perpetual succession of conjectures and refutations in which each successive conjecture or theory can explain both what had been explained by its falsified predecessor and the anomalous cases that falsified the predecessor.   The new theory is therefore more general than its predecessor, while it also replaces and corrects its falsified predecessor.   Popper saw the process of refutation as involving a deductive procedure having the logical form of *modus tollens*. And because it is a procedure in deductive logic, it is not subject to cultural or historical change.   Popper admits that application of the logic in the sense of experimental identification of the falsifying instances may be problematic and may take several years.   But he maintains that the logic of falsification isolates the conditions for scientific progress, and that it represents adequately how science has proceeded historically, when it has proceeded successfully.   He maintains that this procedure may be said to have become institutionalized, but its validity, which is guaranteed by deductive logic, does not depend on its institutional status.   Its validity is ahistorical, and will never be invalidated by historical or institutional change; it is tradition independent.

Both Kuhn and Feyerabend deny that Popper's vision of the development of science is historically faithful.   The principal deficiency in the Popperian vision is its optimistic assessment of the decidability of falsification.   Not only do they view the range of nondecidability of scientific criticism to be greater than Popper thinks, but they also view it as having an integral 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 most of 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; unlike classical physics 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.

Regrettably Feyerabend did not elaborate on his historicist philosophy of scientific explanation. 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's 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. He renders Einstein as saying 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. Einstein, who said that thinking without concepts is like breathing in a vacuum, would not have agreed with Feyerabend's rendering of his views. 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 the linguistic analysis. As mentioned above Kuhn postulates a structured lexical taxonomy, which he also calls a conceptual scheme, and he 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 the taxonomic module is prelinguistic and possessed by animals, therefore 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 do the bonding and structuring. 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 associated semantics by articulating the contradiction to the theory's prediction. The empirically based contradiction due to falsification 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 semantical disintegration due to the more extensive rejection of current beliefs, the more demanding the task.

Two reasons for incommensurability can be distinguished in Kuhn's literary corpus. Firstly incommensurability is due to semantics that is unavailable in the language of an earlier theory that is available in the language of a later one. Secondly incommensurability is due to semantic restructuring of the taxonomic lexicon. However, only the first reason compels 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, may very possibly render 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 described 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 available 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 available 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, introduction of new semantic values, occurs in developmental episodes that appear least to be revolutionary, while those involving extensive reorganization and thus appearing most revolutionary have no semantic incommensurability.

In his "Commensurability, Comparability and Communicability", Kuhn says that if scientists moving forward in time experience revolutions, their switches in gestalts will ordinarily be smaller than the historian's, for 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 sciences. 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 the product of phenomenal-seeing description is a new kind term, 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 revolutions. In summery it must be said that Kuhn was a rather naïve philosopher, and was quite unprepared to undertake a linguistic analysis of science. His idea of learning had been anticipated by Hesse and by Feyerabend. And his idea of Kantianism with movable categories echoes the Kantianism that Heisenberg says is in the views of Bohr, who was also a naïve philosopher. The first object of the human mind is not its own ideas. The first object is reality, and then by reflection it knows its ideas. It is

better to shed irresponsible Kantianism once and forever, and to focus on the semantics, ontology, and pragmatics of the language of science.

Turn next to the philosophy of Feyerabend, which is more elaborate and more sophisticated than Kuhn's. Feyerabend began with an agenda for modern microphysics: to show how a realistic microphysics is possible. Initially the conditions that he believed a realist microphysics must satisfy were taken from Popper's philosophy of science, and these conditions are contained in Popper's idea of universalism. However, there is an ambiguity in Popper's universalism, and that ambiguity was not only brought into Feyerabend's agenda while he had accepted Popper's philosophy, it was also operative in his philosophy after he rejected Popper's philosophy, because he rejected universalism in *both* senses. The first meaning of "universal" refers to the greater scope that a new theory should have relative to its predecessors, and the second meaning refers to the logical quantification of general statements. Feyerabend thought that 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 "universal". Thus his 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: 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. Recent developments in string theory and M-theory notwithstanding, 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, namely Aristotle's doctrine of the four cause, which explained qualitative change as well as mechanical motion. And it might also be noted that quantum theory is less universal than Newtonian mechanics, which was believed to be applicable to microphysical orders of magnitude.

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. In fact Quantum theory must be universal in this second sense, because its experiments are repeatable. Feyerabend's 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 Hempel's and Quine's rejections of the analytic-synthetic dichotomy, and notwithstanding the fact that Quine rejected analyticity altogether, the distinction may still be retained 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 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 with numeric variables having 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 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 in the several statements constituting 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 predicate 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 information. 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. Adding universal negations relating concepts predicated of the common subject clarifies the meaning of the subject term by showing equivocation. 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 turn to science. In all scientific 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. 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 test design statements does not change; it supplies semantical continuity. But the semantics of the descriptive term changes; it is different before and after the test. Before the execution of a test of the 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, such that if the test outcome is an inconsistency between the test design statements and the theory statements, then it is the theory that is to be viewed as falsified. This independence of test design statements is required for contingency in the test, and it also 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 must be vague with respect to the theory. 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 his supporting advocates of the theory may have such high confidence in their theory that for them the theory may also 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 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 proposer and its advocates. However, the original proposer 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 is a strategy that need not be rejected as content decreasing, as Popper would have, but may occasion 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 augmented such that they could be incorporated into a deductive system and derived from the nonfalsified theory after the test. The resulting deductive system would then 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. The concepts associated with the descriptive terms in the original test design statements were initially viewed as vague relative to the terms in the theory, but may later receive more precise meanings 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 had assumed the semantical status that Heisenberg called "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*

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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; there is a third alternative. 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 nonfalsifying test outcome, 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 may be developed, if it ever 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 such 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 microphysical theory, and Heisenberg's term "everyday" is appropriate to describe the vague concepts associated with these terms. But after the nonfalsifying test outcome is known, the whole meaning complex constituting each concept is more properly called a "quantum" concept, because the quantum theory then 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. He thus reconceptualized his observation language, and practiced what Feyerabend called counterinduction.

In summary, semantical analysis reveals that duality need not be expressed in classical terms required 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 scientific criticism. Therefore 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 resulting from any need to express duality with complementarity. 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 Wharf'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 or at any other time.

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 thus 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 1919-eclipse test that had such a formative influence on Popper. Two British astronomers undertook this test, 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 were made by averaging over the many photographs. Such are the essentials of the design of the Eddington eclipse experiment. 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 are universal, 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 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 with part of its meaning contributed by one or several 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 constitutes 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 as 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 nonfalsified 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. 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 are 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. Before the test 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 chosen. 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 conditioned upon chosen universal test design statements. The empirical test is the window to new vision.

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 reconsiderations 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. 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, although such a dissenting Newtonian would likely be expected by his colleagues to offer an alternative test design. In any event 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" 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 offends the droll scholars of the profession, who tend to treat him dismissively. But every profession has its pedantic slow learners. Feyerabend stands above the academic crowd by an order of magnitude. He 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.

RUSSELL HANSON, DAVID BOHM
AND OTHERS ON
THE SEMANTICS OF DISCOVERY

Norwood Russell Hanson (1924-1967), born in New Jersey, was a U.S. Marine Corps fighter pilot during the Second World War, who earned the rank of major, and was awarded the Distinguished Flying Cross and the Air Medal for flying combat missions over Japan. Afterward he studied at the University of Chicago, Columbia University, and Yale University in the United States, and then studied at both Oxford University and Cambridge University in England. He received a Ph.D. from Oxford in 1955 and a Ph.D. from Cambridge in 1956, and was afterward a fellow at the Institute for Advanced Study at Princeton. He accepted a faculty appointment at Indiana University in 1957, where he was founder and chairman of Indiana University's Department of History and Logic of Science from 1960 to 1963. He then accepted a professorship on the philosophy department faculty of Yale University, which he had at the time of his premature death at the age of forty three in a crash of his private airplane in 1967. His principal works are *Patterns of Discovery* (1958) and *Concept of the Positron* (1963). At the time of his death he left an uncompleted textbook in philosophy of science intended for first-year college students, which was edited by Willard C. Humphreys, a former student of Hanson, and then published as *Perception and Discovery* (1969). A year after his death a complete bibliography of his publications appeared in a memorial volume of *Boston Studies in the Philosophy of Science*, Volume III (1968).

David Bohm (1917-1992) was born in Wilkes-Barre, PA, and received his doctorate in physics from the University of California. He taught physics at Princeton, and eventually moved to England. He was professor of theoretical physics from 1961 at Birkbeck College, University of London, where he was professor emeritus from 1983 until his death in 1992. A brief biography may be found in the "General Introduction" in *Quantum Implications* (ed. B.J. Hiley and F. David Peat, 1987), and a three-hundred-

fifty page biography by Peat was published under the title *Infinite Potential: The Life and Times of David Bohm* (1997). Bohm's initial statement of his interpretation was published in 1952 in two articles in the *Physical Review*, in which he reports that the interpretation was originally stimulated by a discussion with Einstein in 1951. His principal statements of his hidden-variable interpretation of quantum theory are set forth in two of his books. The earlier is a brief monograph of only one-hundred-forty pages titled *Causality and Chance in Modern Physics* published in 1957, and the more recent is his more elaborate *Undivided Universe* co-authored with Basil Hiley and posthumously published in 1993. After publishing his seminal articles in 1952, he found that his interpretation had been anticipated in important respects in 1927 by Louis de Broglie (1892-1987). De Broglie's interpretation had been criticized severely, and he had consequently abandoned it, but Bohm had further developed the thesis enough that the fundamental objections confronting de Broglie had been answered. Bohm's interpretation was shown to be consistent with all the essential characteristics of the quantum theory, and additional suggestions were made by Vigier, a colleague of de Broglie. De Broglie then returned to his original proposals, since he believed that the decisive objections against them had been answered. Bohm and Vigier then published a joint paper setting forth the interpretation in the *Physical Review* in 1954, and de Broglie wrote a "Foreword" to Bohm's 1957 book. Bohm was one of the physicists who recognized nonlocality in the quantum theory. Peat's generally sympathetic biography shows how the idea of nonlocality led Bohm firstly to his wholistic ontology for physics, then to his process metaphysics, and finally to his mysticism of the implicate order, according to which mind and matter are indivisibly united. To the dismay and consternation of his friends and colleagues, this mysticism was encouraged by Bohm's long-time association with an Indian guru, and also led Bohm to take seriously the mind-over-matter exhibitions of a stage magician.

Hanson takes very seriously the question of the interpretation of the modern quantum theory, and he truculently defends the Copenhagen interpretation. In "Appendix II" to his *Patterns of Discovery* he notes that while for most practical microphysical problems Born, who accepted the Copenhagen interpretation, and Schrödinger, who did not, would have made the same theoretical calculations. Nevertheless, their alternative interpretations organized their thinking differently, and consequently influenced their future research work in very different ways: after 1930 Born was led to work on collision behavior, on the statistical analysis of scattering

matrices, while Schrödinger pursued investigation of the so-called ghost waves of the elementary particles. The interpretations, therefore, are important because each supplies an agenda that influences the direction of future research in physics. Hanson does not view all interpretations as equally worthy of consideration, and he considers particularly unfortunate is the "hidden-variable" interpretation developed by David Bohm. In contrast to the Copenhagen interpretation with its duality thesis that the wave and particle are two manifestations of the same physical entity, Bohm's alternative interpretation is that the wave and particle are different physical entities, even though they are never found separately, and that the wave oscillates in an as yet experimentally undetected and therefore hidden subquantum field. In the context of the topic of scientific discovery Bohm's views are interesting, because they illustrate the semantical approach to scientific discovery and theory development in physics. They illustrate the use of figures of speech as a technique for theory development based on certain postulated basic similarities between the macrophysical and microphysical orders of magnitude, similarities that are denied by advocates of the Copenhagen interpretation. Consider firstly Bohm's early advocacy of the Copenhagen interpretation, and then his later agenda for future physics including his hidden-variable interpretation for quantum theory.

Bohm's Early Copenhagen Views

The hidden-variable thesis is Bohm's more mature view. He started out as an advocate of the Copenhagen interpretation, which he also calls the usual interpretation, and then changed his mind after the talk with Einstein in 1951, the year in which his textbook titled *Quantum Theory* was published setting forth his earlier view. There are at least two noteworthy features of this early book. The first is Bohm's distorted understanding of Bohr's philosophy of quantum theory. The second is his ontology for quantum theory, the ontology of potentialities, which anticipated Heisenberg's similar ontology of *potentia* by seven years.

In the "Preface" to his *Quantum Theory* Bohm says that as a result of the work of Neils Bohr, it has become possible to express the results of quantum theory in terms of comparatively qualitative and imaginative concepts, which are totally different from those appearing in the classical theory. He rejects the view that the quantum properties of matter imply the renunciation of the possibility of their being understood in the customary

imaginative sense, and that they imply the sufficiency of only a self-consistent mathematical formalism which can in some mysterious way correctly predict the numerical results of experiments. The eighth chapter of the book is titled "An Attempt to Build a Physical Picture of the Quantum Nature of Matter", and Bohm writes in a footnote that many of the ideas appearing in the chapter are an elaboration of material in Bohr's *Atomic Theory and the Description of Matter*. However, Bohm's understanding of Bohr is distorted. Bohr maintained an instrumentalist view of the equations of quantum theory, which rejects any semantics or ontology for quantum theory, and he repeatedly denied explicitly that quantum phenomena are pictureable. From Bohm's statement in his 1952 articles that his hidden-variables thesis was the result of a talk with Einstein in 1951, it is reasonable to speculate that Einstein had read Bohm's book, had recognized that Bohm was ripe for disillusionment with the views in Bohr's philosophy, and had concluded that Bohm was ready for induction into the ranks of Bohr's critics. In any event whatever may have been Einstein's unreported comments to Bohm in their private conversation, the ultimate outcome after forty years was Bohm's *Undivided Universe: An Ontological Interpretation of Quantum Theory* (1973), a book in which Bohm explicitly says he is supplying an ontology to replace Bohr's epistemological interpretation.

The ontology for quantum theory that Bohm described in 1951 is a wholistic ontology of potentialities, in which the world is an indivisible unit where quanta have no component parts describable by hidden variables, and are not even separate objects, but are only a way of talking about indivisible transitions. This metaphysics is also called monism. At the quantum-mechanical level the properties of a given object do not exist separately in the quantum object alone, but rather are potentialities which are realized in a way that depends on the systems with which the object interacts. Thus the electron has the potentiality for developing either its particle-like or its wave-like form, depending on whether it interacts with an apparatus that measures either its position or momentum. Bohm's views are also realist; he does not maintain that the quantum phenomenon has its properties because it is being measured. He says that a quantum-mechanical system can produce classically describable effects not only in a measuring apparatus, but also in all kinds of systems that are not actually being used for the purpose of making measurements. Throughout the process of measurement the potentialities of the electron change in a continuous way, while the forms in which these potentialities can be realized are discrete. The continuously

changing potentialities and the discontinuous forms in which the potentialities may be realized are complementary properties of the electron.

Bohm anticipated Heisenberg's idea of potentiality, which Heisenberg did not propose until his *Physics and Philosophy* in 1958, the only place in Heisenberg's literary corpus where the idea is mentioned. But there are differences in their ideas of potentiality, because unlike Bohm's, Heisenberg's is not a wholistic version. In the 1951 book Bohm said that potentiality makes quantum theory inconsistent with the hidden-variables thesis, because the hidden-variables view is based on the incorrect assumption that there are separately existing and precisely defined elements of reality. The idea of potentiality is much more integral to Bohm's earlier interpretation than to Heisenberg's, and it had distinctive implications for Bohm. One implication is Bohm's thesis that mathematics is inadequate for physics. He says that the interpretation of the properties of the electron as incompletely defined potentialities finds its mathematical reflection in the fact that the wave function does not completely determine its own interpretation until it interacts with the measuring device, and that the wave function is not in one-to-one correspondence with the actual behavior of matter, but is merely an abstraction reflecting only certain aspects of reality. He believes that to obtain a description of all aspects of the world, one must supplement the mathematical description with a physical interpretation in terms of the incompletely defined potentialities.

Shortly afterwards he accepted the hidden-variables idea, and in the second chapter of his *Undivided Universe*, where he mentions in a footnote his anticipation of Heisenberg's idea of potentiality, he rejects altogether the potentiality thesis that the particle itself is created by the measurement process. In Bohm's hidden-variables view, the particle is not a wave-packet or otherwise created out of the wave; the particle is in reality distinct from the wave. His later view is not wave *or* particle, but wave *and* particle. That is, the wave and particle are not two alternative aspects of the same entity, but are different and separate entities.

Bohm's Agenda for Future Microphysics

Bohm's hidden-variable interpretation is an agenda for future microphysics, and his *Causality and Chance* (1957) sets forth three related objectives in this agenda. His first objective is the relatively modest one of demonstrating that an alternative to the Copenhagen interpretation is

possible, in the sense that it is not the only one that is consistent with the formalism and measurements of modern quantum theory. He states this objective not only because he has another interpretation in mind, but also because he maintains that the development of alternative views is important for the advancement of science, while advocates of the Copenhagen interpretation deny that any alternative view including one involving a subquantum level of magnitude is conceivable. For example in his "Questions of Principle in Modern Physics" (1935) in *Philosophical Problems of Quantum Physics* Heisenberg states that the uncertainty principle must be taken as a question of principle making other formulations into false and meaningless questions, just as in relativity theory it is supposed that it is in principle impossible to transmit signals at speeds greater than the velocity of light. But Bohm maintains that without alternatives the physicist is constrained to work along accepted lines of thought in the hope that either new experimental developments or lucky and brilliantly new theoretical insights eventually will lead to a new theory. In contrast Bohm maintains that one of the functions of criticism in physics is to suggest alternative lines of research that are likely to lead in a productive direction. He thus sees criticism with alternatives to be integral to scientific discovery. This objective is particularly attractive to the philosopher of science Paul Feyerabend, once an advocate of Bohm's interpretation, who to the end of his life maintained that creating alternatives is necessary for the advancement of science.

Bohm's second objective is to propose an interpretation of the history of physics, which shows successful precedents for the research strategy represented by his hidden-variable interpretation of quantum theory. The paradigmatic historical precedent he invokes is the atomic theory of matter, which postulated the existence of atoms unobservable at the time the theory was proposed. Analogously Bohm's strategy consists of postulating that there exists an order of physical magnitude below the quantum order of magnitude containing the quantum of action represented by Planck's constant. Bohm postulates that this subquantum order contains new types of qualitative phenomena governed by more deterministic laws than do those known to exist at the quantum level of magnitude. The existence of this postulated subquantum level of microphysical phenomena is denied by the Copenhagen interpretation advocates, and since there is as yet no experimental detection of any such subquantum phenomena, the theory that postulates them is said to have hidden variables.

Bohm opposes his historical interpretation to another that he calls mechanistic, a term that is unfortunately ambiguous in both philosophical and scientific usage, but which has a specific and somewhat elaborate meaning in Bohm's book. According to the objectionable mechanistic philosophy opposed by Bohm the qualitative diversity of things in the world can be reduced completely, without approximation, and in every possible domain of science to nothing more than the effects of some definite and limited general framework of quantitative laws, which are regarded as absolute and final. Prior to the development of quantum theory these quantitative laws were assumed to be deterministic; then with the development of the Copenhagen interpretation of quantum theory these laws were assumed to be indeterministic. Hence there are both deterministic and indeterministic varieties of mechanism. In the former variety causal laws are thought to be fundamental, while in the latter probability laws are thought to be fundamental. Indeterministic mechanism prevails today, because physicists have accepted Heisenberg's thesis that the indeterminacy principle represents an absolute and final limitation on our ability ever to define the state of things by any kind of measurement.

In *Causality and Chance* Bohm maintains that both causality and chance are fundamental and objective, and that both determinism and indeterminism are merely idealizations. Thus he departs from Einstein's determinism. He also rejects the subjective interpretation of probability, which says that the appearance of chance is a result of human ignorance. And he rejects the idea common to both deterministic and indeterministic varieties of mechanism that there is only one general framework of laws and a limited qualitative diversity. Bohm maintains that there are different levels of depth or orders of magnitude with each level having its own laws and qualitative diversity. In the history of physics revolutionary developments have occurred when laws and qualities at a higher level are explained by those of a lower level. Experiments may disclose a breakdown of an entire scheme of laws by the appearance of chance fluctuations not originating in anything at the higher level, but instead originating in qualitatively different kinds of factors at a lower level. For example in classical physics a particle such as an electron follows the classical orbit only approximately, while in a more accurate treatment it is found to undergo random fluctuations in its motions arising outside the context of the classical level. Thus Bohm affirms by way of historical analogy and on the basis of his nonmechanistic interpretation of the history of science that there is a still deeper level, a

subquantum level, which in turn explains the randomness that is detected at the higher quantum level of magnitude.

Bohm's hidden-variable interpretation is both an alternative interpretation of quantum theory motivated by this prior ontological commitment and also a discovery heuristic for which there is historical precedent. He maintains that new work is considerably facilitated by his thesis of a hidden subquantum order of magnitude, because the physicist can imagine what is happening, and can thereby be led to new ideas not only by looking directly for new equations but also by a related procedure of thinking in terms of concepts and models that will help to suggest new equations, equations which would not likely be suggested by mathematics alone. And he uses his postulated subquantum ontology as a basis for figures of speech such as analogy, which are a central feature in his discovery strategy. These figures aid in formulating new hypotheses for future physics both on the basis of similarities between the macrophysical and microphysical orders of magnitude and on the basis of past developments in the history of physics, which he believes justifies his hidden-variables ontology.

Bohm's third objective is to use the hidden-variable interpretation as a guide for future research for a new microphysical theory that will resolve what he sees as the current crisis in quantum physics. This crisis manifests itself in Dirac's relativistic quantum theory, when the wave equation is applied to the description of particle scattering with very high energies and at short distances. For the Schrödinger wave equation to be used in such applications, an *ad hoc* mathematical adjustment called renormalization is necessary. Furthermore the behavior of very high-energy particles in experiments reveals that there exist many new kinds of particles not previously known, and that they are unstable, since they decay into one another and create other particles. Nothing like this is accounted for by current quantum theory. To Bohm these problems for the current quantum theory suggest that elementary particles are not really elementary. The concept of a subquantum level justifies the physicist considering a whole range of qualitatively new kinds of theories that approach the currently accepted theory only as approximations, which hold in limiting cases. He believes that the current crisis in quantum theory portends a revolution in microphysics, and that the hidden-variable interpretation offers a superior guide for research that promises to resolve the crisis.

These three objectives of Bohm's agenda represent successively more ambitious claims. The first claim is merely that an alternative to the

Copenhagen semantical interpretation describing a subquantum level of magnitude is conceivable in the sense that it is consistent with the data and formalism of the current quantum theory. The second claim states more ambitiously that the history of physics reveals that postulating lower levels of magnitude supplies an analogy, which is a productive strategy to guide new research. The third claim is still more ambitious; it states that a new scientific revolution in microphysics is at hand, and that the hidden-variable semantical interpretation will produce a new microphysical theory that will resolve the current crisis in quantum theory. As de Broglie said in the closing sentence of his "Foreword" to *Causality and Chance* (1957), Bohm's book comes at exactly the right time. Thirty-five years later in his *Undivided Universe* Bohm was still predicting this impending revolution. No such revolution has yet occurred, but more recent experiments based on John Stuart Bell's inequality have occasioned reconsideration of the merits of Bohmian mechanics.

Bohm's Hidden-Variable Interpretation of Quantum Theory

Consider next a brief overview of the hidden-variable interpretation, Bohm's means for implementing his three-point agenda for future microphysics. Bohm's hidden-variable interpretation is the Schrödinger wave equation plus trajectories for individual particles, as in Newton's second law of motion, thus rendering both the wave and particle as real and completely causal. Measurement does not realize the particle, and there is no wave collapse to a particle. Bohm postulates that there exists a subquantum-mechanical order of magnitude containing hidden phenomena, and that the statistical character of the current quantum theory originates in random fluctuations of new kinds of entities existing at this lower subquantum-mechanical level. Thus Heisenberg's indeterminacy principle and his particular statistical treatment of it pertain only to phenomena at the quantum-mechanical level. Bohm believes that indeterminacy is a measurement problem like the measurement problems found in Newtonian mechanics, and that by broadening the context of physical theory to include a subquantum-mechanical level, it will become possible to diminish indeterminacy below the limits set by Heisenberg's indeterminacy principle. Bohm states that subquantum processes may be detectable in the domain of very high energies and very small distances, even though at lesser energies and greater distances the high degree of approximation permitted by the laws

of the quantum level means that the entities at the subquantum level cannot be playing a very significant role in quantum-level events. He postulates that associated with each electron there is a particle that has a precisely definable and continuously varying values of position and momentum, and that is so small that at the quantum-mechanical level it can be approximated as a mathematical point, just as in the earliest forms of atomic theory the atom was so described. He also postulates that associated with the particle there is a quantum-level wave that oscillates in a real subquantum field, and which satisfies the Schrödinger wave function. In his later works he also refers to the subquantum field as the quantum field. In summary Bohm says he regards the quantum-mechanical system as a synthesis of a precisely definable particle and a precisely definable subquantum field which exerts a force or potential on the particle.

Bohm uses figures of speech, which he imprecisely calls analogies, and these analogies are not merely illustrative of fully formed thoughts, but have had a self-consciously formative role in his thinking. In his *Causality and Chance* Bohm uses an analogy with Brownian movements of particles in a gravitational field, and illustrates what Heisenberg's indeterminacy principle would mean in terms of a subquantum-mechanical field. In the case of Brownian motion a smoke particle is subject to random fluctuations originating in collisions with the atoms that exist at a lower order of magnitude than the smoke particle. As a result of these random collisions the motion of the smoke particle cannot be completely determined by the position and velocity of the particle at the level of the Brownian motion itself. Bohm cites a 1933 paper by the German physicist, R. Furth, who showed that the lack of determination in Brownian motion is not only qualitatively analogous to that obtained in the quantum theory, but is also quantitatively analogous to the mathematical form of the indeterminacy relations. Thus, for a short-time interval with random fluctuations of a given magnitude in the mean position and a given magnitude in the mean momentum, the magnitudes satisfy a relationship involving a constant that depends on the state of the gas, and the relationship is mathematically analogous to Heisenberg's indeterminacy relation involving Planck's constant. The quantum-level force produces a tendency to pull the particle into regions where the subquantum-level field has its strongest intensity, as described by Born's probability distribution. But this tendency is also resisted by random motions analogous to Brownian motions, which originate at the subquantum level. The origin of these motions is not important; it is sufficient they have the property such that the average of their motions

satisfies the Schrödinger wave equation, and that they are communicated to the particle. The net effect of the quantum-level force and the subquantum-level random motions in the subquantum field is a mean distribution in a statistical ensemble of particles, which favors the regions where the quantum-level force field is most intense, but which still leaves some chance for a typical particle to spend some time in the regions where the field is relatively weak. This result is analogous to the classical Brownian motion of a particle in a gravitational field, where the random motion which tends to carry the particle into all parts of the container, is opposed by the gravitational field, which tends to pull it towards the bottom of the container.

Using these concepts Bohm proposes his alternative explanation of the two-slit experiment. When the particle passes through a slit, it follows an irregular path, because subquantum random motions affect it. After a large number of particles have passed through the slit system with both slits open, a pattern forms with particles accumulating on a screen where the subquantum field intensity is greatest due to the effects of the quantum force, as described by Born's probability distribution. The pattern is different if only one slit is open, than if both are open. Closing one of the two slits influences the particles that pass through the open slit, because it influences the quantum-level force felt by the particle as it moves between the slit system and the screen. Thus the hidden-variable interpretation can explain how the appearance of the wave-particle duality originates, while the Copenhagen interpretation requires acceptance without further discussion of the fact that electrons enter the slit system and appears at the screen with an interference pattern.

In *Causality and Chance* Bohm also comments on Heisenberg's gamma-ray microscope thought experiment. He maintains that Heisenberg's indeterminacy principle should not be regarded as expressing the impossibility of making measurements of unlimited precision. Rather it should be regarded as expressing the incomplete degree of self-determination characteristic only of entities that can be defined in the quantum-mechanical level. The subquantum-mechanical processes involving very small intervals of time and space will not be subject to the same limitations as those of the quantum-mechanical processes, and the unpredictable and uncontrollable disturbances caused by a measurement apparatus at the quantum level can either be eliminated or be controlled and corrected. Thus when the physicist measures processes at the quantum-mechanical level, the process of measurement will have the same limits on its degree of self-determination as every other process at this level. But if

the microphysical theory is generalized to include the subquantum order of magnitude, then the problem of measurement attributed to the uncertainty principle should be regarded not as an inherent limitation on the precision with which it is possible to conceive the simultaneous definition of position and momentum, but rather as merely a practical limitation, because measurement precision in violation of the uncertainty relations is conceivable.

Bohm also gives another analogy with Brownian motion. Bohm compares quantum phenomena with Brownian motion by describing the wave and particle as entities that interact in a way that is essential to their modes of being. He says that this seems plausible, because that fact that wave and particle are never found separately suggests that they are both different aspects of the some fundamentally new kind of entity, which is likely to be quite different from a simple wave or particle. Thus if Brownian motion were viewed not as a motion of particles, but as a motion of a very fine droplet of mist, then the indeterminacy of the droplets in a vapor at its critical temperature, where the distinction between liquid and gaseous states disappears, is a fluctuation in which the droplets are always forming and disappearing. This is an indeterminacy in the very existence of the droplets. Similarly at the quantum level it may be found that the very mode of existence of the electron is indeterminate. The fact that the electron shows its characteristic wave-particle duality in its behavior suggests that the particle showing this critical opalescence is the relevant concept of particle. It is unclear whether or not Bohm is attempting at this stage of his thinking in terms of hidden variables to use this alternative Brownian analogy to reconcile his original potentiality idea with his newer hidden-variables idea. In his later view in *Undivided Universe* potentiality is the presence of the information in the quantum wave, which is inactive except when the particle uses it as a guidance condition for its movement.

Bohm says that because the subquantum level is inadmissible in the Copenhagen interpretation, one is restricted to making blind mathematical manipulations with the hope that somehow one of these manipulations will lead to a new and correct theory. He says that if the subquantum level is admitted, where there are processes of very high energy and very high frequency faster than the processes taking place at the quantum-mechanical level, then the details of the lower level would become significant, and the current formulation of the quantum theory would break down. The creation of a particle such as a meson may thus be conceived as a well defined subquantum-level process, in which the field energy is concentrated in a

certain region of space in discrete amounts, while the destruction of the particle is just the opposite process. At the quantum-mechanical level the precise details of this process are not significant, and therefore can be ignored. This in fact is done in the current quantum theory, which discusses the creation and destruction of particles as merely a kind of popping in and out of existence with special creation and destruction operators in the mathematics. However, with very fast high-energy processes the results may well depend on these subquantum-mechanical details. And if this should be the case, then the current quantum theory would not be adequate for the treatment of such processes.

The original analogy used by Bohm for developing the idea of the subquantum field is a postulated similarity with the electromagnetic field. The analogy appears in his 1952 articles in *Physical Review*, and reappears often in later works. The subquantum field exerts a force on the particle in a way that is analogous to the way that the electromagnetic field exerts a force on a charge. And just as the electromagnetic field obeys Maxwell's equations, so too the subquantum field obeys Schrödinger's equation. In both cases a complete specification of the field at a given instant over every point in space determines the values of the fields for all times. And in both cases once the physicist knows the field fluxions, he can calculate the force on a particle, so that if he also knows the initial position and momentum of the particle, he can calculate its entire trajectory. Physicists are not yet able to make experiments that localize the position and momentum to a region smaller than that in which the intensity of the hidden subquantum field is applicable. Therefore Bohm notes that they cannot yet find clear-cut experimental evidence that the hypothesis of the hidden variables is necessary.

There are also noted dissimilarities from the electromagnetic wave (or negative analogies as Hesse would say). These dissimilarities are the distinctive aspects of the quantum world in contrast to the classical world. One noteworthy dissimilarity is that the Schrödinger equation is homogeneous while Maxwell's equations are inhomogeneous, with the result that unlike the electromagnetic field, the subquantum field is not radiated or absorbed, but simply changes its *form* while its intensity remains constant. In his later works Bohm says that the quantum wave does not impart energy to the particle, but instead functions as a guidance condition, while the particle moves with its own energy. This feature gives rise to Bohm's concept of active information, which he introduces in his later book, *Undivided Universe*. He describes the concept of active information by an

analogy with a radio wave which guides a ship propelled by its own much greater energy while piloted under the guidance of the radio signal. Similarly the elementary particle moves by its own energy under the guidance of the quantum wave. The quantum wave does not push or pull the particle, but rather guides it like the radio wave guides the ship. Bohm explains the two-slit interference experiment in terms of active information. If both slits are open, the quantum wave passes through both slits while the particle passes through only one slit, and the quantum wave contains information about the slits. As the particle reaches certain points in front of the slits, it is informed to accelerate or decelerate accordingly. Bohm says that the electron particle with its own energy source may have a complex and subtle inner structure, perhaps comparable to a radio receiver.

Quite notably Bohm says the fact that the action of the quantum potential upon the particle depends only on its form and not on its magnitude, implies the possibility of a strong nonlocal connection of distant particles and a strong dependence of the particle on its general environmental context. The forces between particles depend on the wave function of the whole system, so that there is what Bohm calls indivisible wholeness, reminiscent of the organic wholeness of a living being in which the very nature of each part depends on the whole. This absence of the mutual externality and separability of all elements which is characteristic of the classical world, makes the quantum world very elusive to the grasp of the physicists' instruments. But Bohm says it is real and more basic than the classical world. According to Bohm's theory the classical world's autonomy emerges wherever the quantum potential is so relatively small that it can be neglected. But the classical subworld is actually an abstraction from the subtle quantum world, which is the ultimate ground for existence. These considerations lead to Bohm's thesis of the implicate order, the order in the quantum world, which supersedes the Cartesian order of the classical world and its mathematics.

In several of his publications Bohm uses the analogy of the lens and the hologram to illustrate the implicate order in ordinary experience. The classical world is like a lens, which produces an approximate correspondence of points on an object to points on an image. In contrast the quantum world is like a hologram, in which each region of the hologram makes possible an image of the whole object. The hologram does not look like the represented object at all, but rather the image is implicit or enfolded. Bohm adds that the term enfolded is not merely a metaphor, but is to be taken literally, and that the order in the hologram is implicate. He also says

that there are algebras of the implicate order, and he exemplifies some in his *Undivided Universe*.

Bohm's most noteworthy analogy is given in the third chapter of Undivided *Universe*, where he develops the basic principles of his ontological interpretation in the context of the one-body system. He begins with what is known as the WKB approximation for the classical limit in quantum mechanics, and concludes with an equation of motion containing separate terms for both classical and quantum forces, and describing the electron as a particle that has a well defined position that varies continuously, is causally determined, and is never separated from a new type of quantum field that affects the particle. Peat, Bohm's editor, explains Bohm's development of this analogy in his biography's seventh chapter titled "Hidden Variables". Bohm later rejected Einstein's idea that the probabilistic results of quantum theory are the result of underlying deterministic motions of smaller particles, as in the Brownian motion analogy. Bohm knew that something analogous to the quantum theory's wave-particle ambiguity already existed in classical physics. In the nineteenth century the Irish mathematician W.R. Hamilton had shown that it is mathematically possible to recast Newton's laws about the movement of particles into a description involving waves. Bohm also knew that Hamilton's approach is used in quantum theory as an approximation which simplifies calculations, the WKB approximation, which Peat says has a position midway between classical and quantum mechanics with its assumption that quantum particles move along actual trajectories. Unlike most physicists, Bohm took the WKB approximation realistically instead of instrumentally, i.e. as merely a convenient approximation. Peat reports that Bohm's strategy was to ask what would have to be added to Hamilton's approach in order to transform this mathematical approximation technique into an equation that can reproduce all the results of quantum theory exactly, and that Bohm's answer was to introduce his radically new quantum potential, in order to explain all the nonclassical effects. Peat reports that Bohm thus dispensed with metaphysical ideas like Heisenbergian potentialities and actualities, collapsing wave functions, and irreducible probabilities.

Bohm's Critique of Heisenberg's Copenhagen Interpretation

Shortly after the publication of Heisenberg's *Physics and Philosophy: The Revolution in Modern Science* (1958) Bohm wrote an article in *The British Journal for the Philosophy of Science* (February, 1962) titled "Classical and Nonclassical Concepts in the Quantum Theory." In a footnote Bohm comments that his article was originally planned as a review of *Physics and Philosophy*, but since he and Heisenberg had on previous occasions criticized one another's views, Bohm decided to subtitle his article "An Answer to Heisenberg's *Physics and Philosophy*." On the first page of his article Bohm says that since Heisenberg's book presents the basic features of the Copenhagen interpretation in such a clear light that it constitutes a useful basis on which further criticisms can be developed. And in this paper Bohm sets forth his own criticisms, one ontological and the other semantical. In summary Bohm's ontological criticism is that in the exposition of his Copenhagen interpretation Heisenberg introduces ideas that are subjectivist and inconsistent. Bohm believes that in expounding his doctrine of *potentia* Heisenberg states that whereas possibilities can exist outside the human mind, physical actuality can only exist when someone perceives it. To assess Bohm's criticism, it is necessary firstly to examine Heisenberg's own statements about the role of subjectivism in quantum theory.

Heisenberg's version of the Copenhagen interpretation is set forth in the third chapter of his *Physics and Philosophy*, which is titled "The Copenhagen Interpretation of Quantum Theory." He begins by comparing experiments in classical and quantum physics. In both types of experiments there are errors of measurement observation, which can be described by probability functions. The error is not a property of the observed system, but is the experimenter's ignorance or lack of knowledge of the true measurement. Thus Heisenberg invokes a subjective interpretation of the probability function describing measurement error. He then states several times in his exposition that in the case of a quantum experiment the probability function combines both objective and subjective elements in the experimental situation, or as he also says, it represents both statements of a fact and statements of our knowledge of the fact. The statement of our incomplete knowledge of the fact is the measurement error, which is subjective, and it may be different for different experimenters, presumably because the different experimenters do not make exactly the same errors when making their measurements. And he comments that the subjective

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element in the probability function may be practically negligible as compared with the objective element, and the physicist can then speak of a pure case. The statement of fact is a statement about possibilities or tendencies, and he references Aristotle's concept of *potentia*. The *potentia* or potential is completely objective and does not depend on any observer. The realization of the potential, the transition from the possible to the actual, takes place during the act of observation, as soon as the of object interacts with the measuring device. Heisenberg explicitly issues some caveats, namely that this transition applies to the physical and not to the psychical act of observation, that it is not connected with the act of registration of the result by the mind of the observer, and that quantum theory does not contain genuinely subjective features, because it does not introduce the mind of the physicist as a part of the atomic event. These comments would suggest that Heisenberg wishes to preclude any metaphysical idealism such as Berkeley's *esse est percipi*. But Bohm argues that these caveats are inconsistent with Heisenberg's preceding statements about subjectivism in these passages.

While denying that the transition from the possible to the actual in the measurement operation is connected with the act of registration of the measurement result by the mind of the observer, Heisenberg states that the discontinuous change in the probability function due to the second measurement takes place with the act of registration, because it is a discontinuous change of our knowledge in the instant of registration, a change that has its image in the discontinuous change in the probability function. Bohm quotes this passage in his article, and he concludes that until an observer actually perceives the result of observation, so that he can write a new wave function representing the actual state to which the previous possibilities have collapsed as a result of his perception, there is no actuality at all as far as anything that can appear in the theory is concerned, but only the set of possibilities. Bohm illustrates his view of Heisenberg's subjectivism with a hypothetical experiment involving a set of Geiger counters arranged in a grid and toward which a free electron is directed. He supposes a point in time at which the electron has already entered the grid system and has triggered off one of the counters, and furthermore supposes that no observer has yet looked to see which counter has been triggered. Bohm says that on Heisenberg's view, at the supposed point in time when no observer has seen which counter has been triggered, one knows the objective possibilities, namely that the counter in question must be one of those located where the amplitude of the electron wave function is appreciable; but if one tries to describe the physical actuality of which counter has been

triggered, there is no way in the theory to do so, because the probability function describes only psychic actualities. In other words until an observer actually perceives which counter has operated, so that he can write a new wave function representing the actual state to which the previous possibilities have collapsed as a result of his perception, there is no actuality described by the theory but only the set of possibilities. Thus the physical actualities play no part at all in the theory, because no predicted result would be changed if the theory were developed without mentioning the physical actualities. It is noteworthy that Bohm seems not merely to be saying that the Copenhagen interpretation is subjectivist because it is probabilistic; he is not merely criticizing Heisenberg's Copenhagen interpretation by assuming the subjectivist interpretation of probability as the only probability interpretation. Bohm's criticism is the claim that the subjectivism is in Heisenberg's Copenhagen interpretation, and that Heisenberg's argument unintentionally but logically implies that to be is to be perceived by the registering mind of the observer. In fact this is not Heisenberg's view. Heisenberg's thesis is that to be is to be produced by the disturbing physical apparatus used by the observer. Thus Bohm's thought experiment involving the grid of Geiger counters demonstrates only that there may be a time interval between production and perception of the new actuality.

The confusion seems to arise from Heisenberg's decision to give the quantum probability function both a subjective and an objective interpretation. Normally these two interpretations are distinguished as alternatives, and for good reason: On the objective interpretation the probability function is a statement in the object language like any other theory in physics with a semantics describing the real physical world. Thus interpreted the probability function is an object language statement with a semantics describing the *potentia* ontology and the ontology of indeterminism. On the subjective interpretation the probability function is a statement in a metalanguage for physics with a semantics describing the physicist's state of knowledge or ignorance expressed by the object language, and it consists of statements making statistical estimates of measurement error. Heisenberg chose to combine these two interpretations, presumably because in quantum theory it is not possible to write a separate probability function for the measurement error. In classical physics, which traditionally assumes an ontology of determinism, variations in repeated measurements under the same experimental conditions are assumed to be due entirely to randomly distributed measurement errors. These could be represented statistically by the standard deviation about the calculated mean

of the measurement values, also known as the standard error of the estimate of the true measurement value, or by a probability function based on a normalization inversely related to the deviations from the mean. In quantum theory, on the other hand, the indeterminist ontology introduces a random variation not originating in measurement errors even though the indeterminacy operates in the measurement process. Therefore, the two sources of variation are inseparable, and the effects of both are taken up in the probability function of quantum theory. What is noteworthy is that Heisenberg does not state in his exposition that the discontinuity in the probability function is due to measurement errors occurring in the second measurement. He says it is a discontinuous change in our knowledge in the instant of registration that has its image in the discontinuous change of the probability function, and that is occasioned by the disturbance produced by measurement process. Thus the objective interpretation would seem to be the operative one in this passage, because the probability function is viewed as constituting the experimenter's knowledge rather than describing that knowledge. The knowledge that Heisenberg says is the image of the new probability function is the semantics that describes the new physical actuality realized by the action of the measurement apparatus.

However, Bohm prefers to construe this passage to mean that the probability function should be taken with a subjective interpretation, and that it describes knowledge, which Heisenberg calls psychical instead of physical. This places the quantum theory entirely in the metalanguage for physics. Thus Bohm says that the physical actualities play no part whatsoever in the theory, since no predicted result would be changed in any way at all, if the theory were developed without mentioning them. Then Bohm says that to avoid subjectivism, Heisenberg adopts the completely metaphysical assumption of physical entities, which play no part in the theory, but which are introduced to avoid what would otherwise be an untenable philosophical position.

Having exposed to his satisfaction the inconsistency in the Copenhagen interpretation, namely the subjectivism he believes implied in Heisenberg's exposition and contradicted by Heisenberg's *ad hoc* attempt to introduce physical actuality, Bohm then goes on to say that it was due to this problem that he himself was led to criticize the Copenhagen interpretation years earlier, and that while trying to find a way to remedy the absence of the actuality function, he developed his own alternative interpretation. In his alternative interpretation, namely the hidden-variable interpretation, he proposes in addition to the Schrödinger wave function the existence of a

particle having a well defined position and momentum, which interacts with the wave in a certain prescribed manner. The position of this particle plays the part of an actuality function in the sense that, when the wave function spreads out over many possibilities, this particle function determines which of these possibilities is actually present.

Consider next Bohm's semantical critique of Heisenberg's version of the Copenhagen interpretation. His semantical critique is distinctive, because it exploits Heisenberg's distinction between on the one hand everyday concepts and on the other hand the Newtonian concepts of classical physics that are said to be refinements of the everyday concepts. Bohm rejects the Copenhagen thesis that the classical concepts are necessary for describing macrophysical objects such as the equipment used in microphysical experiments, and thus maintains that alternatives to the Copenhagen interpretation are conceptually possible. He maintains contrary to Heisenberg that the everyday concepts actually used in ordinary experience including the physicist's description of his laboratory equipment may be refined to topological concepts, and need not be the Cartesian-coordinate concepts of Newtonian physics. He therefore believes that the topological concepts are more fundamental in the mathematical sense for the description of space and time than the Cartesian concepts, and that the latter must be translated from the former for Newtonian physics.

Bohm exemplifies this idea with the problem of location an ordinary pencil. The location is not ordinarily stated in terms of a coordinate system such as latitude and longitude. What is actually done in ordinary experience is to locate the pencil as laying upon a desk within a certain room in a certain house, which is located on a certain street, etc. Thus the pencil is located with the aid of a series of topological relations, in which one entity is within or upon another entity. He then says that the laboratory physicist also uses topological relations in his work. In no experiment does he ever locate anything by giving an exact coordinate, which is to say an infinite number of decimals. Rather what he does in practice for making a measurement is to place a pointer between certain marks on a scale, thus locating it by the topological relation of between. In every experiment the notion of precisely defined coordinates is just an abstraction, which is approximated when a topologically described experimental result is translated into the Cartesian language of continuous coordinates. Bohm adds that everyday concepts could be refined in other ways than to topological concepts, but that for description of space and time, the topological concepts are most appropriate for physical theory.

Bohm then suggests a topological formulation of the quantum theory, and says that these nonclassical concepts make possible new kinds of experimental predictions, which cannot be considered in the framework of the Copenhagen interpretation, and which according to Heisenberg's conclusions are not possible. Bohm says that there is a remarkable analogy between the mathematics of topology and that of the modern quantum mechanical field theory, and that utilization of this analogy can make possible the development of a topological formulation, which while leading to the results of the usual quantum theory in suitable limiting cases, nevertheless possesses certain genuinely novel features with regard both to its mathematical formalism and to its experimental predictions. He also says that he cannot go into the details in this paper, and he is not known to have done so in any other paper he has published. This novation with or without the inspiring analogy is a promissory note backed by an as-yet-unearned income, because Bohm does not set forth an explicit topological formulation of the quantum theory. If he actually had set forth such a new formulation, and if its novel experimental predictions were found to be superior to those made by the current quantum theory, such as resolving the renormalization problem, then his new topological formulation would be a revolutionary development in microphysical theory, and much more than merely a new interpretation of the quantum theory.

Bohm and Bell on the EPR Experiment and Nonlocality

In 1935 Einstein, Podolsky and Rosen (conventionally abbreviated as "EPR") published an article in the *Physical Review* titled "Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?" Their negative answer implies that the current statistical quantum theory is inadequate, and that further development is needed that would presumably involve identifying presently unknown factors conventionally referred to as hidden variables. The authors firstly set forth a necessary condition for completeness, according to which every element of the physical reality must have a counterpart in the physical theory. And they secondly set forth a sufficient ontological condition for affirming the reality of a physical quantity, which consists in the possibility of predicting with certainty the physical quantity under investigation without disturbing the physical system. The three authors propose a hypothetical or *gedanken* experiment, now conventionally known as the "EPR experiment", which assumes among

other things the experimental correctness of the quantum theory and Heisenberg's indeterminacy relations, but which concludes to a demonstration of the present quantum theory's incompleteness.

There are several equivalent versions of this now famous experiment including some that have since actually been performed. The authors postulate two particles initially interacting, such that their properties are correlated, and then subsequently separated spatially by being sent off in opposite directions, so that they can no longer interact but still retain their correlated properties. One of the implicit assumptions of the argument is that there is no instantaneous action at a distance, so that the spatial separation of the two particles precludes the measurement of one particle from disturbing the other particle in any way. This assumption has been called either separability or locality. In this thought experiment the noteworthy properties are the noncommuting observables, position and momentum. If the *momentum* of one of the particles is measured, then since its momentum is correlated to the momentum of the second particle, the momentum of the second is also known by the measurement of the first. Or if the *position* of the first particle is measured, then since its position is correlated to the position of the second particle, the position of the second is also known by the measurement of the first. But according to Heisenberg's indeterminacy relations no quantum wave/particle can simultaneously have both position and momentum as determinate properties The selection of which quantity is determinate is made by the measurement action, a selection which is the free and arbitrary choice of the experimenter. The second particle has no interaction with the first at the time that the first particle is measured, so the second particle cannot know, as it were, which of the noncommuting properties the experimenter selected as the determinate property of the first particle. Yet paradoxically the second particle's determinate property is always correlated to that of the first. The authors, Einstein, Podolsky and Rosen, conclude that the paradox can only be resolved by recognizing that in fact both particles always had both determinate position and determinate momentum from the time of their separation, and that the current quantum theory fails to represent the physical reality of the situation completely. The current quantum theory, in other words, is incomplete.

Bohr responded to this argument in an article with the same title appearing in a later issue of the same journal in the same year. He takes issue with EPR's criterion for physical reality, reaffirms his principle of complementarity, and maintains contrary to EPR that quantum theory is not

incomplete. He says that because it is impossible to control the reaction of the object on the measuring instruments, the interaction between object and measuring devices conditioned by the very existence of the quantum of action entails the necessity of a final renunciation of the classical ideal of causality and a radical revision of our attitude towards the problem of physical reality. Bohr discusses this aspect of measurement in the context of the two-slit experiment of electron diffraction, which is not a hypothetical experiment but an actual one. He references Heisenberg's uncertainty principle, and says that the uncertainty of momentum of the incident particle is inseparably connected with an exchange of momentum between the particle and the diaphragm. This impossibility of a closer analysis of the reactions between the particle and the measuring instrument is an essential property of any arrangement where there is a feature of individuality completely foreign to classical physics. Any attempt to take into account the momentum exchanged between the particle and the separate parts of the apparatus, would imply conclusions about the course of such phenomena, such as what particular slit the particle passes on its way to the photographic plate. This would be quite incompatible with the fact that the probability of the particle reaching a given place on the photographic plate is determined not by the presence of any particular slit, but by the position of all the slits. Bohr explains that complementarity is due to this impossibility in the field of quantum theory of accurately controlling the reaction of the object on the measuring instrument, i.e. the transfer of momentum in the case of position measurements and the displacement in the case of momentum measurements. And he concludes that in such cases the physicist is not dealing with an incomplete description characterized by the arbitrary picking out of different elements of physical reality at the cost of sacrificing other such elements, but with a rational discrimination between essentially different experimental arrangements which are suited either for an unambiguous use of the idea of space location or for the legitimate application of the conservation laws of momentum. There is nothing in this rebuttal by Bohr that was not previously known to physicists and to EPR at the time of their famous paper, and Bohr's arguments cannot be said to have been responsive to the particulars of EPR's thought experiment.

In a section titled "The Paradox of Einstein, Podolsky and Rosen" in his *Quantum Theory* Bohm says that the EPR criticism of quantum theory has been shown to be unjustified, and in a footnote to this statement he references Bohr's critique of EPR published in *Physical Review*. At this time Bohm was sympathetic to the Copenhagen interpretation, and critical of

Einstein's views. In addition to EPR's necessary condition for a complete physical theory and their sufficient condition for recognizing an element of reality, Bohm says that there are two additional assumptions implicit in the EPR argument. These assumptions are firstly that the world can be correctly analyzed in terms of distinct and separately existing elements of reality, and secondly that every one of these elements must be a counterpart of a precisely defined mathematical quantity appearing in a complete theory. Bohm attacks these two implicit assumptions. He says that the one-to-one correspondence between mathematical theory and well defined elements of reality exist only at the classical level. At the quantum level, on the other hand, the properties described by the wave function are not well defined properties, but are only *potentialities* which are more definitely realized in interaction with an appropriate classical system such as a measuring apparatus.

For his own critique of EPR, Bohm offers a modified but equivalent version of the EPR experiment for his analysis. His version considers the spin of the two separated and correlated particles. The second particle's spin is always correlated to the measurement axis, i.e. the spin component, chosen for measurement of the first particle, regardless of the component selected by the experimenter for measurement. On the EPR interpretation precisely defined elements of reality must therefore exist in the second particle corresponding to the simultaneous definition of all three dimensional components of spin. And since the Schrödinger wave function can specify at most only one of these components at a time with precision, it cannot provide a complete description of all elements of reality existing in the second particle. But Bohm maintains that the wave function provides the most complete description of physical reality consistent with the actual structure of matter, because on his view *no* component of spin of a given variable exists with a precisely defined value until interaction with the measuring apparatus has taken place. As soon as the first particle interacts with the measuring apparatus a given spin component is determined. As a result the definite phase relations between the wave functions of the two particles are destroyed, and the wave function of the other particle will take a form that guarantees the development of the opposite value of spin, if the second particle interacts with an apparatus measuring the same component of spin. Bohm therefore says that wave function describes the propagation of correlated potentialities.

Bohm's proposed resolution to the EPR paradox involving his rejection of the two implicit assumptions he believed contained in the EPR

argument resulted in his ontological thesis of potentialities, his wholistic philosophy of nature, and his belief that mathematics is of limited value for physics. Contrary to Einstein's ontology, Bohm maintains the wholistic view that there are no distinct and separately existing elements of reality, and that the present form of the quantum theory implies that the world cannot be put into one-to-one correspondence with any conceivable kind of precisely defined mathematical quantities. Therefore a complete theory will always require concepts that are more general than those for an analysis into precisely defined elements. Thus to obtain a description of *all* aspects of the world, one must supplement the mathematical description with a physical interpretation in terms of incompletely defined potentialities. He later refers to such supplementary description as informal language. Bohm's conclusion that mathematical physics must be supplemented with informal nonmathematical discourse, may be contrasted with the approach of Dirac, who never doubted the adequacy of mathematics for physics, and who instead admitted a new type of variable into mathematical physics, namely the quantum or Q variables, as he called them, as opposed to the traditionally classical or C variables. Finally to cope mathematically with the indeterminacies in microphysics Bohm introduces in his *Undivided Universe* his thesis that quantum theory is an implicate algebra.

In his early statement of his hidden-variable thesis published in *Physical Review* in 1952 Bohm revised his view of Bohr's thesis. He says that Bohr's interpretation of the quantum theory leaves unexplained the correlations between the two separated particles in the EPR experiment, and that the quantum theory needs to be completed by additional elements or parameters. This is the hidden-variables thesis, but there is no mention of potentiality in noncommuting variables or ontological wholism, although there is recognition of the nonlocality implication in his new thesis, and Bohm seems to have been one of the first to recognize it. He states that on his new interpretation, the EPR experiment is describable in terms of a combination of a six-dimensional wave field, the subquantum field, and a precisely definable trajectory in a six-dimensional space. Thus when the experimenter measures either the position or the momentum of the first particle, he introduces uncontrollable fluctuations in the wave function for the entire system, which through the quantum-mechanical forces bring about corresponding uncontrollable fluctuations in the position or momentum respectively of the other particle. And he notes that these quantum-mechanical forces transmit the disturbances *instantaneously* from one particle to the other through the medium of the subquantum field. But Bohm

does not conclude that the instantaneously transmitted disturbances involve signals having velocities greater than that of light. He says that where the quantum theory is correct, his interpretation cannot lead to inconsistencies with relativity theory, and that where the quantum theory may break down in cases of high velocities and short distances, Lorentz invariance may serve as a heuristic principle in the search for new physical laws.

Before examining Bohm's later statements in his *Undivided Universe*, consider firstly Bell's locality inequality and actual EPR experiments. John Stewart Bell (1928-1990), a theoretical physicist associated with CERN in Geneva, Switzerland, is an advocate of the hidden-variable interpretation of the quantum theory, who further developed Bohm's analysis of the EPR experiment. In 1987 Bell published his collected papers under the title *Speakable and Unspeakable in Quantum Mechanics*, in which each chapter is a previously published paper. In the chapter titled "Six Possible Worlds of Quantum Mechanics" (1968) Bell distinguishes six interpretations of the quantum theory, which he divides into the romantic and the unromantic views. The romantic views are those that are principally of interest to journalists, and the unromantic ones are those of interest to professional physicists. The three romantic views are 1) Bohr's complementarity thesis, 2) the mentalistic views of Wigner and Wheeler, and 3) the many-worlds thesis of Everett. The three unromantic views are 1) the pragmatic view that is the philosophy of physicists who work with the quantum theory, 2) a new and not-yet developed classical nonlinear Schrödinger wave equation that makes microscopic and macroscopic physics continuous, and 3) the pilot wave of de Broglie and Bohm. This last alternative, which is the hidden-variable interpretation, makes the whole physical universe classical, and the probability outcome is due entirely to the experimenter's limited control over the initial conditions. Bell says that the pilot wave thesis seems so natural and simple for resolving the wave-particle dilemma that it is a great mystery to him why it had been ignored.

In a chapter titled "Introduction to the Hidden-Variable Question" (1971) Bell discusses his motivations for defending and developing the hidden-variable thesis. His first reason, and the one that he finds most compelling, is the possibility of a homogeneous account of the physical world, which is to say, a single uniform ontology for microphysical and macrophysical domains based on classical concepts. Bell denies that there is a boundary between classical and quantum worlds, the boundary that Heisenberg had called the schism in physics, and Bell agrees with Einstein that the wave function is an incomplete and provisional microphysical

theory. His second motivation concerns the statistical character of quantum mechanical predictions. Once the incompleteness of the wave function is suspected, then the seemingly random statistical fluctuations may be viewed as determined by the extra hidden variables, which are hidden because at the present time physicists can only conjecture their existence. His third motivation is the peculiar character of some quantum-mechanical predictions considered in the famous *gedanken* experiment formulated by EPR, and a refinement proposed by Bohm in 1951, in which Stern-Gerlach magnets are used to measure selected components of spin revealed by the deflections of particles moving simultaneously away from each other in opposite trajectories from a source. The experiment permits the observer to know in advance the result of measuring one particle's deflection by observing the other's deflection even at great distance. The implication intended by Einstein is that the outcomes of such measurements are actually determined in advance by variables over which the physicist has no control, but which are sufficiently revealed by the first measurement that he can anticipate the result of the second. Therefore, contrary to the Copenhagen view there is no need to regard the performance of one measurement as a causal influence on the result of the second distant measurement, and the situation can be described as local.

Heisenberg's indeterminacy principle says no quantum-mechanical state can be dispersion free for every variable. On the other hand the hidden-variable theory says that all observations are fully determined, such that each quantum-mechanical state must correspond to an ensemble of states each with different values of the hidden variables with the component states dispersion free. Therefore, one way to formulate the hidden-variable problem is to search for a formalism permitting such dispersion-free states. Bell proposes such a formalism, a modification of the Schrödinger wave function with a set of hidden variables added, which he says provides an explicit causal mechanism by which operations on one of the two measuring devices in the apparatus can influence the response of the other distant device. However, this revision establishes that the measurement does not reveal some property previously possessed by the quantum system, but rather reveals something that comes into being in the combination of system and apparatus. It is local in configuration space, but nonlocal in ordinary three-dimensional space thus providing an explicit causal mechanism by which one of the two measuring devices in the EPR experiment can influence the response of the distant device. This is the opposite of the resolution hoped for by EPR, who had envisaged that the first device could

serve only to reveal the character of the information already stored in space and propagating in an undisturbed way toward the other detecting equipment.

In a paper titled "On the Einstein-Podolsky-Rosen Paradox" (1964) Bell set forth his locality inequality, the theoretical accomplishment for which he is best known. This probabilistic expression assumes in agreement with EPR that the separated particles and thus their measured properties are statistically independent of one another. The noteworthy consequence is that the values admitted by the inequality are inconsistent with the quantum theory. Bell thus concludes that no local deterministic hidden-variable theory can reproduce all the experimental predictions of the quantum theory. Several years after Bell's 1964 paper physicists began to design and perform actual EPR experiments to test Bell's locality inequality. The first proposed EPR experimental design was published under the title "Proposed Experiment to Test Local Hidden-Variable Theories" in *Physical Review Letters* by J.F. Clauser, M.A. Horne, A. Shimony, and R.A. Holt. These experiments examined the statistical behavior of separated photons with polarization analyzers. The most reliable experiments of the several actually performed have outcomes favoring quantum mechanics, thus violating Bell's locality inequality.

In his "Metaphysical Problems in the Foundations of Quantum Mechanics" in the *International Philosophical Quarterly* (1978) one of these experimenters, Abner Shimony, affirms a realistic interpretation based on the idea that the measurement produces a transition from potentiality to an actuality in both the separated photons. Echoing Bohm's early explanation Shimony adds that the only changes that have occurred concerning the second photon are a transition from indefiniteness of certain dynamical variables to definiteness, and not from one definite value to another. He concludes that there seems to be no way of utilizing quantum nonseparability and action at a distance for the purpose of sending a message faster than the velocity of light. He prefers the idea of wormholes previously proposed by J. A. Wheeler in 1962. Shimony describes wormholes as topological modifications of space-time whereby two points are close to each other by one route and remote by another. Thus the two photons in the EPR experiment are not only distantly separated as ordinary observation shows, but may also be more closely connected through a wormhole.

In "Bertlmann's Socks and the Nature of Reality" (1981) Bell considers four possible positions in connection with nonlocality. The first is

that Einstein was correct in rejecting action at a distance, because the apparatus in any EPR experiment attempted to date is too inefficient to offer conclusive results. But Bell says that the experimental evidence is not encouraging for such a view. The second position is that the physicist's selection of dynamical variables is not truly an independent variable in the EPR experiment, because the mind of the experimenter influences the test outcome. Bell seems unsympathetic to this position. He merely comments that this way of arranging quantum mechanical correlations would be even more mind boggling than one in which causal chains go faster than the speed of light, and that it implies that separate parts of the world are deeply entangled including our apparent free will. A third position that he considers is Bohr's view that there does not exist any reality below some classical or macroscopic level. He says that on Bohr's thesis fundamental physical theory would be fundamentally vague until concepts like macroscopic are made sharper than they are currently. And in an "Appendix" to this article Bell adds that he does not understand the meaning of such statements in Bohr's 1935 rebuttal to EPR. Clearly Bell's polite and reserved response is not intended as a confession of his ignorance, but rather as a criticism of Bohr's obscurantism.

Finally Bell considers the position that causal influences do in fact travel faster than light, and this is the position he prefers. In "Speakable and Unspeakable in Quantum Mechanics" (1984) he says that the problem of quantum theory is not how the world can be divided into the speakable macrophysical apparatus, which we can talk about, and the unspeakable quantum system, which we cannot talk about. The problem is to explain how the consequences of events at one place propagate to other places faster than light, which is in gross violation of relativistic causality. Most notably he says that Aspect, Dalibard, and Roger, who published the findings from their EPR experiments in 1982, have realized specific quantum phenomena which require such superluminal explanation in the laboratory. Bell concludes that there exists an apparent incompatibility at the deepest level between the two fundamental pillars of contemporary physical theory, and that a real synthesis of quantum and classical theories requires not just technical developments but a radical conceptual renewal.

Consider next Bohm's final statements of his views on nonlocality in his *Undivided Universe* (1993). Bohm had affirmed the nonlocality thesis even before he adopted the hidden-variable interpretation, and nonlocality remained a basic feature of his mature view. While nonlocality and wholeness are often associated with Bohr's Copenhagen interpretation, and

are opposed to EPR's criticism, Bohm's ideas of nonlocality and wholeness are not the same as Bohr's. On Bohr's view an attempt to analyze a quantum process in detail is not possible, because the experimental conditions and measurement of the experimental results are a whole that is not further analyzable. Bohm on the other hand not only proposes his hidden-variable interpretation as an analysis of the individual quantum phenomenon, but he also offers a philosophically sophisticated critique of Bohr's rebuttal to EPR in the seventh chapter titled "Nonlocality". Bohm replies that on Bohr's view it is not possible even to talk about nonlocality, because nothing can be said about the detailed behavior of individual systems at the quantum order of magnitude. In his critique Bohm attacks Bohr's philosophy of language, according to which physical phenomena must be described with concepts from classical physics. Bohm references Einstein's statements that concepts are a free creation of the human mind, and says that there is no problem in assuming the simultaneous reality of all properties of the separated particles in the EPR experiment, even though these properties cannot be simultaneously observed. Contemporary philosophers of science refer to these different semantical views expressed by Bohr and Einstein and discussed by Bohm as the naturalistic and the artifactual theses of the semantics of language respectively. Notwithstanding Bohm's minority status among physicists, his philosophy of language is as sophisticated as may be found in the views of any contemporary academic philosopher of science.

Bohm's adoption of the hidden-variable interpretation led him to modify his original explanation of nonlocality. In his *Undivided Universe* he says that the nonlocal connection between the separated particles which causes the correlation in the EPR experiment is the quantum potential in the subquantum field. And he also maintains that the nonlocal quantum potential cannot be used to carry a signal. By signal he means a controllable influence, and he says that there is no way to control the behavior of the remote second particle by anything that might be done to the first particle. This is because any attempt to send a signal by influencing one of the pair of particles under EPR correlations will encounter difficulties arising from the irreducibly participatory nature of all quantum processes due to their wholistic nature. To clarify his view on signals, he says that if an attempt were made in some way to modulate the wave function in a way similar to what is done to make a radio wave signal, the whole pattern of this quantum wave would change radically in a chaotic and complex way, because it is so fragile.

Bohm takes up the relation between nonlocality and special relativity in "On the Relativistic Invariance of Our Ontological Interpretation", the twelfth chapter of *Undivided Universe*. He says that since a particle guided in a nonlocal way is not Lorentz invariant, physicists must either accept nonlocality, in which case relativity is not fully adequate in the quantum domain, or they must reject nonlocality, in which case quantum theory is not fully adequate in the relativistic domain. Bohm does not renounce nonlocality, but instead concludes that physicists must assume the existence of a unique frame in which the nonlocal connections are instantaneous. He says that he does not regard this unique frame to be intrinsically unobservable, but that these new properties cannot be observed presently in the statistical and manifest domains in which the current quantum theory and relativity theory are valid. Just as the observations of atoms became possible where continuity of matter broke down, so the observation of the new properties will become possible where quantum theory and relativity theory break down. He says that the idea of a unique frame fits in with an important historical tradition regarding the way in which new levels of reality, e.g. the atoms, are introduced into physics to explain older levels, e.g. continuous matter, on a qualitatively new basis. Bohm admits it will take time to demonstrate experimentally the existence of the subquantum fields and the unique frame of reference implied by nonlocality. He also considers that the speed of the quantum connection is not actually instantaneous, but is nonetheless much faster than the speed of light, and he proposes the development of the EPR experiment reminiscent of the Michelson-Morley experiment to measure the superluminary velocity of the quantum connection between distant particles. He says such a test might demonstrate the existence of the unique frame, indicate a failure of both quantum and relativity theories, eliminate quantum nonlocality, and indicate a deeper level of reality in which the basic laws are neither those of quantum theory nor relativity theory.

The new EPR experiments using Bell's locality inequality are empirical developments that have supplied ample grist for the philosophy dissertation mills. Nonetheless, their interesting findings are of greater significance to physicists than to philosophers of science. They may be the Michelson-Morley experiment for the contemporary physicists' theory of relativity, but they present no anomalies for the contemporary philosopher's Pragmatist philosophy of science. The modern quantum theory brought down the Positivist philosophy by occasioning the rejection of the naturalistic thesis of the semantics of descriptive language including notably

those terms that the Positivists called observation terms. This was analogous to rejecting the parallel postulate in Euclidian geometry, and it brought in its train the development of the contemporary Pragmatist philosophy of science based on the thesis that the semantics of descriptive language is artifactual. Following this development in philosophy of science, however, the new EPR experiments have not warranted any revision to the contemporary Pragmatist philosophy of science. For the contemporary Pragmatist, the EPR experimental findings may be viewed as business as usual for science. In view of Bell's sympathy for Bohm's hidden-variable thesis, it is ironic that the experiments performed using Bell's inequality have yielded findings that contradict the expectations of Einstein, Podolsky and Rosen.

Bohm on Perception and Metaphor in Scientific Discovery

For forty years following his initial 1952 statement of his hidden-variable interpretation Bohm continued to expound his views in philosophy of science, metaphysics, and epistemology. His statements that are most relevant to the subject of scientific discovery are found in *Science, Order and Creativity*, particularly in the introductory chapter and in the two succeeding chapters, which altogether take up about half of the book. There he also sets forth his philosophy of perception, which is explicitly opposed to that of the Logical Positivists, and is characteristic of contemporary post-Positivist philosophy of science. It also reveals some influence from Einstein, because he says perception takes place in the mind and in terms of theories. For example the observational data obtained by Archimedes in his bath had little value in themselves. What was significant was their meaning as perceived through the mind in an act of creative imagination. The principal historical change that has occurred in modern science is that this mental perception is more mediated through elaborate instruments that have been constructed on the basis of theories. Bohm's philosophy of perception is central to his views on scientific discovery and he assigns a special role for metaphor.

Bohm believes that the development of science is now obstructed by fragmentation that is caused by subliminal rigidities in thought that he calls the tacit infrastructure of scientific ideas. One example of the tacit infrastructure of scientific ideas is the Newtonian notions of space and time that led Lorentz to preserve both the idea of the constancy of the velocity of light and the ideas of absolute space and time by explaining the anomalous

results of the measurements of light by postulating changes in the measuring apparatus as the apparatus moves through the ether. He notes that the tendency of the scientist's mind to hold to what is familiar is reinforced by the fact that the overall tacit infrastructure is interwoven in the institutions on which depends the professional security of the scientist. The means for breaking out of the tacit infrastructure of scientific ideas and to create new theories is metaphor. Bohm defines metaphor as the simultaneous equating and negating of two concepts. Metaphor is especially important for Bohm, since he maintained that microphysics and macrophysics should have the same basic ontology, such that features from the latter domain projected into the former enables a discovery strategy. This role of metaphor in discovery is possible because the realm of physics is now that of perception through the mind, and theory dominates experiment in the development of the scientific perception of nature. Bohm says that metaphor occasions creative perception, and he also refers to metaphoric perception. Metaphoric perception brings together previously incompatible ideas in radically new ways. He says that the unfolding of a metaphor that equates different and even semantically incommensurable concepts can be very fruitful. In using the term incommensurable Bohm references Kuhn, and he equates his own thesis of the tacit infrastructure of scientific ideas with Kuhn's thesis of scientific paradigm. A paradigm is not just the articulate theory, but also the scientist's whole way of working, thinking, communicating, and perceiving with the mind. However, Bohm rejects Kuhn's thesis that normal science is without any creativity, and that revolution is completely discontinuous. Bohm maintains that semantic incommensurability can be overcome with metaphor. He furthermore says that revolution occurs when a new metaphor is developed, and normal science is the creative unfolding of that new metaphor. In Bohm's view there is much more creativity in normal science, than Kuhn admits. Bohm also criticizes Popper's thesis of falsifiability. He maintains that today an excessive emphasis is being placed on falsifiability in the sense that unless a theory can immediately or very shortly be falsified, then that theory cannot be regarded as properly scientific. A new idea with broad implications may require a long period of gestation before falsifiable consequences can be drawn from it.

Bohm also maintains that communication is essential to perception in science. He understands communication in a very broad sense to include the individual's own articulate mental dialogue with himself. The scientist engages in an inner dialogue with himself as well as with his colleagues, and in this dialogue he is disposed in his thinking by the social background.

Insights enfolded in this inner dialogue must be unfolded by discourse with colleagues and eventually by publishing. Fragmentation may proceed to the point that communication becomes blocked, because the tacit infrastructure of ideas not only limits the individual but also the whole scientific community in their creative acts of perception. Both paradigms and specialization may cause fragmentation in this way. One very central thesis of Bohm's is that a fragmentation has occurred in modern microphysics between mathematical formalism and informal discourse in microphysics. Differences in the informal discourse gave rise to an issue between Bohr and Einstein, as well as among later physicists. Bohm considers communication to be so central to perception that he speaks of perception-communication. The change in the language of physics occasioned by the development of quantum theory has led to a communication breakdown. Both Bohr and Einstein agreed on the mathematical formalism, but there is still no common informal language. Bohm believes that if Bohr and Einstein had been willing to entertain a free dialogue to eliminate the rigidities that block communication, then perhaps a new creative metaphor might have emerged for microphysics. In such a dialogue each person must be able to hold several points of view in a sort of active suspension, while treating others' views with the consideration he gives to his own. This would lead to the intellectual free play needed for a new creative metaphor.

Bohm proposes his hidden-variable interpretation for consideration in this spirit. He maintains that the interpretation of a formalism is something that is in the informal discourse, not in the measurements or the equations. This view is fundamentally contrary to Hanson's, who says the exact opposite. In Bohm's view all the available interpretations of the quantum theory, as with any other physical theory, depend fundamentally on implicit or explicit philosophical assumptions, as well as on assumptions that arise in countless other ways. The image of the hard-nosed scientist, who does not admit to the existence of the philosophical assumptions in the informal language, is just another example of the subliminal influence that is exerted on scientists by the tacit infrastructure of ideas shared by the scientific community at large.

Bohm on Mathematics and Scientific Discovery

In *Science, Order and Creativity* Bohm maintains that there is no difference between science and philosophy. While Hanson also states that

physics is natural philosophy, Bohm's statement means something very distinctive. Bohm explicitly rejects the prevailing view of the aim of physics, which he says is to produce mathematical formalisms that can correctly predict the results of experiments. He maintains that, since quantum theory and relativity theory were never understood adequately in terms of what he calls physical concepts, physics gradually slipped into the practice of talking about equations. And he states that Heisenberg gave this practice an enormous boost with the idea that science can no longer visualize atomic reality in terms of physical concepts, and with the idea that mathematics is the basic expression of our knowledge of reality. Bohm maintains that the current emphasis on mathematics has gone too far. In stating that science is the same as philosophy, Bohm means that as philosophy had traditionally done, now science must unify knowledge instead of offering physicists a fragmentation as it has today. In times past there was a general vision of the universe, of humanity, and of man's place in the whole. But specialization in modern science became narrower and led eventually to the present approach, which is fragmentary. Bohm also opposes what he sees as another wayward aim of modern physics, which is to analyze everything into independent elements that can be dealt with separately. This further contributes to fragmentation. Bohm believes that the time has come to change what is meant by science. This change is to be implemented by a creative surge that will eliminate the fragmentation.

In the fourteenth chapter of *Undivided Universe* Bohm offers a somewhat more balanced statement of the relation between physical concepts and mathematical concepts. Again he says that the prevailing attitude today is take the present mathematical formalism of quantum theory as an essential truth, and then to try to derive the physical interpretation as something that is implicit in the mathematics. He denies that his own approach is simply a return to the historically earlier view that the mathematics merely enables the physicist to talk about the physical concepts more precisely. His view is that the two types of concepts represent two extremes, and that it is necessary to be in a process of thinking that moves between these extremes in such a way that they complement one another. He says he does not regard such physical concepts as particle, quantum wave, subquantum field, position, and momentum as mere imaginative displays of the meaning of the equations. He maintains that what he is doing with his hidden-variable interpretation, is moving to the other side of the extreme in the thought process and taking the physical concepts as a guide for the development of new equations. He says that the clue for a creative

new approach may come from either side, and may flow back and forth indefinitely between them.

Bohm's Philosophy of Science

Aim of Science

Bohm's view of the aim of science contains a fundamental ambiguity. One aim is to supply a basically uniform and consistent ontology for science admitting variations at different orders of magnitude. But it does not admit to the inconsistency or pluralism that exists between quantum theory and relativity theory, which Heisenberg called the schism in physics, and which Bohm called fragmentation. This is the integrating aim that Bohm has in mind when he says that physics is philosophy. The other aim is the more conventional one in contemporary physics, the aim of producing more empirically adequate equations. Bohm maintains that these two aims of science need not and should not be divergent, even though lamentably they presently diverge. And he says that the fragmentation in contemporary physics is due to an exclusive concern with the formal language, the equations of mathematical physics.

Discovery

Bohm's philosophy of scientific discovery follows from these views on the aim of science. The fragmentation-produced divergence between these aims will be eliminated *and* both aims will be more adequately realized, if physicists attend to both the formal and the informal language, to both the mathematical and physical concepts. Employing figures of speech such as analogy and metaphor containing physical concepts will facilitate developing better equations.

Criticism

Bohm's views on scientific criticism do not lead him to invalidate the empirical adequacy of the Schrödinger wave function. Like other critics of the Copenhagen interpretation he advocates developing an alternative interpretation for the equations of the quantum theory. He never denies that the second aim of science, the production of empirically superior equations,

is realized by the equations of the quantum theory. But just as there is an ambiguity in his aim of science, so too there is a corresponding dualism in his criteria for scientific criticism. He spent most of his career attempting to persuade the physics profession that there exists another criterion that is unabashedly philosophical. That criterion is the integrated, consistent ontology for both microphysics and macrophysics. And some physicists like John Bell have been persuaded to pursue this agenda.

Explanation

Bohm does not set forth an explicit statement of his philosophy of scientific explanation. But if satisfaction of the criteria for scientific criticism is taken as yielding a scientific explanation, then Bohm's philosophy of scientific explanation follows from his views on criticism. The salient consideration in this context is the role for a uniform and consistent ontology in his integration aim of science and its associated criterion for scientific criticism.

Hanson on the Copenhagen Interpretation and Scientific Discovery

Hanson rejects all three of the objectives in Bohm's agenda for future physics. His argument against Bohm's third objective that a future hidden-variable theory will resolve the difficulties in current quantum theory, is that Bohm and other advocates of alternatives to the Copenhagen interpretation offer nothing but promises. In *Quanta and Reality* Hanson calls Bohm's proposal a congeries of excitingly vague, bold-but-largely-formless, promising-but-as-yet-unarticulated speculations. The Copenhagen interpretation on the other hand is a working theory however imperfect it may be, and a speculation is never an alternative to a working theory.

Hanson's argument against Bohm's first objective that an alternative to the Copenhagen interpretation is possible, is similar to his criticism of the third objective. Hanson denies that an alternative to the Copenhagen interpretation is possible until a new mathematical quantum theory formalism is developed, because on his thesis the Copenhagen interpretation is not a semantics supplied by related philosophical or metaphysical ideas about the subject, but rather is the semantical interpretation resulting from the logicogrammatical form of the theory's mathematical formalism. Therefore contrary to physicists such as Bohm and Lande, and contrary to

philosophers such as Feyerabend and Popper, the Copenhagen interpretation even after disengagement from what Hanson calls Bohr's naive epistemology, is not just one of several alternative semantical interpretations; it is a unique interpretation that is defined by the relationships in the mathematical formalism. In *Concept of the Positron* and elsewhere Hanson distinguishes the Copenhagen interpretation from what he calls the Bohr interpretation. He rejects efforts by philosophers such as Feyerabend to include what Feyerabend admits are the dogmatic elements of the Bohr interpretation in the Copenhagen interpretation. The dogmatic elements consist particularly in what Hanson calls Bohr's naive epistemology with its forms of perception. Perhaps it could be said with caution that with the rejection Bohr's naive epistemology Hanson's philosophy of quantum theory is one that Heisenberg might have formulated, had Heisenberg rejected Bohr's epistemological ideas which he included in his doctrine of closed-off theories, and instead followed through on Einstein's admonition that theory decides what the physicist can observe. With his rejection of the Bohr interpretation Hanson places himself in agreement with Bohm and Feyerabend, when the latter maintain that the quantum theory is not permanently valid, and he agrees that the current quantum theory may be superseded. But contrary to these authors he considers the wave-particle duality to be the defining characteristic of the Copenhagen interpretation and integral to the formalism. Because he maintains that the Copenhagen interpretation is defined by the logicogrammatical form of the mathematical formalism itself, he defends it as the only interpretation that works. He therefore says that in the absence of any algebraically detailed and experimentally adaptable alternative, the Copenhagen interpretation represents the conceptual possibilities currently open to practicing physicists, and that it will not be abandoned until it is completely replaced by an alternative, completely detailed, algebraically articulated theory.

Bohm's second objective in his agenda for future physics is that the history of physics suggests (contrary to a mechanistic thesis, as he uses that term) that the future microphysical theory will describe phenomena at the lower level of magnitude than does the current quantum theory, and that his proposal of a hidden-variable theory of the subquantum level may serve as a heuristic for future microphysics. The idea of developing a heuristic for future scientific discovery or theory development is closely related to Hanson's interest, and Hanson does not attack Bohm's second objective in terms of Bohm's antimechanistic historical thesis. But he has his own

historical thesis influencing his views on scientific discovery. His analyses are greatly influenced by the Cambrian physicist Paul A. Dirac. Dirac (1902-1984) was a theoretical physicist at Cambridge University, who shared the Nobel Memorial Prize for physics in 1933 with Schrödinger. Dirac had published a methodological statement on the future of physics in his "The Evolution of the Physicist's Picture of Nature: An account of how physical theory has developed in the past and how, in the light of this development, it can perhaps be expected to develop in the future" (*Scientific American*, May, 1963). In this brief paper Dirac contrasted the theory development approaches of Schrödinger and Heisenberg. Dirac was much more sympathetic to the former's approach, according to which the development of physical theory should be guided by the aesthetics of the mathematics of the theory, in contrast to the latter's approach in which a mathematical formalism is developed by data analysis.

However, this is not the issue in Dirac's views that influenced Hanson, who was actually much more sympathetic to Heisenberg's approach in which theory originates with the experimental data. Hanson was influenced by Dirac's historic accomplishment, the transformation theory developed by Dirac in 1928, which not only combines relativity and quantum mechanical descriptions of electron properties, but also enables physicists to exhibit the wave-particle duality by transforming mathematically the wave description into the quantum description and vice versa. Both in his "Copenhagen Interpretation of Quantum Theory" in the *American Journal of Physics* (1959) and in his chapter "Interpreting" in *Concept of the Positron*, Hanson states that objections to the Copenhagen interpretation arise from a failure to appreciate the historical and conceptual role it had played in Dirac's 1928 paper, and he reports that in conversation with Dirac, Dirac told him that the Copenhagen interpretation figured essentially in his development of his relativistic quantum field theory, and not as merely a philosophical after-thought appended to the mathematical formalism. This personal conversation with Dirac more than anything explains Hanson's motivation for maintaining that the Copenhagen interpretation is integral to the formalism of the quantum theory. He argues against Feyerabend that even if it were possible to have a minimum statement of quantum theory with no more interpretation than is required barely to describe the facts, this is what Dirac felt he had, and Dirac's paper would not have been the paper that it actually was, had its assumptions been purified of the Copenhagen interpretation, as Feyerabend advocates. But for his thesis of scientific

discovery Hanson turned not to Dirac's aesthetic thesis, but to the logical thesis proposed by the founder of Pragmatism, Charles S. Peirce.

Peirce, Retroductive Logic, and Semantical Constraints in Discovery

Hanson was influenced by Charles S. Peirce, but he did not accept Peirce's views on observation. In his "How to Make Our Ideas Clear" (1878) Peirce set forth his pragmatic maxim, which says that our conception of the practical effects that we conceive an object might have, is the whole of our conception of that object. He distinguishes observed facts from judgments of fact, and says that observations have to be accepted as they occur, while judgments of fact are controllable. According to Peirce's theory of scientific discovery, hypotheses are judgments of fact expressed in propositions, and all such propositions are additions to observed facts that are sense impressions of singular events associated with particular circumstances. That which is added to observed facts by propositions Peirce calls practical knowledge, and it is something that is controllable and subject to error. Hypotheses are the result of inference, and Peirce distinguishes inductive and abductive types of inference. Abduction (which Hanson also calls retroduction) involves both formulating of hypotheses and then selecting of one hypothesis by testing its ability to account for surprising facts. The difference between abduction and induction is that the former involves guesswork and originality, while the latter only tests a suggestion previously made. Once the hypothesis is formulated, abduction is an inference that satisfies the following form: 1) a surprising fact, *C*, is observed; 2) if *A* were true, then *C* would be a matter of course; 3) hence, there is reason to hypothesize that *A* is true. This is actually a logical fallacy known as affirming the consequent clause of the conditional statement. Peirce says that Kepler's development of his three laws is the greatest piece of retroductive reasoning ever performed. He rejects J. S. Mill's view that Kepler merely generalized on Tycho's data, and that there was no reasoning in Kepler's procedure. Peirce maintains that at each step of Kepler's investigation, Kepler had a theory which approximated the data, that Kepler modified his theory to make his theory closer to the observed facts, and that the modifications were never capricious. Hanson adds that given a choice between two hypotheses, the simpler is preferable, where simplicity is to be understood not as a logical simplicity but rather as an instinctive simplicity,

because unless man has a natural bent in accordance with nature's, he has no chance of understanding nature at all.

In his chapter on theories in *Patterns of Discovery* Hanson rejects both the hypothetico-deductive and the Positivists' inductive accounts of scientific discovery. He rejects the inductivist thesis that scientific theories are developed by an enumerating and summarizing of observable data, as the Positivists maintained for the development of empirical generalizations; he states that empirical laws explain, they do not simply summarize. He also rejects the hypothetico-deductive thesis that scientists start from hypotheses for the development of theories, as Popper maintained; he says that scientists do not start from hypothesis, but rather they start from data. The initial inference is not from higher level hypotheses to observations, but the other way around. The article setting forth his most mature views on retroduction is "Notes Toward A Logic of Discovery" in *Perspectives on Peirce* (ed. Bernstein, 1965), which includes summaries of Hanson's earlier papers. The logic of retroduction pertains to the scientist's actual reasoning, which proceeds from an anomalous situation to the formulation of an explanatory hypothesis that fits into an organized pattern of concepts. In *Patterns of Discovery* Hanson refers to the pattern of concepts as a conceptual *gestalt*, which functions to make the anomalous situation appear intelligible. The conceptual *gestalt* supplies the semantics for the theory or hypothesis. In Hanson's philosophy the semantics of observation is variable, while in Peirce's it is fixed and uncontrollable.

In "Notes..." he says that the formal criteria for the retroductive logic of discovery are the same as those for the hypothetico-deductive logic of explanation. They both contain the same elements: a hypothesis, statements of initial conditions, and the conclusion deductively derived from the hypothesis and statements of initial conditions. One difference between them is the direction of the inference. In the hypothetico-deductive logic the inference is from the hypothesis and statements of initial conditions of an experiment, to the statements describing the observed outcome of the experiment as a conclusion. This process is used for experimental testing, and if the results are not anomalous, it also serves as the logic of the explanation of the resultant phenomenon. But in the retroductive logic the direction of inference is in the opposite direction. The statement reporting an observed experimental outcome describes an anomaly relative to what is expected, and the problem is one of finding the hypothesis capable of functioning in a hypothetico-deductive account that will explain the anomalous situation as occurring as a matter of course. But the difference

between the hypothetico-deductive and the retroductive types of inference is not just a matter of the directionality of the inference. They are also different because the former is determinate, while the latter is not. In hypothetico-deductive inference consistent premises must produce consistent and unique conclusions, while in the retroductive inference there may be many alternative and mutually inconsistent hypotheses that are able to explain deductively the formerly anomalous test outcome from the same set of statements of initial conditions. From this nondeterminate character of retroductive inference Hanson concludes that retroduction cannot yield a uniquely specific and detailed hypothesis. But he maintains that it can yield an indication of the type of hypothesis that is most plausibly to be considered as worthy of serious attention. And the decision about what type of hypothesis is the most plausible depends in turn on the structure of presently accepted theories and on the shape of the most reliable conceptual frameworks that highlight hypothesis types for the problem solver. Therefore, much as it is only against the background of the intelligible and the conceptually comprehensible offered by existing theories that the anomalies stand out at all, so it is also in these same terms that the scientist comes to know which types of hypotheses will do the job and which do not. Reflection on this analysis reveals why Hanson defends the Copenhagen interpretation, understood as the semantics that is defined by the formalism of the quantum theory. The Copenhagen interpretation is the type of hypothesis that (in Hanson's view) will most plausibly resolve the current anomalies to Dirac's relativistic quantum theory, just as it had enabled Dirac to develop his quantum theory in 1928.

Hanson also maintains that the conceptual *gestalten* constituting the semantics for currently accepted theories not only supply some guidance for the creation of new theories, but also offer what he calls conceptual resistance, which must be overcome for scientific discoveries. The development of a new theory requires a new *gestalt* just as in the reinterpretation of the ambiguous drawing, and similarly there is a resistance to such a change. In *Patterns of Discovery* Hanson illustrates this in the historical episode in which Kepler developed the theory that the orbit of Mars is elliptical. In formulating this theory Kepler had to reject the traditional belief held since Aristotle that the orbits of the planets are circular, because unlike sublunar motions the celestial motions are perfect. This is also the thesis in Hanson's most significant historical analysis set forth in his *Concept of the Positron.* This work is original historical research in which Hanson interviewed several physicists including the three

principals in the episode: Carl Anderson, P.A.M. Dirac, and P.M.S. Blackett. All three physicists discovered the positron, but only Blackett recognized that the particle discovered experimentally by Anderson was the same one that was postulated theoretically by Dirac. Dirac's 1928 paper offered a relativistic quantum theory that was Lorentz-invariant, but it also contained negative energy solutions that could not be eliminated. Originally he had hoped that these strange solutions could be construed as protons, and then he thought of them as vacancies which are positive charge solutions with the mass of the electron. This constituted the gradual development of his prediction of the existence of positive electrons before they were observed. Anderson made photographs of electron tracks in the cloud chamber, and he concluded that one of them showed a positive electron, because the change of the particle was positive while its mass was too small to be that of a proton. Dirac had published his theoretical paper on the positron in 1931, a year before Anderson's photograph. In 1933 Blackett and Occhialini reported that the Anderson particle and the Dirac particle are the same thing, by using the new photographic technique in which the particles took photographs of themselves.

Hanson states that one reason Anderson did not recognize any connection between his cloud chamber experiments and Dirac's quantum theory, is that such experiments rely on concepts that are largely classical in nature such as track-leaving particles. But the greatest conceptual constraint, the one that led many physicists to reject the idea of the positive electron, was in the semantics of the concept of the electron. That semantics was such that an intimate association between the electron and the proton, and between the two basic units of electricity, negative and positive, made the very idea of a particle other than a proton or an electron very difficult to conceive. Just as positive/negative exhaust the totality of electrical charge, so too the proton/electron was thought to exhaust the totality of charged particles, since the proton and the electron came to be viewed not as carrying the charge but as being the charge. Hence there was a conceptual resistance to the idea of a third charged particle built into the structure of classical electrodynamics and the elementary particle theory.

Hanson on Perception, Observation and Theory

Hanson defends the Copenhagen interpretation, and he criticizes the hidden-variable interpretation and Bohm's agenda. He maintains that in

microphysics all the limitations placed on our conceptions of what the microphysical world is like and what we can observe, are really limitations arising out of the linguistic features of the formal languages available. Such is particularly the case with Heisenberg's uncertainty relations. The uncertainty relations and Heisenberg's thought experiment involving a gamma-ray microscope are often said to state limits to the possibility of observation within microphysics. Hanson says that this is true in an unsuspecting way: there never have been nor could there ever be experiments or observations pertinent to the establishment of the uncertainty relations, because these relations are the conceptual or logical consequence of the language of quantum theory. In the formalisms for modern quantum physics there is a logicolinguistic obstacle to any attempt to describe with precision the total state of an elementary particle, and if there is a conceptual limit to such a description, then there is *ipso facto* a limit to such observation. The conceptually impossible is observationally impossible. Hanson's thesis is that theory is integral to observation or, as he also says, observation is theory-laden. This is also implied by Einstein's admonition to Heisenberg that it is the theory that decides what the physicist can observe. Hanson's is the same philosophy of observation that Einstein told Heisenberg in 1925, and that Heisenberg used to develop the uncertainty relations.

But Hanson was not led to develop his philosophy of observation by reflection on Heisenberg's autobiographical chronicles, in which Heisenberg relates his discussion with Einstein and the use that he made of it. Hanson identified Heisenberg's views on observation with those of Bohr, which Heisenberg included in his explicit and systematic philosophy. Nor was Hanson led to develop his philosophy in response to Feyerabend's criticisms of Bohr's dogmatic interpretation of quantum theory; Hanson's philosophy of observation was developed many years previously. His philosophy of observation was drawn from Wittgenstein's *Investigations* and from the *gestalt* psychology. It is necessary, therefore, to consider briefly Wittgenstein's ordinary-language philosophy and Hanson's use of it in his philosophy of science. Ludwig Wittgenstein (1889-1951) was a somewhat reclusive individual who wrote a somewhat unsystematic philosophy of language in a somewhat obscure style, and who is thought to have anticipated certain ascendant trends in philosophical thinking. In fact Wittgenstein seems twice in his lifetime to have anticipated successfully an ascendant trend in philosophical thought with his two principal works: firstly his *Tractatus Logicus-Philosophicus* (1922) and then later his *Philosophical*

Investigations (1953). The thesis of the latter explicitly includes a repudiation of the thesis of the former, yet each work gathered its own retinue of sympathetic interpreters and devout disciples. Both the *Tractatus* and its author attracted the attention of Schlick and his Vienna Circle (with the noteworthy exception of Carnap, who after his one and only meeting with Wittgenstein was unforgettably unimpressed). But in spite of Schlick's invitations to join the Vienna Circle, Wittgenstein remained aloof from them, just as he remained aloof from all other sublunar states of affairs.

About thirty years later the *Investigations* inspired philosophers who were becoming disillusioned with the technical pedantics of Logical Positivism, and its thesis occasioned the formation of a new philosophy of language. Conventionally historians of philosophy now refer to the two opposing dogmas in these two books as the ideal-language tradition and the ordinary-language tradition respectively. The ideal-language view set forth in the *Tractatus* has a reformist flavor, which accords special status to symbolic logic, such as may be found in Russell's *Principia Mathematica*. The *Tractatus* advanced an ideal (not metaphysical Idealist) interpretation for symbolic logic, consisting of what is called a picture-theory semantics. This is one of many variations on the naturalistic theory of the semantics of language, and it is also the most naive. This first book also advanced a constructionalist view of language. It described all sentences in the ideal language as consisting of elementary sentences, which in turn consist of semantically independent names of simple objects. All nonelementary sentences are constructable from the elementary ones. The former is said to be truth functional, which means that the truth of the constructed compound sentences depends completely on that of their component elementary sentences. As a result of this semantical atomism and logical constructionalism, the understanding of any sentence ultimately reduces to knowing its logical structure and what its constituent names reference. This is a variation on the mechanistic philosophy of the semantics of language, and was called logical atomism. The principal argument in defense of the ideal-language tradition is that ordinary language is unsuitably vague and misleading for philosophy, just as it is unsuitable for empirical sciences like modern physics, which rely on mathematics. The initial attractiveness of symbolic logic to philosophers of science was the expectation that it could serve philosophy as mathematics serves physics. This programme evolved into the Logical Positivist reductionist programme of Carnap and others such as Feigl and Hempel, in which the controlling agenda was the logical reduction of theories to a semantically significant observation language, in

order to demonstrate the meaningfulness and semantics of the scientific theories.

But experience with the reformist efforts of the ideal-language philosophers, notably the Logical Positivists, led some younger philosophers to charge that ideal languages are even more unsuitable than ordinary language for philosophy, and that philosophical analysis should be directed toward the examination of colloquial language. The outcome was a new folk philosophy that is self-consciously naive. Wittgenstein anticipated this reaction, perhaps because it was also his own reaction to his own *Tractatus*, and he was led to develop his ordinary-language philosophy.   Early statements of his new philosophy were set down in a set of notebooks later published as *The Blue and Brown Books* (1958), and the more mature statement is the *Investigations*. The latter work describes philosophy as a kind of empirical linguistics, and its main themes are (1) the variety of uses of language, (2) the need for the philosopher to consider statements not in isolation but in the context that occasions their utterances, and (3) the definition of meaning in relation to usage. Wittgenstein maintained that the problems of philosophy originate in philosophers' misunderstanding of certain crucial terms such as "know", "see", "free", "true", "reason", and that the resolution of these problems requires examination of the uses of these words as they occur in ordinary-language discourse. The later Wittgenstein seems clearly to have rejected the naturalistic theory of the semantics of language. He asks rhetorically in the *Investigations*, if the formation of concepts can be explained by facts of nature, then should the philosopher not be interested not in grammar, but rather in that in nature which is the basis of grammar. He answers that the philosopher is not interested in natural science or in natural history, and he affirms an artifactual theory of the semantics of language stating that a concept is comparable to a style of painting. But the artifactual theory that he accepts seems to be a wholistic one, since he states in the opening pages of *The Blue and Brown Books* that understanding a sentence means understanding a language.

Hanson was of the generation of philosophers who took their professional education after the Second World War, and he was also one of those who looked to Wittgenstein's new philosophy to rise above the inadequacies of the Logical Positivist philosophy of science. He was not an ordinary ordinary-language philosopher; he was firstly a philosopher of science, and if there was an ordinary language of interest to him, it was the language ordinary to contemporary physics including most notably microphysics. He was specifically drawn to Wittgenstein's comments in the

Investigations about seeing, in order to re-approach the subject of observation in physics, which modern quantum theory had made so problematic. Hanson's discussions about observation and theory are set forth in *Patterns of Discovery*, in "Observation and Interpretation" in *Philosophy of Science Today* (1967), and in *Perception and Discovery*. Hanson rejects the Positivist view that seeing is merely a matter of predetermined sensations, sense data, phenomena, or retinal reactions in the eye, and that interpretation is something added to the predetermined perception as a secondary and discrete step in the perceptual process. Instead he says there is more to seeing than meets the eye, and he follows Wittgenstein's view that interpretation is an integral component of seeing instead of something forced on it. The significance of this point is that perception is not predetermined and fixed by nature but is variable, and he illustrates this variability in perception by using both Wittgenstein's and others' ambiguous drawings that admit to reversible optical interpretations. He explicitly invokes Gestalt psychology (something that Wittgenstein did not do), to explain the reversibility of interpretations of ambiguous drawings as changes in the conceptual organization of what is observed. In this context Hanson references Duhem's example in *The Aim and Structure of Physical Theory* of the layman visiting a physicist's laboratory. The layman would have to learn physical theory before he could observe what the trained physicist observes. Duhem had described this commonplace state of affairs in terms of his Positivist semantics of observation and theory. But Hanson is a critic of Positivism, and he does not maintain any such two-tiered semantical thesis, as Duhem had. Hanson maintains that the postulated laboratory situation reveals that the elements of the laboratory in the visitor's field of perception are not organized as they are for the trained physicist. Physical theory provides the physicist with patterns within which data appear intelligible; it is what makes possible observation of phenomena as being of a certain kind and as related to other phenomena.

To illustrate his thesis that perception is theory-laden, Hanson uses the example of the second-century and the seventeenth-century astronomers who both look at the dawn. They both have the visual experience of the rising sun, but they do not see the same thing, because each believes different astronomical theories: the former, Ptolemy, believes in the geocentric theory, the latter, Galileo, in the heliocentric theory. Nevertheless, it can still be said that they see the same thing, since the sun could be described by both as a brilliant yellow disk. Hanson calls this latter kind of description phenomenal seeing, but he maintains contrary to the

Positivists that such phenomenal seeing is not the ordinary way of seeing. It is something that requires special effort, because seeing is normally interpretative, and is used when the observer is confronted with a new seeing experience in which case what is seen cannot be characterized by reference to his background knowledge. Observation in science aims to pass beyond the phenomenal seeing occurring in the case of the new experience, and to get the visual experience to cohere against a background of accepted knowledge.

The differences between *gestalten* are due to differences in previously acquired background knowledge, knowledge that involves language. Hanson is therefore led to follow Wittgenstein's ordinary-language analysis, because examination of commonly used locutions in colloquial discourse reveals the relation between language and the variability of interpretation in observation. The locution "seeing as" reveals that seeing is to see an object as a certain kind of thing, which is brought out by the verbal context in which the locution occurs. The text in its context supplies the interpretation. But his thesis is still stronger than merely stating that language reveals an interpreting conceptual component; he invokes the locution "seeing that" to exhibit a necessary role for language in interpretation. The idea of "seeing that" explains the relation of "seeing as" and the observer's background knowledge: to see something as a certain kind of thing is to see that it behaves in a certain known and expected manner. The "seeing that" locution supplies a statement of the background knowledge, which can be true or false. Seeing is therefore a theory-laden activity in the sense that the seeing is interpreted by reference to our background knowledge. Without a linguistic component to seeing, nothing we saw could be relevant to our knowledge. Before the wheels of knowledge can turn relative to a given visual experience, some assertive or propositional aspect of the experience must have been advanced. Only statements can be true or false; visual experiences must be cast into the form of a language to be considered in terms of what we know to be true, i.e. in terms of our theories.

Furthermore, Hanson's thesis is not only that language is necessary for the interpretation that is integral to perception, but also that the logicogrammatical form of the language used for description exercises a formative control over the interpretative thinking that occurs in perceiving. Just as seeing may be stated locutions which are "that..." clauses, so too can facts and theories. For this reason Hanson says that Ptolemy could not express in the second century what were facts for Galileo fifteen centuries later. Physical concepts are intimately connected with the formalisms and

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notations in which scientists express them, including the formalisms used today in contemporary microphysics. The dependence of physical concepts on the mathematical formalisms is a very strategic consideration in Hanson's rejection of attempts by Bohm and Feyerabend to propose interpretations of the uncertainty relations and the Schrödinger wave function that are alternatives to the Copenhagen interpretation of modern quantum theory. For Hanson the Copenhagen interpretation is precisely that interpretation which is supplied by the formalism of the modern quantum theory, because contrary to both the Positivists and to Bohr, it is the formalism that supplies the intelligible patterns and conceptual organization in perception for the observations relevant to microphysics. Interestingly in his *Primer of Quantum Mechanics* (1992) Chester Martin explicitly exhibits Dirac's notational system as a language, and references the linguistic philosophy of Benjamin Lee Whorf.

Hanson further follows Wittgenstein when he maintains that the meaning of a sentence is its use, and that there are multiple uses for a sentence. Thus he states that the laws and theories of physics have many uses, and not just one, as most philosophers have maintained. The contingently empirical status of a statement is one of the uses of the theory in science. Another is to make the phenomena cohere in an intelligible way, such that empirical disconfirmation does not result in the negation of the concept described by the theory, but rather results in no coherent concept at all. The dynamical laws of classical physics, for example, are a system of propositions that are empirically true, and the fundamental propositions on which the system rests are empirically true. But these fundamental propositions are also treated as axioms, such that the system delimits and defines its subject matter. Then nothing describable within the system could refute its law statements; disconfirmatory evidence counts against the system as a whole, and only shows that the system does not hold, where formerly it was thought to hold. Hanson calls this use of laws and theories functionally *a priori*. These ideas are reminiscent of Heisenberg's comments in "Questions of Principle in Modern Physics", in which he says that it is not the validity but only the applicability of classical laws, which is restricted by modern relativity and quantum physics. Hanson does not reference Heisenberg, but his thesis of the functionally *a priori* use of laws and theories is in this respect similar to Heisenberg's doctrine of a closed-off theories, with the noteworthy exception that Hanson does not reserve certain axiomatic systems such as classical mechanics for observation in physics, as does Heisenberg in his explicit philosophy of physics. Heisenberg's

philosophy of observation in his doctrine of closed-off theories does not admit the variability in perception that Hanson's philosophy asserts. Instead in his explicit philosophy Heisenberg followed Bohr's thesis that there are forms of perception that are found only in colloquial language and in its refinements in classical physics.

Hanson's semantical investigations sometimes took a turn away from the wholistic approach of Gestalt psychology. In the chapter on classical particle physics in *Patterns of Discovery* he considers the idea that the meanings of some names have their properties built into them, such that falsification of statements predicating those properties of the named substances is effectively impossible. And in "Newton's First Law: A Philosopher's Door into Natural Philosophy" in *Beyond the Edge of Certainty* (1965), he states that rectilinearity, motion *ad infinitum*, and free force, are conceptions within classical mechanics that are interdependent, in such a way that it is possible to treat the idea of uniform, rectilinear motion *ad infinitum* as itself built into the notion of free force, as part of the latter's semantical content. The terms in Newton's first law are semantically linked: the meaning of some of its component terms unpacks sometimes from one or two of the others, but then sometimes the meaning of these unpacks from that of the first. Which are the contained and which are the semantical containers can affect the logical exposition of any mechanical theory built thereon. These are semantical decisions which guarantee that in different formalizations of Newton's theory different meaning relations will hold between the law's constituent terms. The term "unpack" in connection with semantical analysis is a phrase used by the early Pragmatist philosopher William James, although Hanson does not reference James. It is unclear whether or not Hanson ever thought of this type of semantical analysis as an alternative to his frequent recourse to Gestalt psychology. Nevertheless it is an alternative approach in semantical analysis, because it is not wholistic. On the *gestalt* thesis it is not possible to unpack a *gestalt* into its component parts, because the *gestalt* is more than a mechanical organization of its parts. In his discussions of quantum theory Hanson never exploited this mechanistic or logical analysis of meanings into component parts.

Hanson's Philosophy of Science

Aim of Science and Discovery

Hanson's ideas about the aim of science pertain to what he calls research science, as opposed to what he calls almanac science, and are integral to ideas of scientific discovery. In his "Introduction" in *Patterns of Discovery* he states that in a growing research discipline, inquiry is directed not to rearranging old facts and explanations into more elegant formal patterns, but rather to the discovery of new patterns of explanation. The idea that observation is theory-laden is strategic to this purpose. In the chapter titled "Observation" in *Patterns of Discovery* he states that the scientist aims to get his observations to cohere against a background of established knowledge. This kind of seeing is the goal of observation. And similarly in the last chapter titled "Elementary Particle Physics", the area of contemporary physics that he says is presently a research science, he states that intelligibility is the goal of physics, the conceptual struggle to fit each new observation of phenomena into a pattern of explanation. Often the pattern precedes recognition of the phenomena, as Dirac's theory of 1928 preceded discovery of the positron, the antiproton, and the antineutron. But then Dirac's pattern was itself the outcome of an effort to find a suitable explanation for prior phenomena, namely a unified, relativistically invariant theory of electron spin, which would give the correct fine structure formula, explain the Zeeman effect of the doublet atoms, describe the Compton scattering, and supply a model of the hydrogen atom.

Explanation

Hanson offers an evolutionary perspective on scientific explanation. In the third chapter of *Concept of the Positron* he states that the concept of scientific explanation has experienced a historical evolution that follows upon the historical development of physics. Leibniz denied that Newton's theory offers explanation, even though he admitted that it offers acceptable predictions. Today the concept of explanation advanced by the Positivists, such as Hempel, is based on the concepts of Newton's physics including notably the deterministic thesis that explanation implies deterministic prediction. The concept of explanation implied in the nondeterministic quantum theory is not yet accepted. Hanson states that if just after Leverrier had predicted the existence of the planet Neptune in 1847, a time when

Newtonian physics had reached its apex, some physicist who had proposed a new theory that explained all that Newton's theory explained and furthermore explained several minor flaws in Newton's theory, the new and better theory would have been viewed as merely a predictive device, not an explanation. But if Newton's theory then began to show major weaknesses, while the new theory succeeded where Newton's had failed, still these accomplishments would decide nothing. The scientists would begin to show increasing reliance on the new theory, yet it would not be accepted as an explanation. All the same, younger physicists would develop the new theory further. Finally if Newton's physics had begun to fall apart while the new theory opened up new branches of science, focused on problems never before perceived, fused disciplines previously thought to be distinct, and sharpened experimental techniques to an unprecedented degree, then the very pattern of thinking in an inquiry properly called scientific would reflect the new physics with its new concept of scientific explanation; to be able to cope with a scientific problem at all, would be to have become able to build it into the conceptual framework of the new physics.

Hanson distinguishes three stages in this process of the evolution of a new concept of explanation; they are the black box, the gray-box, and the glass box. In the first stage, the stage of the black box, there is an algorithmic novelty, a new formalism, which is able to account for all the phenomena that an existing formalism can account for. Scientists use this technique, but they then attempt to translate its results into the more familiar terms of the orthodoxy, in order to provide understanding. In the second stage, the stage of the gray box, the new formalism makes superior predictions in comparison to the older alternative, but it is still viewed as offering no understanding. Nonetheless it is suspected as having some structure that is in common with the reality it predicts. In the third stage, the stage of the glass box, the success of the new theory will have so permeated the operation and techniques of the body of the science that its structure will also appear as the proper pattern of scientific inquiry. Hanson says that quantum theory is in the second stage, because scientists have not yet ceased to distinguish between the theory's structure and that of the phenomena themselves. This evolution is the gradual adoption of the practice of scientific realism, in which (to mix metaphors) the glass becomes the spectacles through which reality is seen. Explanatory langauge is customarily thought to be explanatory, because it describes the real causes of the phenomena explained. Therefore, the concept of causality also undergoes the kind of evolution that occurs with the concept of explanation. In the

chapter titled "Causality" in *Patterns of Discovery* Hanson says that cause words are theory-laden; they are the details in an intricate pattern of concepts. Causes are connected with effects, but only because theories connect them, not because the universe is held together with a cosmic glue. Questions about the nature of causation are to a large degree questions about how certain descriptive terms in definite contexts coupled together complement and interlock in a pattern of other terms. The elements of explanation, causation, and theorizing become worked into a comprehensive language pattern.

Criticism

Hanson's discussion of scientific criticism is principally concerned with the topic of crucial experiments. He takes up the topic in a chapter in *Concept of the Positron* in which he discusses the different concepts of light in the history of physics, and he discusses it again later in a special chapter in *Perception and Discovery*. Hanson's rejection of the idea of crucial experiments has its basis in his thesis that observation is theory-laden. A commonly referenced example of a crucial experiment is Foucault's 1850 crucial test between the wave and particle concepts of light. In that experiment Foucault demonstrated that light travels more rapidly in air than in water. According to the doctrine of the crucial experiment the corpuscular hypothesis should have been banished forever. But this has not happened. The photoelectric effect and the Compton effect can only be explained on a corpuscular theory of the nature of light. The experiments are not crucial, because the observations are important only against the assumptions, theories, and hypotheses that are in the balance before the experiment is performed. One of the assumptions is that light cannot be both wave and particle. The crucial test is a test of the alternative hypotheses together with all of their assumptions, just as in ordinary scientific observation there is a pure registration or sensation plus all of the assumptions necessary to give those sensations meaning. If we were forced to revise our assumptions, then the crucial experiment must be re-interpreted, so that it need not decide against one of the hypotheses. Some of the most profound revolutions in modern science have consisted not in the criticisms of old hypotheses, but in the criticism of the assumptions underlying the hypotheses. Crucial experiments are crucial against some hypothesis only in relation to a stable set of assumptions that we do not wish to abandon. But no set of assumptions is permanently valid. Hanson says

that crucial experiments are out of the same bag as pure observations and uninterpreted facts; they are philosophers' myths.

Wittgenstein said language has many uses. Hanson's discussions of crucial experiments pertain only to theories that may intelligently be disconfirmed. Although in principle all statements of science are testable and can be falsified, in practice theories often have another use or function. Following Wittgenstein's thesis that language may have many uses, Hanson maintains that theories functioning as pattern statements supplying a conceptual *gestalt* will not yield an intelligible statement negating the theory, if the theory is viewed as disconfirmed. This is because the theory gives the phenomena their intelligibility; and this explains why scientist will not reject a theory even while they recognize the existence of anomalies that are not intelligible in the theory. What scientists do in practice is to attempt to save the theory with small modifications or wait until a new and more adequate theory is proposed that explains all that the old theory explains as well as the anomalies to the old theory. Anomalies do not make scientists give up intelligibility. It is for this reason that physicists have not given up the Copenhagen interpretation in spite of the anomalies confronting Dirac's theory. Thus Hanson, opposing Bohm in the "Postscript" chapter in *Quanta and Reality*, states that dropping orthodox quantum theory right now would be to stop doing microphysics altogether. Then Hanson immediately adds that should the heretics (Bohm *et al.*) succeed in accounting for everything that orthodox theory now describes, and do so without the divergence difficulties and the renormalization nuisance even without the uncertainty relations and the irreducibly statistical laws, should they do all this, then physicists of the world will be at their feet, and science will have ascended to a new plane of power and fertility.

Hesse on Models and Analogy

Quanta and Reality (1962) is a collection of discourses initially broadcast as a radio series by the BBC in 1961. It includes a dialogue involving Bohm, a "Postscript" commentary by Hanson, and a commentary titled "Models and Matter" by the Cambridge University philosopher of science, Mary B. Hesse. Hanson's comments are generally critical of Bohm; Hesse's are more sympathetic. This alignment among the participants is not limited to the specifics about the contemporary quantum theory; it divides along issues about the semantics of scientific theories in general and also

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about the role of semantics in scientific discovery. All participants have much to say about the semantics involved in scientific discovery.

On Hanson's view the semantics of a theory is determined completely by the mathematical formalism and the measurements that the equations of the formalism relate. The relations expressed by the theory including its grammatical/mathematical form determine the conceptual *gestalt*, which constitutes the semantics of the theory. And in the case of quantum theory the Copenhagen semantical interpretation with its wave-particle duality thesis is integral to the mathematical formalism of the quantum theory. Furthermore the semantics of the quantum theory so understood is strategic to the further development of microphysics, as evidenced by the fact that Dirac said he relied on it for his development of his field quantum theory. Hanson does not deny that there may also be other language about the microphysical domain explained by the equations of the quantum theory, language that does not contradict the quantum theory. But he views such supplementary language as mere philosophy, and not as part of the theory itself. He places Bohr's naive epistemology in this category of supplementary philosophical language.

Opponents to the Copenhagen interpretation agree with Hanson that semantics has a strategic role in scientific discovery. But they do not agree that the Copenhagen interpretation is integral to the formalism of the theory. They are motivated to disagree not only because some of them propose alternatives to the wave-particle duality thesis, but also because in general they maintain that there is more that determines the semantics of theories than just the formalism and measurement concepts. The source of this additional semantics that they say is found in many if not all theories, is the nonliteral figurative and often imaginative language, which they find historically characteristic of theories in physics. This figurative language involves analogies and metaphors, and the distinctively additional semantics is often called a model. This is one of several common meanings for the term model, and in the present context the term functions to articulate the different views on the issue at hand. Unlike Hanson, Hesse views the ideas of waves and particles as theoretical models for quantum theory, and her view proceeds from a sophisticated examination of these questions.

Hesse's views about the semantics of theories are influenced by her former mentor at Cambridge, R. B. Braithwaite, a Logical Positivist philosopher of science. Their views are similar but not the same. Both Hesse and Braithwaite are Positivists, and thus distinguish observation and theoretical terms, although Hesse's views evolved beyond Positivism later in

her career.   The distinction between observation and theoretical terms produces for Positivists the peculiar problem as to how theoretical terms contained in a semantically uninterpreted formal calculus can be meaningful instead of meaningless or metaphysical. In his *Scientific Explanation* (1953) Braithwaite distinguishes two sources of semantical interpretation for an uninterpreted formal calculus containing theoretical terms: Firstly the formal calculus may receive its semantical interpretation that makes it a meaningful scientific theory containing theoretical terms, when the logically posterior statements of implied consequences, the observation sentences, determine the meaning of the theoretical terms in the calculus of the logically prior premises. Theoretical terms are thus said to receive indirect meaning, since their meanings are determined by their contexts in relation to one another and to the sentences expressing the observable directly testable outcomes, which the experimentalist can logically derive from them. In other words the meanings of the theoretical terms are indirect, because they receive all their semantics contextually and not ostensively, as do observation terms. Braithwaite labeled this view contextualism. Yet Braithwaite also maintains that a good theory is capable of growth, such that it must be an alternative way of describing the empirical statements upon which it is based. Therefore he admits that the meanings of the theoretical terms need not be limited to being contextually defined explicitly, because the indirect contextual interpretation does not satisfy this growth criterion for theories.

Then Braithwaite states secondly that a theory may furthermore be given an interpretation by another source called a model. A model is additional language that contributes meaning to the terms, both those occurring in the premises and those in the conclusions, both to the theoretical terms and the observation terms. Most notably, unlike the contextual definition the model is not a *literal* interpretation for the domain explained by the theory. Thus Braithwaite says that theories and models have different epistemological structures, even when they have the same calculus. It might also be said that the introduction of the model makes the theoretical terms equivocal with one meaning the literal one defined in context and another the nonliteral one defined by the model language. For example according to Braithwaite the solar system may serve as a model for the hydrogen atom, even though it is understood that the atom is not literally to be taken as a solar system. Braithwaite says that thinking of theories by means of models is always "as-if" thinking, e.g. thinking of the atom as if it were a solar system. But he makes an exception for quantum theory: he says that for the physicist, Schrödinger's wave function is exhaustively

interpreted in terms of its use in the calculus of the quantum theory, and he states in a footnote that no one supposes that the wave function denotes a wave in any ordinary sense of wave. In Braithwaite's view modern quantum theory does not have any model.

Hesse's semantical theory is set forth in her *Models and Analogies* (1953) and also in her article "Models and Analogies in Science" in *The Encyclopedia of Philosophy* (1967). There she compares two earlier conflicting protagonists in the issues of models and the semantical interpretation of theories. One is Duhem, and the other is Campbell whose views on the semantics of theories is more like Hesse's than Braithwaite's. In his *Aim and Structure of Physical Theory* Duhem had argued a view similar to Hanson's that the semantics of a physical theory is determined only by the equations and measurement concepts, and that even if models based on analogy with more familiar phenomena have served some heuristic value for developing the new theory, nonetheless these models are not part of the theory itself and may be discarded after the theory is constructed.

On the other hand in his *Physics, The Elements* (1920) the Cambrian philosopher Norman R. Campbell argued that analogically based models are not merely dispensable aids, but rather are indispensable to a theory, because they assist in the continuous extension of the theory. He argued that the Positivists' hypothetico-deductive form of explanation alone is insufficient to account for the role of theory in science. He maintained that in addition to the three elements, (1) the formal deductive system of hypothesized axioms and theorems, (2) the dictionary for translating some of the descriptive terms in the formal system into experimental terms, and (3) the experimental laws such as the gas laws, which are confirmed by empirical tests and also can be deduced from the system of hypothesis plus dictionary, there is a fourth element in theories, namely (4) the analogy, such as may be exemplified in gas theory by the model of point particles moving at random in the vessel containing the gas. The motivating intent behind this view is that scientific theories are not static museum items, but rather are always growing as an integral part of the growth of science; and this latter view, which might be called the Cambrian thesis, is the one that is accepted by both Braithwaite and Hesse.

But her views are not quite the same as Braithwaite's. Most notably unlike Braithwaite, Hesse does not distinguish the semantics of theoretical terms from the semantics of models. In fact for Hesse it is the models that supply the indirect meaning had by the theoretical terms. And since extrapolation on the basis of the models explains how the theories grow,

Hesse's interest in the semantics of theoretical terms leads her into the topic of scientific discovery. Hesse also differs with Braithwaite about the interpretation of quantum theory. She believes that the concepts of wave and particle supply modern quantum theory with two contrary models. In her examination of analogical models Hesse distinguishes three parts to an analogy, which she calls the positive analogy, the negative analogy, and the neutral analogy. The positive analogy consists of those aspects of some familiar phenomena which are known to apply to the phenomenon explained by the theory. These include the similarities that have occasioned recognition of the analogy in the first place. The negative analogy consists of those aspects of the familiar phenomena that are known not to apply or are known to be irrelevant to the phenomenon explained by the theory, and the theorist ignores them. Hesse views the neutral analogy as strategic for scientific discovery. The neutral analogy consists of those aspects of the familiar phenomena whose relevance to the problematic phenomena in the domain of the theory is presently unknown, and therefore whose explanatory potential for further development of the theory is not yet known. She calls the semantics supplied by the neutral analogy, i.e. the concepts and conceptual relations not present in the empirical data alone, what she also calls the "surplus" meaning. She also uses the phrase open texture property of meaning without referencing any previous usage of the phrase in the literature. The further theoretical exploration of the problematic phenomena will be guided by the neutral analogy. Exploitation of the model for scientific discovery consists in investigating this neutral analogy, because it suggests modifications and developments of the theory that can be subsequently tested empirically. Such in Hesse's view is how neutral analogies enable theories to grow.

In "Models and Matter" Hesse says that in quantum theory the wave and particle models are such that what is positive analogy in the one model is negative analogy in the other. She also says without elaboration that in the two models there are still features that physicists cannot classify as either positive or negative, and that it is due to these features that the particle and wave models are yet essential. Like Bohm, Hesse says that if physicists were forbidden to talk in terms of models at all, then they would have no expectations, and would be imprisoned forever inside the range of existing experiments. In her discussion of subquantum theories in the chapter "Modern Physics" in her *Forces and Fields: The Concept of Action at a Distance in the History of Physics* (1962) she expresses agreement with Bohm's thesis that a new quantum theory postulating a subquantum order of

magnitude is possible. Specifically she rejects the Copenhagen thesis that current formulations of quantum theory and current models of physical reality are unalterable. She says that if two models each turn out to be unsatisfactory in isolation, but usable when regarded as complementary to each other, it is curiously conservative to assert that no other models can be conceived and to elevate the principle of complementarity to a quasimetaphysical status, when it should instead be regarded as a consequence of the poverty of our imagination. She adds that it may be very difficult to conceive new models, especially when it is remembered that they cannot be entirely abstract formalisms because they must be tied to the observable at some level, but difficulty does not entail logical impossibility.

Hesse on Metaphor

The thesis that analogically created models supply nonliteral interpretation for theoretical explanations leads Hesse to consider also the semantics of metaphorical language. In her "Explanatory Function of Metaphor" in *Logic, Methodology and Philosophy of Science* (ed. Bar-Hillel, 1965) she states that her views are significantly influenced by the interactionist concept of metaphor proposed by her Cambrian colleague Max Black in his *Models and Metaphors* (1962). Black opposes his interactionist view to the comparison view. On his rendering of the comparison view the metaphorical statement is nonliteral for two reasons: Firstly if it is taken literally, it is a false statement. Secondly it can be restated as an exhaustive list of similes, which are literal statements expressing the similarities implied in the metaphor. In other words in rejecting the comparison view Black rejects the thesis that metaphors are elliptical similes. In her paper on the function of metaphor in theoretical explanation Hesse distinguishes a primary system and a secondary system, where both systems may be taken as real or physical systems that are described literally. The metaphoric use of language to describe the primary system consists of transferring to the description a word or words that normally are used in connection with the literal description of the secondary system. In a scientific theory the primary system is the domain of the *explanandum*, the statements that describe the explained phenomenon in an observation language, while the secondary system is the domain of the *explanans*, the statements constituting the explanation and containing either observation language or a familiar theory from which the explanatory model is taken. The explanation of the

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explanandum for the primary system consists of statements that metaphorically use vocabulary describing the secondary system and that are applied to the primary system on the basis of some similarity or analogy.

In his statement of his interactionist thesis Black lays down a criterion for the literal equivalence of a metaphor: the metaphor can be re-expressed as an exhaustive list of statements expressing all the similarities in the metaphor as literal similes. Then he rejects the possibility of reducing the metaphor to such a list of similes, because such a list can never be exhaustive. This inexhaustibility is especially important to Hesse, because the possibility of indefinitely extending and explaining the metaphor constitutes the fruitfulness of the explanatory model containing the metaphorical language. But the thesis that metaphor cannot be reduced to literal language is not all there is to Black's interactive view of metaphor. The interactive thesis is called interactive, because the metaphorical use of language is seen as changing the literal meanings of the words that are used metaphorically; there is an interaction of the meanings of the words in their descriptions of both the primary and secondary systems. For example the metaphorical statement "Man is a wolf" makes wolves seem more human and men seem more vulpine. This is contrasted with the comparison thesis, which purportedly assumes that the literal description of both primary and secondary systems is unaffected by the metaphor, such that the meanings of the terms remain semantically invariant. In Hesse's view the semantical variance postulated by the interaction view of metaphor is relevant to scientific explanation, because metaphor changes the semantics of the observation language. This thesis distances Hesse from the Positivists, for whom the observation language must remain completely uncontaminated by theoretical language. Hesse sees this meaning variance in the observation language as contrary to the assumptions of the hypothetico-deductive account of explanation, in which it is assumed that descriptive laws pertaining to the domain of the explanandum remain empirically independent and semantically invariant through all changes of explanatory theory. She therefore advances the view that the deductive model of explanation should be modified and supplemented by a view of theoretical explanation as metaphoric redescription of the domain of the explanation.

The interactive view of metaphor advanced by Black and used by Hesse, is not the prevailing view. Conventionally metaphor is construed as an elliptical simile containing implicitly the idea of an underlying similarity that can be explicitly and literally expressed by a simile with the words "like" or "as". For example in his *Philosophy of Language* (1964) William

P. Alston sets forth what may be taken as the comparison thesis of metaphor. Like Black and Hesse, Alston maintains that metaphor has an indeterminacy in it that is inexhaustible. But he also maintains that it is a mistake to believe that metaphorical and literal language are different kinds of meaning. On Alston's view the difference between metaphorical and literal language is one of degree, where literal language may be identified with established usage and metaphor is a new usage that is derived from established usage. All meanings are literal meanings, and the derived and unconventional usage in a metaphor may be expressed literally with greater or lesser extent of explanation. When the new usage is forgotten, the metaphor becomes a dead metaphor in the sense that it is dead and buried. But when it has become part of the established usage, then the metaphor has become a dead metaphor in the sense that it has become part of the conventional literal language, and explanation of its derivation from the original established usage becomes an exercise in etymology. Furthermore unlike Black or Hesse, Alston does not say that metaphor must be capable of being reduced to an exhaustive list of similes, in order to be reduced to literal use, because there is indeterminacy in literal language as well as in metaphor. Alston references Friedrich Waismann's "Verifiability" in *Logic and Language* (1952) stating that literal words denoting physical objects have an inexhaustible vagueness which remains even after all attempts at clarification. This vagueness remains because in addition to actual cases of indeterminacy of application, one can think of an indefinite number of possible cases in which one would not know what to say. Waismann calls this inexhaustible vagueness the "open texture" of descriptive language. Alston denies that metaphor is simply vagueness, but he says that in both metaphorical and established language there is an inexhaustible indeterminacy due to the fact that it is impossible to decide in advance on every possible usage of a word.

The conclusion to be drawn from this is that Black's criticism for the reduction of metaphor to literal language by means of an exhaustive list of similes is not a feasible criterion, because it would demand more determinateness of nonliteral language than of literal language. A weaker criterion therefore is in order. It would seem sufficient to require only that a metaphor be re-expressible with at least one simile that makes explicit an implicit underlying similarity, presumably but not necessarily the similarity that is intended by the speaker or writer initiating the metaphor. Furthermore semantical variability or meaning variance must therefore be a property of both metaphorical and literal language, or it must be a property of neither, since the former is merely the elliptical expression of the latter.

These considerations are relevant to Hesse's thesis about metaphor in theoretical explanation in science. Her tacitly assumed premise is that meaning variance does not occur in literal language, i.e. in the absence of metaphor. On this premise the nonreducibility of metaphor to literal language is strategic to her rejection of the adequacy of the hypothetico-deductive thesis of theoretical explanation, and it is strategic to her reliance on metaphor to account for semantical change or meaning variance in the language for description of observed phenomena. On the other hand if as Alston says metaphor is reducible to literal language, then semantical variability must be a property of both metaphorical and literal language, or it must be a property of neither. And it is clearly a property of metaphor; otherwise there would be no dead metaphors indicating that the new metaphorical use has either been forgotten or has become a new alternative literal use. Thus the reducibility of metaphor to conventional literal language implies that metaphor cannot satisfactorily be used as a general explanation of semantical change in science, even if it can serve to indicate that semantical change has occurred relative to currently established meaning. The theory-laden character of observation discourse resulting from theory revision is a much more general aspect of the semantics of language than just its metaphorical usage. The explanation of semantical change or meaning variance demands a general theory of semantical description for all literal language. At the same time metaphor seems clearly to have a role in occasioning semantical change, and it may have a strategic utility for the development of new theories in science.

Two decades after these 1960's-vintage papers on analogy, metaphor, and models Hesse finally reconciled herself to the artifactual thesis of the semantics of language and the phenomenon of pervasive meaning variance in the semantics of descriptive terms. But her pathway was a circuitous one. In her *Construction of Reality* (1986), co-authored with Michael A. Arbib, she says that her starting point is Max Black's interaction theory of metaphor as modified in the light of Wittgenstein's family-resemblance theory of meaning. At the end of her philosophical trek she is not consistent with Black's irreducible separation of literal and metaphorical meanings, although she continues to advocate it. Firstly she rejects literal meaning understood as invariant meaning, and announces (placing her own words in quotes) that all language is metaphorical, a phraseology that she says some will find shocking. It might better have been described as mocking the meaning of literal. Her thesis is that the use of general terms is always metaphorical in the sense of relying on perceived similarities and differences

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between various individuals, similarities that are family resemblances for which a term has been acceptably used in the past. She dichotomously opposes Wittgenstein's family-resemblance thesis to the Aristotelian natural-kinds thesis. She says that either the world is really Aristotelian, such that objects really fall into sharply discriminated species; or in practice we allow that language works by capturing *approximate* meanings, such that *degrees* of similarity and difference are sufficiently accessible to perception to avoid confusion in ordinary usage. Hesse believes that the second option is more realistic. She adds that it implies we lose potential information every time we use a general descriptive term - either information that is present to perception but neglected for purposes of the description (e.g. no one discriminates *every* potential shade of red), or information present in reality but below the level of conscious perception. In the latter case the information may later be made accessible by instrumental aids such as microscopes, etc. Understood in terms of the family-resemblance analysis, metaphorical shifts of meaning depending on similarities and differences between objects are pervasive in language - not deviant - and some of the mechanisms of metaphor are essential to the meaning of any descriptive language whatever. She explains that this is what she means by her thesis that all language is metaphorical. This peculiar outcome is due to her identification of the naturalistic thesis of the meaning of terms, which she calls semantical naturalism, with the concept of literal meaning, and is also due to her earlier conclusion that metaphor enables a nonliteral redescription of observed phenomena in scientific explanation.

Yet she does not abandon altogether the intuitively recognized distinction between literal and metaphorical usages in language. Having firstly rejected the meaning-invariant idea of literalness she then secondly redefines the meaning of "literal" by making the distinction between literal and metaphoric pragmatic instead of semantic. And it is here that Black's interactionist thesis would seem to serve her no longer, because what now distinguishes metaphor from the literal is not Black's semantical irreducibility but rather conventionality. In fact rejecting Black's irreducibility thesis would seem implied by a pragmatic distinction, because she says that her new definition of "literal" merely enshrines the use that is most frequent in familiar context - the use that least disturbs the network of meanings. It is the one generally put first in dictionary entries, where it is followed by comparatively dead metaphors. And metaphor denotes particular forms of literary expressions that depend on explicit recognition of similarities and analogies. For example "Richard is a lion" is a metaphor,

because it based on elaborate analogy between particular human and animal dispositions, in which the obvious differences between human beings and lions are consciously discarded. A metaphor in this sense is usually recognized only when it is newly minted. When metaphors become entrenched in a language, they become a new literal usage. Such is the fate of dead metaphors.

Hesse says scientific language conforms closely to her metaphorical model of meaning. Not only is theoretical explanation a metaphoric redescription of the domain of the phenomena, as she said in the 1960's, but now she also says that scientific revolutions are metaphoric revolutions. In her earlier years as a Positivist, Hesse had been critical of Kuhn often referring to his views pejoratively as historicist. Now using the Kuhnian terminology and referencing Kuhn she says that in the development of science a tension always exists between normal and revolutionary science: normal science seeks to reduce instability of meaning and consistency and to evolve logically connected theories. Revolutionary science makes metaphoric leaps that are creative of new meanings and applications and that may constitute genuine theoretical progress. Ironically in his later writings Kuhn rejected Hesse's thesis that all meaning is metaphorical, and he embraced Black's interactionist view.

Comment and Conclusion

Contrary to often expressed opinion the topic of scientific discovery has not been a neglected one in philosophy of science. The above survey reveals that many philosophers and scientists have addressed it with a semantical approach using figures of speech. But no application of a metatheory of scientific theory development using a purely semantical approach has yet succeeded in generating a new and successful scientific theory in any science, even though many noteworthy historic scientific discoveries have resulted from the intuitive use of such semantical devices as analogy and metaphor. To date the only metatheories that are sufficiently practical to function as applicable procedures for scientific discovery are those based on the discovery-systems approach, and most of these have been academic exercises involving the reconstruction of existing or historical theories. Only a few discovery systems have actually been used to make new theories at the contemporary frontier of a science. Due to his semantical views Hanson had not examined the use of figures of speech, and very few discovery systems existed before his death in 1967. But in his

examination of historical episodes in the history of science he recognized and documented cases in which semantics has operated as a *constraint* upon discovery, and he understood that this phenomenon implies the need for a reconsideration of the nature of scientific language, especially the language for observation. However, he himself had only suggested a metatheory of semantical description in his discussion of the semantics of Newton's mechanics.

The following commentary is divided into five topics: Firstly Hanson's attempt at a logic of discovery with his wholistic *gestalt* semantics is critiqued. Secondly Hanson's defense of the Copenhagen interpretation with its duality thesis is considered in the context of semantical change in science. Thirdly Hanson's principal criticism of Bohm's hidden-variable thesis is viewed in historical retrospect. Fourthly some comments are given on Bohm's and Hesse's use of metaphor, and Wittgenstein's family-resemblance theory of meaning is critiqued. And finally a semantical metatheory of analogy, metaphor, and simile is set forth.

Consider firstly Hanson's proposed logic of scientific discovery, which took as its point of departure Peirce's investigations. Peirce's abductive (AKA retroductive) logic of discovery does not conclude to a unique theory from a given set of premises as deductive logic concludes to a unique theorem. And Hanson does not propose that there exists a resolution for this indeterminacy, much less does he supply one. But Hanson adds something to Peirce, namely the controlling role of logical syntax in the determination of semantics, which in turn strongly influences the selection of possible hypotheses available for abduction. Thus he says that the mathematical formalism or syntax of the empirically adequate quantum theory defines the conceptual possibilities for any future development of microphysical theory, while paradoxically he also maintains that it offers a conceptual resistance to any future development of an alternative microphysical having a different formalism. This controlling role for syntactical structure in statements and equations believed to be true implies an artifactual thesis of the semantics of language. But in spite of the importance that Hanson places on semantics, he never used or developed a systematic philosophy of language. His principal inspiration was Wittgenstein's *Investigations*, which is not without its insights but is an aphoristic approach to philosophy of language. In his discussion of "seeing" Wittgenstein employed ambiguous drawings such as are commonly used in texts on *gestalt* psychology, and Hanson developed a semantics of language based on the idea of the conceptual *gestalt*. Unfortunately Gestalt

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psychology is a very blunt instrument for semantical analysis, because it is a wholistic approach to semantical description.

Hanson's philosophy of scientific discovery was greatly influenced by the physicist Paul Dirac. Dirac had told Hanson that the Copenhagen interpretation figured essentially in his development of the formalism of his relativistic quantum theory. Hanson therefore took the position that the Copenhagen interpretation (without Bohr's naive epistemology based on forms of perception) is that one, unique, and distinctive semantical interpretation supplied by the formalism itself, and is not merely some philosophical idea appended to the formalism. However, the *gestalt* semantics is not adequate to the defense of Hanson's view of that Copenhagen interpretation is integral to the formalism of the modern quantum theory. Had Dirac said just the opposite of what Hanson reports he said about the Copenhagen interpretation's relation to the formalism of quantum theory, then the *gestalt* semantics would have been neither more nor less serviceable for a semantical analysis of quantum theory. This is because the conceptual *gestalt* is wholistic and does not enable the philosopher of science to separate or even distinguish the semantics that may in some way be integral to the quantum theory's formalism, from that which may not be integral to the formalism but is merely appended to the formalism - what Hanson calls mere philosophy and Bohm calls informal language. In fact Hanson's *gestalt* semantics does not even offer him a basis for his distinction between the Copenhagen interpretation and the Bohr interpretation. The wholistic character of the conceptual *gestalt* makes it impossible to partition the semantics of the quantum theory into parts, to identify those parts that are integral to the formalism and those parts that are not, or those parts that are properly called the Copenhagen interpretation and those parts that are distinctive to the Bohr interpretation. In *Patterns of Discovery* Hanson had a brief flirtation with the idea that the meanings of terms contain each other as parts, but he failed to explore the idea. Had he done so, he would have found that semantics can be as analyzable as the syntax of any semantically interpreted and empirically warranted text.

The wholistic character of the conceptual *gestalt* also thwarts Hanson's attempt to explain scientific discovery. On the one hand the conceptual *gestalt* offers conceptual resistance to any change to a new *gestalt* and therefore to any new theory. In other words it is an impediment to the semantical change integral to scientific discovery. On the other hand it is also a guide to scientific discovery, because it informs the scientist of the kind of hypothesis that may satisfy the retroductive logic of scientific

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discovery. Semantics may function in both of these contrary ways, but the *gestalt* psychology cannot explain how. More specifically in connection with the modern quantum theory, the *gestalt* psychology does not explain why Hanson should be defending the Copenhagen interpretation as a guide instead of attacking it as an impediment to the discovery of a new and more empirically adequate quantum theory. The reason for this problem is the basic fact that the wholistic *gestalt* cannot function in a logic of scientific discovery or in any other application of logic, because its wholistic character deprives the retroductive logic of any procedural character. Retroduction can only describe the conditions that the new *gestalt* must satisfy after it has been hit upon, which is to say that it is a statement of a scientific problem that the discovery must solve rather than a procedure for obtaining a solution. On the *gestalt* view the discovery itself is a transition that does not admit to a procedure, just as the transition from one interpretation of an ambiguous drawing to another does not admit to a procedure. Just as there could never be a logical or mathematical formalism to describe the transition occurring in a change of a substantial form described in Aristotle's physics, so too there could never be a logical formalism to describe the change in a change of a *gestalt* form in modern physics. In both cases the transition from one form to the other is a substitution, which is instantaneous, whole and complete, and with no intelligible continuity to warrant calling it a process instead of a simple replacement.

Turn next to the second topic, Hanson's defense of the Copenhagen interpretation and his view that the formalism of the equations and statements of the theory necessarily imply it. The central question is whether the semantics of physical theory is exhaustively specified by the equations of the theory together with the statements describing the measurement apparatus and procedures used to obtain the measurement data related by the equations, or whether additional discourse is involved characterizing the domain of the equations and measurements. Hanson rejects any semantical role in scientific explanation for any discourse other than the equations of the theory and the statements required for experimental description and measurement procedures. Accordingly he maintains that the wave-particle duality, which is the distinctive characteristic of the Copenhagen interpretation, is not some semantics added to the formalism of the quantum theory by those statements that he calls mere philosophy, but rather is an ontological claim that is expressed by the formalism due to the formalism's control of the semantics of the theory. His motive for stating this position is Dirac's statement made personally to Hanson that the wave-

particle duality is integral to the formalism, and that it was strategic in Dirac's development of his own relativistic quantum theory. And it is built into the syntax of Dirac's operator calculus.

There are physicists who disagree with Hanson's view. Some disagree because they do not recognize the occurrence of semantical change. Hanson illustrates the phenomenon of semantical change in the first chapter of his *Concept of the Positron*, where he gives a brief historical overview of the wave and particle theories of light. He notes that Newton did not have a semantics for the terms "wave" and "particle" making the concepts dichotomous or mutually exclusive, when Newton proposed his theory of fits. Only later did these concepts assume their dichotomous implications, when the experiments of Foucault, Frenzel, and Young were believed to have the force of crucial experiments that persuaded the physicist that they must decide between one and the other characterization. Thus the concepts of wave and particle had undergone semantical change with the advance of physical experiment and theory. By the twentieth century the wave-particle dichotomy had become very well established even though the discoveries of Planck's quantum constant in 1900, Einstein's equation for the photoelectric effect for light in 1905, Compton's equation for his Compton effect for light in 1922, and de Broglie's relation for matter waves in 1924 enabled physicists to express the wave-particle duality mathematically prior to development of the modern quantum theory by Heisenberg and Schrödinger. Interestingly in his *Conceptual Development of Quantum Mechanics* (1966) Max Jammer observed that Bohr had come to his complementarity principle by consideration of these earlier equations, and he references a four-page postscript to a paper written by Bohr in 1925. This is one year before Heisenberg reports that Bohr had developed his complementarity principle. Yet in spite of having been led by these considerations to conclude that wave and particle are alternative manifestations of the same physical reality, the inconsistent concepts were retained by Bohr, because he retained the classical concepts of wave and particle in his complementarity principle and relegated mathematical formalism to an instrumentalist status, even as he affirmed the wave-particle duality. His complementarity principle is a contradiction resulting from his belief in the naturalistic philosophy of perception, which in turn implies that like all classical concepts, those of wave and particle cannot be changed. And the complementarity principle is an example of the philosophical discourse defining the semantics in a way that is inconsistent with the semantics defined by acceptance of the mathematically expressed theory. After some weeks of disagreement with

Bohr, Heisenberg concluded that he could accommodate Bohr's complementarity thesis by accepting the idea that the wave-particle duality is expressed by the uncertainty principle, save that the mathematical formalism of the uncertainty principle is consistent while the complementarity principle is inconsistent. Heisenberg made this accommodation, because he accepted Bohr's naturalistic philosophy of perception. Yet in so doing, he was himself philosophically inconsistent, since unlike Bohr, he did not construe the formalism instrumentally. Instead by accepting Einstein's admonition that the theory decides what the physicist can observe, Heisenberg let his theory decide what the physicist observes, and furthermore following Einstein's precedent applying scientific realism to the concept of time in relativity theory, Heisenberg likewise attempted to construe his indeterminacy relations realistically.

The only way the Copenhagen wave-particle duality thesis can be affirmed consistently is to let the equations control the semantics of the terms "wave" and "particle", as these terms relate to the descriptive variables in the mathematically consistent formalism. Accepting this mathematical context produces a semantical change in the meanings of the terms with the result that they no longer stand for classical concepts and are therefore no longer antilogies. The empirical adequacy of the quantum theory demonstrated after testing enables its equations to function as definitions. This amounts to using the equations of the theory in a functionally *a priori* manner and as pattern statements, as Hanson said, and to letting the theory decide what is observed, as Einstein said. Heisenberg may have been approaching the recognition of the semantical change, when in his "Questions of Principle" (1935) he said the restrictions on classical concepts as enunciated in the uncertainty relations acquire their "creative value" only by making them questions of principle, such that they can have the freedom necessary for a noncontradictory ordering of experience. In the light of his autobiographical description of his development of the uncertainty relations, his phrase "creative value" may be taken to refer to the role of the mathematical equations in defining the semantics, when the concepts of the formalism are used for observation as in the case of his reconsideration of the tracks in the Wilson cloud chamber. In other words he recognized that the formation of a new semantics is integral to the new scientific discovery. In this paper Heisenberg also states that the system of mathematical axioms of quantum mechanics entitles the physicist to regard the question the simultaneous determination of position and impulse values as a false problem, just as Einstein's relativity theory makes the question of absolute

time a false question in the sense that they are devoid of meaning. Clearly the reason Heisenberg said such questions become devoid of meaning, is that the meanings of the variables have been changed by the in-principle maneuver of giving semantical control to the new theory.

Hanson reiterates Heisenberg's in-principle approach. In the chapter "Elementary Particle Physics" in his *Patterns of Discovery* he states that one cannot maintain a quantum-theoretic position and still aspire to the day when the difficulties of the uncertainty relations have been overcome, because this would be like playing chess and yet hoping for the day when the difficulties of having but one king piece will have been overcome. But Hanson proceeds beyond Heisenberg. Heisenberg's explicit and systematic theory of semantical change, his doctrine of closed-off theories developed under the influence of Bohr, was not only intended to explain semantical change, but was also intended to explain semantical permanence for classical concepts used for observation. In contrast Hanson said that the uncertainty principle is built into every observation of every fruitful experiment since 1925. In Hanson's explicit and systematic philosophy of science, unlike Heisenberg's, the theory controls even the semantics of the language used for description of observed phenomena. Hanson states how a theory has its creative value in ways that Heisenberg actually used and chronicled in his development of the uncertainty principle, but which Heisenberg did not incorporate into his explicit and systematic philosophy, his doctrine of closed-off theories. Heisenberg was inconsistent when he viewed the semantics of the variables in the mathematical quantum theory as classical concepts with restricted applicability for observation.

One problematic and indeed controversial outcome of the semantical change resulting from giving semantical control to the formalism of the theory, as Hanson advocates, is a complication in the problem of how empirical control is also exercised over the theory in scientific criticism, such that independent evidence enabling empirical decidability is possible and tautology is prevented. This is a problem that still vexes those contemporary Pragmatists who employ a wholistic thesis of the semantics of language. Hanson could have called upon his thesis of theory-independent phenomenalist seeing as an observation language. But he never invokes this idea to defend the empiricism of science, even while he never doubts either the empirical decidability of science or the theory-laden character of observation language. Instead he regrettably invokes Wittgenstein's idea of the multiple uses of language with theory language having a concept-

defining function for observation only in some uses and a testing function in others. This seems no better than Heisenberg's inconsistency.

Letting the consistent mathematical formalism of the theory control its semantics and thus the ontology its semantics describes, enables the new theory to supply a new semantics and ontology. But recognition of semantical change does not resolve the central ontological issues associated with the quantum theory. In fact there is no compelling evidence either from experiment or from the formalism of the quantum theory for the Copenhagen ontology. Whether the wave and particle are two alternative manifestations of the same entity, as Bohr and Heisenberg say, or whether they are copresent but separate entities, as de Broglie and Bohm say, or whether the particle is the only real entity, as Lande says, or whether the wave is the only real entity, as Schrödinger says - are all different ontological commitments that cannot be decided by reference to the mathematical syntax, because mathematics does not reference entities, or in Carnap's phraseology it is not a "thing language". The mathematical syntax does not express instantiation in things or entities like the syntax of the Aristotelian categorical logic, the Russellian predicate calculus, or ordinary language.

In the mathematical equations the semantically interpreted calculus expresses the universal claim, when no numeric measurement values are assigned to the descriptive variables. And the claim is made particular when any of the variables are assigned numeric values either by measurement actions of the experimenter or by calculation with the equation from measurement values assigned to other descriptive variables in the equation. The individual measurement action is the referenced instance at a specific place and time, and no claim is made about instantiated entities. In categorical logic on the other hand entities are explicitly referenced by the subject term, which is quantified, and their existence is claimed by the copula, a form of the verb "to be", when the considered categorical statement is proposed as true. Similarly in the Russellian predicate calculus quantified (or bound) variables also reference entities, although in the Russellian predicate calculus ontology and quantification are commingled so that the syntax implies nominalist ontology, such that one may blithely ignore such subtleties as simple or personal supposition, and say with Quine that in the Russellian predicate calculus to be is to be the value of a variable. In ordinary substantive discourse reference to entities is often implicit, but can be made explicit with terms such as "thing" or "entity".

In order for any mathematically expressed theory to make ontological claims about entities, it is necessary to supplement its mathematical

language with additional thing-language discourse having the syntactical categories that enable reference to entities. This could be as elementary as a statement of a measurement procedure in terms of counting certain types of entities, such as members of a population. Even if the mathematics were set theory, it would be necessary to add information identifying which sets have elements as entities. Thus there is merit in Bohm's thesis that the interpretation of a mathematical formalism is something that is in the informal language and not in the measurements or the equations themselves. And he was furthermore correct in maintaining that the informal language contains philosophical assumptions, because the statements of test design used to make quantum-theory measurements do not describe the microphysical entities adequately to decide between the various ontological interpretations. Thus any discourse purporting to describe the one or more microphysical entities in terms of wave and particle attributes must be relegated to what Hanson called mere philosophy. In this respect whether or not Bohm's hidden-variable interpretation is the correct interpretation, he seems to have been philosophically correct in stating that the interpretation is in the informal language, and that the discourse is philosophical, because that informal discourse is not yet empirically testable. Thus it is not yet empirically decidable - and the ontological debate goes on.

In conclusion the thesis of scientific realism is that the descriptive terms occurring in universally quantified statements accepted as true describe reality, because the statements are empirically warranted. Thus each ontological interpretation for quantum theory can be construed as realist, but only to the extent that the theories are empirically warranted. Thus scientific realism does not resolve issues of ontology. Due to ontological relativity the empirical under-determination of language carries over into ontology, and the unresolved ontological issues in quantum theory result from the empirical underdetermination of the theory and its associated test design language. Consider the following analogy for the quantum theory measurement problem: A survey researcher asks a respondent to express his agreement (or disagreement) about a viewpoint using a scale of 1 to 10. The respondent answers stating a value on the scale, and the interviewer dutifully records the measurement. Two ontological scenarios are possible in this measurement situation: 1. The respondent had an opinion and made his response by recalling his opinion. 2. The respondent had no opinion but formed one upon being asked, and issued his response accordingly. Both scenarios yield the same response and valid empirical measurement, just as the quantum measurements are empirically valid. The issue of when the

respondent's opinion was formed is a supplementary consideration to be resolved by further inquiry. So too with the time of the formation of the electron's wave or particle manifestations or its position or momentum. In anticipation of empirical evidence from future inquiry the physicist like the survey researcher can indulge in ontological speculation. And both Einstein and Heisenberg like many others indulged in such speculation. Einstein's realist ontology resembles the first survey respondent scenario, and Heisenberg's realist ontology of *potentia* the second.

Thirdly consider Hanson's principal criticism of Bohm's hidden-variable interpretation of quantum theory. Hanson's criticism is that Bohm has not developed any new empirically testable equations. Initially Bohm had proposed his hidden-variable hypothesis as a heuristic for developing new microphysical equations that would resolve the renormalization problem, as well as unify physics with an ontology that is consistent for both macrophysics and microphysics. For forty years he elaborated his interpretation of the existing quantum theory formalism, while the postulated subquantum field has remained remote from experimental detection, and while the renormalization problem remains unsolved. In his "Hidden Variables and the Implicate Order" in *Quantum Implications* Bohm admits that his proposed hidden-variable interpretation did not catch on among physicists, since it gives exactly the same predictions for all experimental results as does the Copenhagen interpretation, which he calls the usual theory.

Hanson's critique of Bohm's hidden-variable interpretation in his "Postscript" in *Quanta and Reality* seems to have been vindicated to date by the behavior of the physics profession in the years that have since elapsed notwithstanding Bell's nonlocality theorem. There is no shortage of sociological and conspiracy theories about the exclusion of Bohm and his supporters. Some philosophers of science as well as supporters of Bohm claim that the advocates of the Copenhagen interpretation have imposed some kind of hegemony on the physics profession. Bohm claims in his *Undivided Universe*, that the Copenhagen interpretation prevails only because it was prior to his interpretation, and says that it is merely a historical circumstance if not an accident that the Copenhagen interpretation was chronologically prior to his alternative interpretation.

But such claims reveal a failure to understand the institutional value system of empirical science that guides and motivates scientists' opportunistic decisions - including the decision by the majority to ignore Bohm's hypotheses about phenomena occurring at an order of magnitude

that is still experimentally undetectable. Physicists exhibit what Bell calls a pragmatic attitude. Feynman's sum-over-paths approach to quantum theory notwithstanding, physicists are not interested in alternative interpretations for their own sake, i.e. interpretations that are not associated with new and empirically testable equations that solve problems which the current mathematical physics has yet to solve. In fact the whole issue of alternative semantical and ontological interpretations for the quantum theory's formalism is often ignored in textbooks on quantum theory. Instead researchers in microphysics have allocated their time and effort to theorizing about the wealth of new data made available with the particle accelerators by developing the standard model and by developing string theory to account for gravitation as well. Eventually new experimental techniques and apparati will enable physicists to detect and examine subquantum phenomena (it would be quite remarkable if in fact absolutely *nothing* actually exists at subquantum orders of magnitude, as Bohr had thought). The question as to whether Bohm's hidden-subquantum-field thesis or the string theory thesis elementary point particle composition will eventually enjoy the glorious destiny of the hidden-atomic theory of matter, or whether it will eventually suffer the inglorious denouement of the hidden-ether theory of light, remains to be seen.

Finally consider Bohm and Hesse's comments on metaphor. Their differences not withstanding, Bohm and Hanson have a common belief underlying their interests in scientific discovery. Traditionally it was thought that language has merely a passive role, such that firstly a discovery is made by observation of nature, and then language is employed to report the discovery. But Hanson, Bohm, and later Hesse rejected the naturalistic philosophy of the semantics of language, which assigns to language such a passive role in scientific discovery. Instead they recognized that language has an active role that enables language construction to function as an instrument or heuristic and thus to admit to a discovery strategy. In their writings retroduction, analogy, and metaphor represent such semantical discovery strategies. But to date neither their semantical strategies using figures of speech nor even Thagard's computational efforts employing his analogical discovery strategy, have yielded new and consequential theories for any science. The inspiring muses of ancient Greek mythology are still as operative in the use of figures of speech for scientific discovery, as they are for poetry and music. While metaphor is not yet serviceable as a discovery *procedure*, it may be recognized as an outcome of mechanized discovery

procedures due to the very unconventional statements generated as system outputs.

Hesse's reliance on Wittgenstein's family-resemblance theory of meaning, however, is unfortunate. Wittgenstein noted that humans are able to distinguish individuals without articulately characterizing the individuals' distinguishing features or attributes, and to group of individuals without characterizing their common features or attributes that make them similar and that serve as the basis for grouping. But so too can dogs and cats, neither of which practice scientific research. Hesse draws upon this banal observation, and then confronts her readers with the dichotomous choice between Aristotle's natural-kinds doctrine and Wittgenstein's family-resemblance doctrine. This is a false dichotomy. It is also a rhetorical one, since Aristotle's philosophy of natural kinds, substantial forms, and species has accumulated a long baggage train of implications and associated ideas during the interim two thousand years, and few contemporary philosophers would welcome being harnessed to pull this baggage train. But Wittgenstein's family-resemblance theory of meaning is a poor alternative. As a wholistic theory of meaning, it is an exercise in vagueness about vagueness. Furthermore semantical differences are not reducible to only differences in *degree* of similarity or difference. Few concepts are like the color words, such as shades of red, which Hesse uses as an example. If meanings may be said to be approximate, as Hesse maintains, it is because they are vague. And if meanings may be said to be similar or different, it is because they are fundamentally complexes that may share many or only a few discrete semantic components, which may be called semantic values. When they share many components, or semantic values, they are similar, and when they share few, they are dissimilar. Furthermore, Hesse is not even consistent with her Wittgensteinian theory of meaning. For example in a discussion of how science can reclassify observed phenomena she notes the case in which whales become classified as mammals and not fish, because the property of suckling their young comes to be a more salient property than the fact that they live in the sea. Clearly this property of suckling young is a difference between mammals and fish that is not a matter of degree or reducible to such. A more adequate theory of meaning description than the family-resemblance thesis is needed, and a proposed alternative is set forth immediately below.

Consider the following metatheory of meaning and of figures of speech such as metaphor, which does not propose that meanings are somehow continuous with one another such that differences and similarities

are fundamentally matters of degree. As a linguistic phenomenon metaphor may be explained with the semantical thesis that the meanings of descriptive terms have complex composition. For purposes of analysis metaphor may be viewed in the context of predication to form a sentence.   Other modes of expression such as phrases or texts larger than sentences may reveal metaphorical use, when these expressions are transformed grammatically into the subject-predicate sentence form. One of the identifying features of a metaphorical description is that if the term that is metaphorically predicated of a subject is taken in its literal, i.e. conventional sense, then the statement is false, although this is a feature only for metaphors occurring in affirmative predications. For example in his *Mental Leaps* Thagard notes that the statement "No man is an island" is not literally false, even though "island" is also denied metaphorically of "man" in the statement. Another feature is that when the statement is false, it is not an unrecognized mistake; it is deliberately issued with no intention to deceive and for the purpose of revealing something believed to be true. Thus, there is merit to Bohm's definition of metaphor as the simultaneous equating and negating of two concepts.   The central problem, therefore, is how the metaphorical description can be both true and false. One possible answer is that metaphor is a kind of equivocation, and this proposal seems inevitable so long as meanings are viewed as simple wholes, such that the metaphorical description is completely true on its one meaning and completely false on its other.

A more suggestive way to formulate the question is to ask how the metaphorical predication can be partially true and partially false rather than simply true and simply false simultaneously. This suggests an alternative to simple equivocation, because it suggests that meanings have parts. A metaphorical predication invokes only part of the meaning complex associated with the descriptive predicate, and it excludes the remainder of the meaning complex. A speaker's conventional linguistic usage associates the entire meaning complex with the predicate term, and the metaphor is false if the term is predicated with its full and conventional semantics. But the speaker or writer of the metaphor recognizes the part of the meaning which is truly predicated of the subject, and he implicitly expects the hearer or reader to suspend other parts of the predicate's semantics, while the speaker or writer uses the portion that he wishes for describing the subject. A listener or reader may or may not succeed in understanding the metaphorical use of the predicated term depending on his ability to select the applicable parts of the predicate's semantics intended by the speaker or

writer. Some authors discussing metaphor, such as Max Black, render it as a kind of esoteric mode of speech, which cannot be reduced to literal language. But in fact metaphors are explained in literal (i.e. conventional) terms to the uncomprehending listener or reader.   To explain the metaphorical predication of a descriptive term to a subject, is to list those sentences or clauses believed to be true of the subject, which may substitute for the predicated metaphor, and which set forth precisely those parts of the predicate's meaning that the issuer intends to be applicable.   And the explanation may also be elaborated by listing those sentences or clauses that are not believed to be true of the subject, but which are conventionally associated with the predicated term when it is predicated literally.   These negative sentences state what is intended to be excluded from the predicate's meaning complex in the metaphorical usage.

For example to explain the metaphor "Man is a wolf", the speaker may say, "Man is a wolf, because man is ..., and man is ..., and...” where in the clauses he substitutes predicates that identify those characteristics of wolf that he intends to be applicable to man. And if in this substitute predication he finds himself further using metaphorical descriptions, then the substitution process is repeated with other clauses, until the entire explanation is literal. The explanation may be elaborated for clarity by the sentence "Man is not a wolf, because man is not..., and man is not..., and....” Substitutions in this negative sentence results in subordinate clauses that have predicates describing characteristics conventionally associated with wolves, but which the issuer of the metaphor does not intend to be truly predicated of men. The affirmative explanatory sentence sets forth those parts of the meaning associated with "wolf" that are intended to describe man in the metaphorical use of "wolf", and the negative explanatory sentence sets forth whatever parts of the conventional or literal meaning associated with "wolf" that the issuer intends to suspend for metaphorical purposes. Semantical change for the term "wolf" occurs when the metaphorical predication becomes conventional, and this produces an equivocation.   The equivocation consists of two literal meanings, the original one and a second meaning, which is now a dead metaphor. As a dead man is no longer a man, so too a dead metaphor is no longer a metaphor; it is a meaning from which the suspended parts have become conventionally excluded to produce a second literal meaning. The dead metaphor may also be a change of meaning in which the first meaning has become archaic. This may occur in some cases of scientific discovery or theory development. The new theory supersedes an old one, such that the

old meaning becomes as archaic as the old theory containing it, and the new meaning eventually becomes the only conventional meaning applicable to the subject of the superseding theory. However, the change cannot be a complete semantical change, if the fact that the new and old theories address the same subject cannot even be detected. The semantical change applies only to some parts of the term's meaning with other parts providing the needed semantical continuity, namely those supplied by statements of test design.

Simile is similar to metaphor except that the occurrence of the terms "like" or "as" alerts the listener that only part of the meaning complex is applicable, and with explanatory elaboration it may furthermore inform him of which parts. With the listener thus alerted, his awareness of the partial applicability of the predicate's meaning complex enables him to retain the term's conventional semantics. Unlike metaphor the simile is not partly true and partly false, but is wholly true, if it is true at all, even if the expressed similarity signified by the applicable part of the meaning intended by the issuer of the simile, is not the same as the meaning part selected by the listener. Thus the simile "Man is like the wolf" may be explained with the sentence "Man is like the wolf, because man is..., or man is..., or...." The terms "like" or "as" alone only inform the listener that the full meaning of "wolf" is not applicable, but the added "because…" clause explains what parts of the meaning complex are applicable.

Consider next analogy. In a conventional generic sense the term "analogy" might include metaphor and simile, because they are all figures of speech expressing similarity. But in its more restrictive sense based on the idea of a grammatical form, it is a compound sentence having two independent clauses connected with the conjunction "as". The typical form is "A is to B as C is to D." For example: "The electron is to the atomic nucleus as a planet is to the sun." This sentence may have appended to it a subordinate "because" clause explaining the underlying similarity consisting of both electrons and planets moving in orbits around a center having a relatively greater mass. There may be many such explanatory clauses explaining various underlying similarities, and perhaps also describing dissimilarities. Hesse's thesis of positive, negative, and neutral analogy would seem to pertain to such explanatory clauses. The positive analogy is what is expressed in the explanatory clauses, the negative analogy is what is expressed in the clauses describing dissimilarities, and the neutral analogy consists either in what is not yet considered, or more usefully what is actually considered and expressed with much more hypothetical attitude than

the affirmed similarities and dissimilarities. It is the neutral analogy that Hesse considers to be of distinctive value for formulating scientific theories as hypotheses proposed for testing. In the research context, instead of the literary or poetic context motivated by aesthetic considerations, the central feature of the analogy statement is that one of the independent clauses connected by "as" is believed to be true with a high degree of confidence if not conviction, while the credence status of the other independent clause is much more hypothetical in the judgment of the issuer. Historically in the above example of analogy, the solar-system description involving planets in orbits around the sun was believed much more firmly than the description of the atom in terms of electrons moving in orbits around the nucleus of the atom, which at the time was a much more tentative hypothesis. And semantically the predicate "planet" in the clause with the higher degree of credence has the idea of orbits built into its associated meaning complex, while the more hypothetical attitude toward the description of the atom deprived the predicate "electron" of the idea of orbits as a component part.

Both metaphor and simile too may be said to have positive, negative, and neutral aspects in the context of scientific discovery. The positive aspect of either a metaphor or a simile consists of those parts of the meaning complex associated with the predicate term that are also conventionally included in the meaning complex associated with the subject term, and that are the basis for the affirmed similarity. Conversely the negative aspect consists of those parts of the meaning complex associated with the predicate term that are not also conventionally included in the meaning complex associated with the subject term. And the neutral aspect consists of those parts of the meaning complex that the issuer has not considered in connection with the meaning complex associated with the subject term, but which he may consider at a later time. As a figure of speech, this later consideration involves reflection on the semantics associated conventionally with the predicate terms. But if the later consideration involves new empirical research either by formulating a new hypothesis or by examination or consideration of a test outcome, then there is a semantical change that has not yet become conventional. For example at one time a proposed metaphor was "the electron is a small orbiting planet", and the corresponding simile is "the electron is like a small orbiting planet." At that time these components of meaning were not conventionally included in the concept of electron.

HERBERT SIMON, PAUL THAGARD AND OTHERS ON DISCOVERY SYSTEMS

Herbert Simon is the principal figure considered in this chapter. This chapter's material is presented in reverse chronological order, and the exposition therefore starts with the work of Paul Thagard, who follows Simon's cognitive-psychology orientation for his computational philosophy of science investigations. Thagard's philosophy of science is rich, and lends itself to exposition in terms of the four basic topics in philosophy of science. But before considering Thagard's treatment of the four topics, consider firstly his psychologistic views on the nature of philosophy of science and the semantics of conceptual change in scientific revolutions.

Thagard's Psychologistic Computational Philosophy of Science

Thagard is a Professor of Philosophy at the University of Waterloo since 1992, and is also Adjunct Professor of Psychology and Computer Science, Director of his Computational Epistemology Laboratory, and Director of the Cognitive Science Program. He has been an associate professor of philosophy at University of Michigan, Detroit, where he was associated with their Cognitive Sciences Program, and a Senior Research Cognitive Scientist at Princeton University. He is a graduate of the University of Saskatchewan, Cambridge, Toronto (Ph.D. in philosophy, 1977) and the University of Michigan (M.S. in computer science, 1985).

Computational philosophy of science has become the new frontier in philosophy of science in recent years, and it portends to become essential to and definitive of twenty-first century philosophy of science. There are many philosophers now jumping on the bandwagon by writing about the computational approach in philosophy of science, but only authors who have actually designed, written and exhibited such computer systems are

considered in this chapter of this book. Thagard is one of the handful of academic philosophers of science, who has the requisite technical skills to make such contributions, and has demonstrated them by actually writing such systems. His work is also selected because in the closing decades of the twentieth century he is one of the movement's most prolific authors and most inventive academic philosophers of science.

Thagard follows the artificial-intelligence approach and psychological interpretation of the AI systems previously proposed by Herbert Simon, who is one of the founding fathers of artificial intelligence. In his *Computational Philosophy of Science* (1988) Thagard explicitly proposes a concept of philosophy of science that views the subject as a type of cognitive psychology. The linguistic-analysis tradition in philosophy had achieved ascendancy in twentieth-century philosophy of science. The analysis of language has been characterized by a nominalist view, also often called "extensionalism or the "referential theory of meaning." The nominalist view proposes a two-level semantics, which recognizes only the linguistic symbol, such as word and sentence, and the objects or individual entities they reference. It recognizes no third level consisting of the idea, concept, "intension" (as opposed to extension), proposition, or any other mental reality mediating between linguistic signs and nonlinguistic objects. The two-level semantics is the view typically held by the Positivist philosophers, who rejected mentalism in psychology and preferred behaviorism. Thagard explicitly rejects the behavioristic approach in psychology and prefers cognitive psychology, which recognizes mediating mental realities. The two-level semantics is the view that is also characteristic of philosophers who accepted the Russellian predicate calculus. This calculus of symbolic logic contains a notational convention that uses quantification to express existence claims. It therefore fabricates a nominalist newspeak in which predicate terms are semantically vacuous, unless they are placed in the range of quantifiers, such that they reference some kind of entities, called either "mental entities" or Platonic "abstract entities." The philosopher Nelson Goodman for example divides all philosophers into nominalists and Platonists. Not surprisingly the Russellian symbolic logic was adopted by the Logical Positivists. Oddly Thagard does not reject the Russellian symbolic logic, although it is not clear that he recognizes the ontological implications of its notational conventions. His turn away from linguistic analysis and toward psychologism has been motivated by recognition of the mentalistic semantical level. Like Simon, Thagard wants to admit the existence of the mental semantical level, so that he can investigate concepts by viewing computer systems as analogs for the mental realities, and then

hypothesize about the human cognitive processes of scientists on the basis of the computer system designs and procedures. He refers to this new discipline as "computational philosophy of science", the name that will probably become the conventional one for this area specialty. And he defines computational philosophy of science as an attempt to understand the structure and growth of scientific knowledge in terms of computational and psychological structures with the aim of offering new accounts both of the nature of theories and explanations and of the processes underlying their development. Thagard distinguishes computational philosophy of science from cognitive psychology by the former's normative perspective.

In his *Mind: Introduction to Cognitive Science* (1996), intended as an undergraduate textbook, he states that the central hypothesis of cognitive science is that thinking can best be understood in terms both of representational structures in the mind and of computational procedures that operate on those structures. He labels this central hypothesis with the acronym "CRUM", by which he means "Computational Representational Understanding of Mind." He says that this hypothesis assumes that the mind has mental representations analogous to data structures and computational procedures analogous to algorithms, such that computer programs using algorithms applied to data structures can model the mind and its processes.

His *How Scientists Explain Disease* (1999) reveals some evolution in his thinking, although this book reports no new computer-system contribution to computational philosophy of science. In the book he examines the development of the bacteriological explanation for peptic ulcers. He finds that collaboration, communication, consensus, and funding are important for research, and he uses the investigation to propose an integration of psychological and sociological perspectives for a better understanding of scientific rationality. He also states that principles of rationality are not to be derived a priori, but should develop in interaction with increasing understanding of human cognitive and social processes.

Thagard's computational philosophy of science addresses the topics of discovery, criticism, explanation, and the aim of science. He has created several computer systems for computational philosophy of science, none of which produce mathematically expressed theories. And all of his systems have been applied to the reconstruction of past episodes in the history of science. None of his systems have been applied to the contemporary state of any science, either to propose any new scientific theory or to forecast the resolution of any current scientific theory-choice issue.

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Thagard on Conceptual Change, Scientific Revolutions, and System PI

Thagard's semantical views are set forth in the opening chapters of his *Conceptual Revolutions* (1992). He says that previous work on scientific discovery, such as *Scientific Discovery; Computational Explorations of the Creative Process* by Langley, Simon, Bradshaw, and Zytkow in 1987 has neglected conceptual change. (This important 1987 work is discussed below in the sections reporting on the views and systems developed by Simon and his colleagues.) Thagard proposes both a general semantical thesis about conceptual change in science and a thesis specifically about theoretical terms. His general thesis is that (1) scientific revolutions involve transformations in conceptual and propositional systems, (2) kind-hierarchies and part-hierarchies structure conceptual systems, and (3) relations of explanatory coherence structure propositional systems. His theory of explanatory coherence is his philosophy of scientific criticism, which is described separately below. Consider firstly his general semantical thesis.

Thagard opposes his psychological account of conceptual change to the view that the development of scientific knowledge can be fully understood in terms of belief revision, the prevailing view in analytic philosophy. He says that his view is that concepts are mental representations that are largely learned and are open, i.e. not defined in terms of necessary and sufficient conditions. He maintains that a cognitive-psychology account of concepts and their organization or structure in hierarchies shows how a theory of conceptual change can involve much more than belief revision. He notes that such hierarchies are important in **WORDNET**, an electronic lexical reference system. Thagard states that an understanding of conceptual revolutions requires seeing how concepts can fit together into conceptual systems and seeing what is involved in the revolutionary replacement of such systems. He says conceptual systems consist of concepts organized into kind-hierarchies and part-hierarchies linked to one another by rules. This idea suggests the ancient tree-hierarchical arrangement proposed by the third-century logician Porphyry, which Umberto Eco says in his *Semiotics and Philosophy of Language* is a "disguised encyclopedia." It is not clear why Thagard believes that these structures cannot be expressed in language and explained by belief revision, unless he mistakenly associates belief revision with the nominalism of analytic philosophy.

Thagard maintains that a conceptual system can be analyzed as a computational network of nodes with each node corresponding to a concept, and each line in the network corresponding to a link between concepts. The

most dramatic changes involve the addition of new concepts and especially new rule-links and kind-links, where the new concepts and links replace ones from the old network. Thagard calls the two most severe types of conceptual change "branch jumping" and "tree switching", and says that neither can be accounted for by belief revision. Branch jumping is a reorganization of hierarchies by shifting a concept from one branch of a hierarchical tree to another, and it is exemplified by the Copernican revolution in astronomy, where the earth was reclassified as a kind of planet instead an object *sui generis*. Tree switching is the most dramatic change, and consists of reorganization by changing the organizing principle of a hierarchical tree, and it is exemplified by Darwin's reclassification of human as animal while changing the meaning of classification to a historical one. He also says that adopting a new conceptual system is more "holistic" than piecemeal belief revision. Historically the term "holistic" was opposed to any analysis, but clearly Thagard is not opposed to analysis; "systematic" would be a better term in his context.

In his *Computational Philosophy of Science* Thagard references Willard Van Quine's statements that science is a web of belief, a connected fabric of sentences that faces the tribunal of sense experience collectively, all susceptible to revision and adjustment like the planks of a ship. He agrees with Quine, but adds that Quine does not go far enough. Thagard advocates a more procedural viewpoint and the abandonment of the fabric-of-sentences metaphor in favor of more complex cognitive structures and operations. He concludes that the web of beliefs does not consist of beliefs, but rather consists of rules, concepts, and problem solutions, and the procedures for using them. By way of commentary, it may be said that Thagard's theory of conceptual change is a theory of conceptual organization rather than a theory of meaning description enabled by accepting a defining role for beliefs. A belief is any unit of language that may be true or false, and that is accepted as true for any reason including notably reasons acceptable in science. And once accepted as true, the meaning of its subject term is defined in part by the meaning associated with the descriptive predicate in the believed statement thereby offering a partial meaning description of the subject term. Thus belief revision occasions a change in definition, and thereby both produces and describes conceptual change including revolutionary change in science. It may be added that kind-hierarchies and part-hierarchies can be expressed linguistically in statements believed to be true, as even ancient logicians had recognized.

In *Conceptual Revolutions* Thagard maintains that continuity is maintained through the conceptual change by the survival of links to other

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concepts, and he explicitly rejects Kuhn's thesis that scientific revolutions are world changes. He says that old and new theories have links to concepts not contained in the affected theories, and he cites by way of example that while Priestly and Lavoisier had very different conceptual systems describing combustion, there was an enormous amount on which they agreed concerning many experimental techniques and findings. He also says that he agrees with Hanson's thesis that observations are theory-laden, but he maintains that they are not theory-determined. He says that the key question is whether proponents of successive theories can agree on what counts as data, and that the doctrine that observation is theory-laden might be taken to count against such agreement, but that the doctrine only undermines the Positivist thesis that there is a neutral observation language sharable by competing theories. He states that his own position requires only that the proponents of different theories be able to appreciate each other's experiments. This view contrasts slightly with his earlier statement in his *Computational Philosophy of Science*, where he said that observation is inferential. He says that observation might be influenced by theory, but that the inferential processes in observation are not so loose as to allow us to make any observations we want. He adds that there are few cases of disagreement about scientific observations, because all humans operate with the same sort of stimulus-driven inference mechanisms. This statement is not enlightening, since Thagard does not describe this inferential process he claims occurs in observation. It should be commented that in both his earlier and later statements Thagard has finessed the vexing problem of meaning variance that arises due to the theory-laden nature of observation language. Without a theory of meaning description he cannot characterize the concepts in language used for observation, and thus cannot explain how descriptive terms can be theory-laden. Since beliefs can function as partial definitions, they are both empirical and analytical statements that enable analysis of the composition in the concept or meaning associated with a descriptive term. Beliefs thereby reveal the meaning components defined in terms of a theory that make the meaning theory-laden due to the context supplied by the theory. And they also reveal the meaning components defined in terms of the observation and experimental results that are not in the theory, and that supply the descriptive language needed for independent empirical testing. Thus Thagard is correct in saying that continuity is maintained through the conceptual change by the survival of links to other concepts, i.e. the nontheory concepts, but he does not explain how it occurs. It occurs because the links to those other concepts constitutes the linguistic context that is believed to be true, that occurs in the language used to report observation,

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and that supplies the components to the meaning complex that are unaffected by theory change.

Consider next Thagard's thesis specific to theoretical terms. Both Thagard and Simon accept the ideas of theoretical and observation terms, and both use the distinction in some of their computer systems. In these systems the theoretical terms are those developed by a system and the observation term are those inputted to the system. But in both their literatures the distinction between theoretical and observation terms has a philosophical significance apart from their roles in their systems. Thagard says that new theoretical concepts arise by conceptual combination, and that new theoretical hypotheses, i.e. propositions containing theoretical terms, arise by abduction. Abduction including analogy is his philosophy of scientific discovery, which is described separately below. Thagard's belief in theoretical terms suggests a residual Positivism in his philosophy of science. But he attempts to distance himself from the Positivists' foundations-of-science agenda and their naturalistic philosophy of the philosophy of the semantics of language. But he rejects assuming a strict or absolute distinction between theoretical and observable entities, and says that what counts as observable can change with technological advances. Therefore Thagard does not have the Positivists' problem with the meaningfulness of theoretical terms. But he retains the distinction thus modified, because believes that science has concepts intended to refer to a host of postulated entities and has propositions containing these theoretical concepts that make such references. These propositions have concepts that refer to nonobservable entities, and these propositions cannot be derived by empirical generalization due to the unavailability of any observed instances from which to generalize. He subscribes to the semantical thesis that all descriptive terms - observational terms as well as theoretical terms - acquire their meanings from their functional role in thinking. Thus instead of a naturalistic semantics, he admits to a relativistic semantics. However, while Thagard subscribes to a relativistic theory of semantics, he does not recognize the contemporary Pragmatist view that a relativistic semantical view implies a relativistic ontology, which in turn implies that all entities are theoretical entities. For example Quine calls relativistic ontological determination "ontological relativity", and says that all entities are "posits" whether microphysical or macrophysical. From the vantage of the contemporary Pragmatist philosophy of language the philosophical distinction between theoretical and observation terms is anachronistic. Thagard could retire these linguistic anachronisms "theoretical" and "observational" as needless paleo-Positivist fossils, if instead he used the

terms "endogenous" and "exogenous" respectively, which are used by contemporary modelers to distinguish between the descriptive terms developed by a system and those inputted to it.

Thagard collaboratively with Keith J. Holyoak developed an artificial-intelligence system called **PI** (an acronym meaning "Process of Induction"), which among other capabilities creates theoretical terms by conceptual combination. In view of the above discussion it may be said that in the expository language used in science all descriptive terms - not just Thagard's theoretical terms - have associated with them concepts which are combinations of other concepts functioning as semantic values structured by the set of beliefs in which they occur. Thagard's system **PI** system is described in "Discovering the Wave Theory of Sound: Inductive Inference in the Context of Problem Solving" in *IJCAI Proceedings* (1985) and in his *Computational Philosophy of Science*. **PI** is written in the **LISP** computer programming language. In a simulation of the discovery of the wave theory of sound, **PI** created the theoretical concept of sound wave by combining the concepts of sound and wave. The sound wave is not observable, while instances of water waves and sound have been observed. In **PI** the combination is triggered when two active concepts have instances in common. However, most combinations of concepts of observables are uninteresting, but **PI** only forms permanent combinations when the constituent concepts produce differing expectations, as determined by the rules for them in **PI**. In such cases **PI** reconciles the conflict in the direction of one of the two donor concepts. In the case of sound wave the conflict is that water waves are observed in a two-dimensional water surface, while sound is perceived in three-dimensional space. In **PI** the rule that sound spreads spherically is stronger than the rule that waves spread in a single plane. Strength is a parameter developed in the operation of the system. Thus the combination of the three-dimensional wave is formed. The meaningfulness of this theoretical term is unproblematic for Thagard, due to his functionalist view of semantics, which gives the theoretical term its meaning by the rules, concepts, and messages in **PI**.

Thagard on Discovery by Analogy and Systems ACME and ARCS

In *Conceptual Revolutions* Thagard distinguishes three types or methods of scientific discovery. They are: 1) data-driven discovery by simple abduction to make empirical generalizations from observations and experimental results, 2) explanation-driven discovery using existential

abduction and rule abduction to form theories referencing theoretical entities, and 3) coherence-driven discovery by making new theories due to the need to overcome internal contradictions in existing theories. To date Thagard has offered no discovery system that creates new theories by the coherence-driven method, but the other two methods have been implemented in his cognitive systems.

Consider firstly generalization. The central activity of artificial-intelligence system **PI** is problem solving with the goal of creating explanations. The system represents knowledge by rules and concepts with nodes in a network representing concepts and the rules linking the nodes representing propositions. Generalization is the formation of general statements, such as may have the simple form "All X are Y." The creation of such rules by empirical generalization is implemented in **PI**, which takes into account both the number of instances supporting a generalization, and the background knowledge of the variety in the kinds of instances involved.

Consider next abduction. By "abduction" Thagard means inference to a hypothesis that offers a possible explanation of some puzzling phenomenon. The **PI** system contains three complex data structures or data-types in named **LISP** property lists, which are called "messages", "concepts", and "rules." The messages data-type represents particular results of observations and inferences. The concept data-type locates a concept in a hierarchical network of kinds and subkinds. The concepts manage storage for abductive problem solving. The rules data-type represents laws in an "if…then" form, and also contains a measure of strength. The system fires rules that lead from the set of starting conditions to the goal of explanation. Four types of abductive inference accomplish this goal: (1) Simple abduction, which produces hypotheses about individual objects; these hypotheses are laws or empirical generalizations. (2) Existential abduction, which postulates the existence of formerly unknown objects; this type results in theoretical terms referencing theoretical entities, which is discussed in the previous section above. (3) Rule-forming abduction, which produces rules that explain other rules; these rules are theories that explain laws. Since Thagard retains a version of the doctrine of theoretical terms referencing theoretical entities, he advocates the Positivists' traditional three-layered view of the structure of scientific knowledge consisting of (a) observations expressed in statements of evidence, (b) laws based on generalization from the observations, and (c) theories, which explain the laws. (4) Analogical abduction, which uses past cases of hypothesis formation to generate hypotheses similar to existing ones.

Consider specifically analogy. This topic is treated at length in Thagard's *Mental Leaps: Analogy in Creative Thought* (1995) co-authored with Holyoak. In this book the authors propose a general theory of analogical thinking, which they illustrate in a variety of applications drawn from a wide spectrum. Thagard states that analogy is a kind on nondeductive logic, which he calls "analogic." Analogic contains two poles, as it were. They are firstly the "source analogue", which is the known domain that the investigator already understands in terms of familiar patterns, and secondly the "target analogue", which is the unfamiliar domain that the investigator is trying to understand. Analogic then consists of the way the investigator uses analogy to try to understand the targeted domain by seeing it in terms of the source domain, and it involves a mental leap, because the two analogues may initially seem unrelated, but the act of making the analogy creates new connections between them. Thagard calls his theory of analogy the "multiconstraint theory", because he identifies three regulating constraints: (1) similarity, (2) structure, and (3) purpose. Firstly the analogy is guided by a direct similarity between the elements involved. Secondly it is guided by proposed structural parallels between the roles in the source and target domains. And thirdly the exploration of the analogy is guided by the investigator's goals, which provide the purpose for considering the analogy. Thagard lists four purposes of analogies in science. They are (1) discovery, (2) development, (3) evaluation, and (4) exploration. Discovery is the formulation of a new hypothesis. Development is the theoretical elaboration of the hypothesis. Evaluation is arguments given for its acceptance. And exploration is the communication of new ideas by comparing them to the old ones. He notes that some would keep evaluation free of analogy, but he maintains that to do so would contravene practice of several historic scientists. Each of the three regulating constraints - similarity, structure, and purpose - is operative in four steps that Thagard distinguished in the process of analogic: (1) selecting, (2) mapping, (3) evaluating, and (4) learning. Firstly the investigator selects a source analogy often from memory. Secondly he maps the source to the target to generate inferences about the target. Thirdly he evaluates and adapts these inferences to take account of unique aspects of the target. And finally he learns something more general from the success or failure of the analogy.

Thagard notes two computational approaches for the mechanization of analogic: the "symbolic" approach and the "connectionist" approach. The symbolic systems represent explicit knowledge, while the connectionist systems can only represent knowledge implicitly as the strengths of weights associated with connected links of neuron-like units in networks. Thagard

says that his multiconstraint theory of analogy is implemented computationally as a kind of hybrid combining symbolic representations of explicit knowledge with connectionist processing. Thagard and Holyoak have developed two analogic systems: **ACME** (Analogical Constraint Mapping Engine) and more recently **ARCS** (Analog Retrieval by Constraint Satisfaction). Reflecting in 1987 on interpreting the Necker cube, a kind of ambiguous drawing, Holyoak and Thagard worked together to develop a procedure whereby a network could be used to perform analogical mapping by simultaneously satisfying the four constraints. Their result was the **ACME** system. This system mechanizes the mapping problem. It creates a network when given the source and target analogues, and a simple algorithm updates the activation of each unit in parallel, to determine which mapping hypothesis should be accepted. **ARCS** deals with the more difficult problem of retrieving an interesting and useful source analog from memory in response to a novel target analog, and it must do so without having to consider every potential source analog in the memory. The capability of matching a given structure to those stored in memory that have semantic overlays with it is facilitated by information from **WORDNET**, an electronic thesaurus in which a large part of the English language is encoded. The output from **ARCS** is then passed to **ACME** for mapping.

Thagard on Criticism by "Explanatory Coherence"

Thagard's theory of explanatory coherence set forth in detail in his *Conceptual Revolutions* describes the mechanisms whereby scientists choose to abandon an old theory with its conceptual system, and accept a new one. He sets forth a set of principles that enable the assessment of the global coherence of an explanatory system. Local "coherence" is a relation between two propositions. The term "incohere" means that more than just two propositions do not cohere; i.e. they resist holding together. The terms "explanatory" and "analogous" are primitive terms in the theory, and the following principles define the meaning of "coherence" and "incoherence" in the context of his principles, as paraphrased and summarized below:

Symmetry.
   If propositions P and Q cohere or incohere, then Q and P cohere or incohere respectively.

Coherence.
   The global explanatory coherence of a system of propositions depends on the pairwise local coherence of the propositions in the system.

Explanation.

> If a set of explanatory propositions explain proposition Q, then the explanatory propositions in the set cohere with Q, and each of the explanatory propositions cohere with one another.

Analogy.

> If P_1 explains Q_1, P_2 explains Q_2, and if the P's are analogous to each other and the Q's are analogous to each other, then the P's cohere with each other, and the Q's cohere with each other.

Data Priority.

> Propositions describing the results of observation are evidence propositions having independent acceptability.

Contradiction.

> Mutually contradictory propositions incohere.

Competition.

> Two propositions incohere if both explain the same evidence proposition and are not themselves explanatorily connected.

Acceptability.

> The acceptability of a proposition in a system of propositions depends on its coherence with those propositions. Furthermore the acceptability of a proposition that explains a set of evidence propositions is greater then the acceptability of a proposition that explains only a subset or less than the number in the set including a subset.

Thagard's theory of explanatory coherence is implemented in a computer system written in the **LISP** computer language that applies connectionist algorithms to a network of units. The system is called **ECHO** (Explanatory Coherence by Harmony Optimization). Although Thagard mentions a coherence-driven discovery method, his **ECHO** system is not a discovery system. Before execution the operator of the system inputs the propositions for the conceptual systems considered by the system, and also inputs instructions identifying which hypothesis propositions explain which other propositions, and which propositions are observation reports and have evidence status. In **ECHO** each proposition has associated with it two values, a weight value and an activation value. A positive activation value represents a degree of acceptance of the hypothesis or evidence statement, and a negative value the degree of rejection. The weight value represents the explanatory strength of the link between the propositions. When one of principles of explanatory coherence in the above list says that one proposition coheres with another, an excitatory link is established between the two propositions in the computer network. And when one of the principles says that two incohere, an inhibitory link is established. In summary in the **ECHO** system network: (1) A proposition is a unit in the

network. (2) Coherence is an excitatory link between units with activation and weight having a positive value, and incoherence is an inhibitory link with activation and weight having a negative value. (3) Data priority is an excitatory link from a special evidence unit.   (4) Acceptability of a proposition is activation.   Prior to execution the operator has choices of parameter values that he inputs, which influence the system's output. One of these is the "tolerance" of the system for alternative competing theories, which is measured by the absolute value of the ratio of excitatory weights to inhibitory weights. If the tolerance parameter is low, wining hypotheses will deactivate losers, and only the most coherent will be outputted.

When **ECHO** runs, activation spreads from the special evidence unit to the data represented by evidence propositions, and then to the explanatory hypotheses, preferring those that firstly explain a greater breadth of the evidence than their competitors, and secondly explain with fewer propositions, i.e. are simpler. But the system prefers unified theories to those that explain evidence with special *ad hoc* hypotheses for each evidence statement explained. Thagard says that by preferring theories that explain more hypotheses, the system demonstrates the kind of conservatism seen in human scientists when selecting theories.   And he says that like human scientists **ECHO** rejects Popper's falsificationism, because **ECHO** does not give up a promising theory just because it has empirical problems, but rather makes rejection a matter of choosing among competing theories. However, whether scientists reject theories in isolation depends on how one individuates theories, and Thagard offers no criterion for individuating theories.   If theories are individuated semantically, then when a theory makes inaccurate predictions, the response by scientists is to change the theory, thereby *ipso facto* creating a new theory regardless of whether there are alternatives. But **ECHO** is not a discovery system, and therefore is not designed to make this kind of response.   And thirdly the system prefers explanations that are analogous to other previously successful explanations. In his *Computational Philosophy of Science* he notes that many philosophers of science would argue that analogy is at best relevant to the discovery of theories but has no bearing on their justification. But he maintains that the historical record, such as Darwin's defense of natural selection, shows the need to include analogy as one of the criteria for the best explanation among competing hypotheses. In summary, therefore, other things being equal activation accrues to units corresponding to hypotheses that: (1) explains more evidence, (2) provide simpler explanations, or (3) are analogous to other explanatory hypotheses. These three criteria are also operative in his earlier **PI** system, where breadth is called "consilience." During execution

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the system proceeds through a series of iterations adjusting the weights and activation levels, in order to maximize the coherence of the entire system of propositions. Thagard calls the network "holistic" in the sense that the activation of every unit can potentially have an affect on every other unit linked to it by a path, however lengthy. Usually not more than one hundred cycles are needed to achieve stable optimization. The maximized coherence value is calculated as the sum of each of the weight values multiplied by the activation value of the propositions associated with each weight.

Thagard has applied system **ECHO** to several revolutionary episodes in the history of science. These include: (1) Lavoisier's oxygen theory of combustion, (2) Darwin's theory of the evolution of species, (3) Copernicus' heliocentric astronomical theory of the planets, (4) Newton's theory of gravitation, and (5) Hess' geological theory of plate tectonics. In reviewing his historical simulations Thagard reports that the criterion in **ECHO** having the largest contribution to explanatory coherence in scientific revolutions is explanatory breadth – the preference for the theory that explains more evidence than its competitors – as opposed to the other criteria of simplicity and analogy.

ECHO seems best suited either to evaluate nonmathematically expressed alternative theories, or to evaluate mathematically expressed alternative theories in only certain circumstances. Scientists like to quantify phenomena, so that they can compare the prediction errors in their theories net of the estimated measurement error. They estimate measurement error by repetition of the measurement procedures, and they reduce it by improvement in their experimental designs. It is in cases of empirical indeterminacy that considerations such a breadth, simplicity, and analogous similarity may operate in the scientists' preferences among mathematically expressed theories. Those are cases of nonfalsified theories having prediction errors that are large relative to measurement error, yet small relative to the deviations between the alternative theories' prediction errors, such that the measurement error makes the theories empirically indistinguishable.

Thagard on Explanation and the Aim of Science

Thagard's views on the three levels of explanation were mentioned above, but he has also made some other statements that warrant mention. In *Conceptual Revolutions* he distinguishes six different approaches to the topic of scientific explanation in the philosophy of science literature, the first five

of which he finds are also discussed in the artificial-intelligence literature. The six types are: (1) deductive, (2) statistical, (3) schematic - which uses organized patterns, (4) analogical, (5) causal – which he opposes to specious correlation, and (6) linguistic/pragmatic. For the last he finds no correlative in the artificial-intelligence literature. Thagard says that he views these approaches as different aspects of explanation, and that what is needed is a theory of explanation that integrates all these aspects. He says that in artificial intelligence such integration is called a cognitive architecture, by which is meant a general specification of the fundamental operations of thinking, and he references Simon's General Problem Solver agenda. He adds that some of these approaches may operate as subprocesses in the complex process of explanation.

The topic of the aim of science has special relevance to Thagard's philosophy, since he defines computational philosophy of science as normative cognitive psychology. Thagard's discussions of his theory of inference to the best explanation implemented in his system **PI** set forth in *Computational Philosophy of Science* and his later statement as the theory of optimized explanatory coherence implemented in his system **ECHO** set forth in *Conceptual Revolutions*, reveal much of his view on the aim of science. His statement of the aim of science might be expressed as follows: to develop hypotheses with maximum explanatory coherence including coherence with statements reporting available empirical findings. He notes that no rule relating concepts in a conceptual system will be true in isolation, but he maintains that the rules taken together as a whole in a conceptual system constituting an optimally coherent theory provide a set of true descriptions. In *Computational Philosophy of Science* Thagard states that his theory of explanatory coherence is compatible with both realist and nonrealist philosophies. But he maintains that science aims not only to explain and predict phenomena, but furthermore to describe the world as it really is, and he explicitly advocates the philosophical thesis of scientific realism, which he defines as the thesis that science in general leads to truth. Thagard's concept of "scientific realism" seems acceptable as far as it goes, but it does not go far enough. The meaning of "scientific realism" in the contemporary Pragmatist philosophy of science is based upon the subordination of ontological claims to empirical criteria in science, a subordination that is due to the recognition of ontological relativity.

Herbert Simon and Logic Theorist

Herbert Simon (1916-2001), born in Milwaukee, Wisconsin, entered the University of Chicago in 1933 where he received a BA degree in 1936 and a Ph.D. in political science in 1942. He was awarded the Nobel Memorial prize for economics in 1978. He spent his career as a faculty member at Carnegie-Mellon University in Pittsburgh, most of it in the Graduate School of Industrial Administration, and later as a faculty member in both the Psychology and Computer Science Departments and also as a member of the University's board of trustees. His autobiography, *Models of My Life*, was published in 1991.

In his autobiography he reports that the most important years of his life were 1955 and 1956, when his interest turned from administration and economics to the psychology of human problem solving, and specifically to considering the symbolic processes that people use in thinking. He and his long-time collaborator, Alan Newell, had concluded that computers could be applied generally to imitating intelligence symbolically, instead of just numerically, an insight that Simon says is a crucial step required for genuine artificial intelligence to emerge. In 1956 his first artificial-intelligence system named **LOGIC THEORIST** used his "heuristic search" methods to develop deductive logic proofs of the theorems in Whitehead and Russell's *Principia Mathematica*, the seminal text for the Russellian symbolic logic. However, the fact that this system found proofs in formal logic is purely incidental; Simon rejects the view held by some artificial-intelligence advocates, that formal logic is the appropriate language for artificial-intelligence systems and that problem solving is merely a process of proving theorems. The significance of **LOGIC THEORIST** is its use of the authors' "heuristic search" methods and of symbol manipulation. Simon defines artificial intelligence as symbolic processing, and he defines cognitive psychology as understanding human thinking by modeling ordinary problem solving with artificial-intelligence systems.

Newell and Simon have developed many artificial-intelligence systems, several of which are described in their book titled *Human Problem Solving* (1972). Simon views scientific discovery as a special case of human problem solving, and therefore maintains that it can be examined with the artificial-intelligence approach. However, his artificial-intelligence systems development work was not directed to scientific discovery until later in the 1970's. His principal publications pertaining to scientific discovery are *Models of Discovery* (1977), which contains reprints of some of his earlier articles relating information processing concepts to scientific discovery, and

most notably his *Scientific Discovery; Computational Explorations of the Creative Process* (1987), which describes several discovery systems that rediscovered various historic scientific laws and theories. Just as examination of the evolution of the contemporary Pragmatist philosophy of science requires consideration of the issues in physics and especially quantum theory, similarly examination of the development of the artificial-intelligence discovery systems requires consideration of issues in the social sciences and especially economics. Therefore, to appreciate Simon's views on scientific discovery, it is necessary to consider his views on human problem solving by artificial-intelligence systems. And to appreciate his views on human problem solving, it is informative to consider what he calls his most important contribution to economics, his postulate of bounded rationality. And to appreciate Simon's postulate of bounded rationality, it is helpful firstly to review both the various alternative rationality postulates and Max Weber's semantical thesis of "ideal types."

Neoclassical Maximizing Rationality and Weber's Ideal Types

Simon proposes his thesis of bounded rationality as an alternative to two other concepts of rationality that have currency among economists. The principal alternative to Simon's view is the prevailing neoclassical postulate, which says that consumers are rational because they maximize their utility, and that producers are rational because they maximize their profits. The second alternative to Simon's is the rational expectations postulate, which is a distinctive extension of the neoclassical postulate of utility and profit maximization. The rational expectations view will be considered below in the discussion of the **BVAR** type of discovery system. And since the rational expectations postulate is an extended version of the neoclassical view, Simon's critique of neoclassicism also applies to the rational expectations thesis, which he explicitly rejects. Simon's bounded rationality postulate is similar to an earlier view originating in the U.S. called "Institutionalist economics", which will also be examined below. Before turning to Simon's bounded rationality postulate, however, consider firstly the still prevailing view in academic economics, the neoclassical rationality postulate.

The neoclassical postulate of rationality has its origins in Adam Smith's doctrine of self interest set forth in his *Wealth of Nations* (1776), the seminal document for modern economics. Smith was greatly impressed by Isaac Newton's celestial mechanics. In his *Essay on the History of*

Astronomy Smith described Newton's celestial mechanics as the greatest discovery ever made by man, and Smith aspired to describe economic life as a harmonious mechanism, as Newton had done for the heavens. In Smith's system entrepreneurs' rational behavior in pursuit of their economic self-interest unintentionally produces a beneficial and harmonious outcome for the national economy; this is his doctrine of the "invisible hand." However Smith's perspective is not closed or self-contained. It was part of a larger moral universe of natural laws, which Smith had earlier described in his *Theory of Moral Sentiments* (1759). In Smith's natural-law philosophy the pursuit of economic self-interest is morally constrained by men's natural sympathy for others and also by their natural desire for the approval of others - a distinctively sociological idea. Later economists excluded from theoretical economics Smith's moral constraints on the pursuit of self-interest. In the twentieth century these constraints came to be recognized as sociological or institutional structures instead of natural moral laws, and an attempt to re-introduce them into economic analysis was made by the American Institutionalists.

Almost one hundred years after the *Wealth of Nations* a new development occurred in economic theory, which is now called the "marginalist revolution", and which might also be described as the completion of Smith's agenda for a Newtonian economics. The term "marginal" means incremental or differential, and the incremental economic analysis lends itself to mathematical expression with the differential calculus developed by Newton. The result is an elegant mathematical rendering of economic theory, in which the rationality postulate became a matter of calculating the global maximization of consumer utility and producer profits by suitable values for the first and second derivatives of the relevant mathematically expressed demand functions. The resulting theory of price determination describes the allocation of goods and services in an optimally efficient manner later called "Pareto optimality" after the Italian economist, Vilfredo Pareto. A half century later there was another revolution called the "Keynesian revolution" named after the economist, John Maynard Keynes. Pre-Keynesian economic theory had assumed that the Pareto optimum resulting from rational maximizing behavior by each individual would also maximize income and output for the whole economy, as Adam Smith and the marginalists had believed. In his *General Theory* (1936), however, Keynes set forth a new thesis saying that individual maximizing behavior could result in less-than-full-employment equilibrium, which he said had occurred during the Great Depression of the 1930's. This resulted in economists' dividing economics into the "microeconomic" theory of price

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determination and the "macroeconomic" theory of national income determination. Keynes thus produced a revolution in economic theory, but he did not explicitly attack the classical economists' rationality postulate of individual human behavior. His stagnation thesis of underemployment equilibrium only attacked the classical economists' optimistic thesis of a maximizing macroeconomic outcome.

Soon afterwards economists began applying statistical inference techniques to estimate equations with the macroeconomic data that were being collected by Nobel Laureate economist Simon Kuznets of the National Bureau of Economic Research, in order to describe national economic conditions. Both the availability of these data and the development of the computer occasioned the evolution of a specialty area in economics called "econometrics", although earlier there were Institutionalist economists whose statistical analyses of economic data have also been called econometrics. Since Haavelmo, however, nearly all the econometricians have been neoclassical economists, who require that the selection of variables for the equations constituting the econometric model be "justified" by neoclassical theory. Thus, until very recent years econometrics was exclusively the application of statistical inference and testing techniques to economic data structured by neoclassical microeconomic and macroeconomic theory. Even today any econometric model that does not result from such *a priori* imposition of the neoclassical theory upon the data is derisively referred to as "atheoretical." In this respect neoclassical economics still bears a burdensome legacy from the Romantic era in the history of European culture.

The above overview of the neoclassical rationality postulate of human behavior reveals that it is not viewed by economists as just one of many alternatives; it has served as the foundation for modern economics since its founder, Adam Smith. Anyone attempting to overthrow the use of maximizing rationality postulates is attempting a new scientific revolution in economics that would be much more radical than any of the revolutionary developments within the history of neoclassical theory. Nevertheless, there have been dissenters such as the American Institutionalists, and the reason for their dissent has always been the empirical inadequacy and heroic unrealism of the neoclassical theory with its basis in rationality postulates. Neoclassical theorists have not been completely unaware of these problems caused by their maximizing rationality postulates. Before turning to Simon's alternative, consider briefly Max Weber's thesis of the "ideal type", a semantical contrivance proposed to defend the neoclassical rationality

concept against its critics. Simon does not refer to Weber, but Weber explicitly proposes the same ideas that Simon explicitly opposes.

Weber's discussion of his doctrine of the ideal type or "*idealtypus*" can be found in English translation from the German in *The Methodology of the Social Sciences* (Tr. by Shils and Finch, 1949), and principally in the chapters titled "'Objectivity' in Social Science and Social Policy" and "The Meaning of 'Ethical Neutrality' in Sociology and Economics", and in *Max Weber's Ideal Type Theory* (1969) by Rolf E. Rogers. Weber's philosophy of sociology contains ambiguities that have been noted by recognized Weberian scholars including "Weber's dilemma", which is discussed below. The ideal type is distinctive of the interpretative understanding to which cultural sociology aims. Weber defined the ideal type as a mental construct that has two basic features: Firstly it involves one or several points of view. According to Weber's theory of knowledge this perspectivism is characteristic of all concepts including both natural science and social science concepts, because no concept can capture reality in all its potentially infinite variety of aspects. Weber explicitly rejects the copy theory of knowledge, which he finds in the Historicist philosophy of social science, and he refers to the Historicists' claim of pure objectivity in science as the "naturalistic prejudice." In the present context what is noteworthy is that the rational aspect of human behavior is a central aspect and perspective of reality that Weber includes in the ideal-type concepts in pure economic theory. The second of the two features of the ideal type is that it involves a one-sided accentuation or intensification of the perspective or point of view in the ideal type concept. Nonrational considerations are not denied, but the maximizing postulate is knowingly made unrealistically extreme as a limiting case. Weber explicitly rejects the charge that the ideal type is a complete fiction, but he calls it "utopian", since historical concrete individuals do not conform in their behavior to the accentuated, maximizing rationality described by the ideal type.   Thus individual instances not conforming to pure economic theory do not falsify the theory containing the ideal-type concepts; as Weber states, the ideal type is not a hypothesis, and it is not tested by its application to reality. Weber says that the ideal type is used to compare theory to reality, in order to reveal by contrast the irrational aspects of human behavior. What neoclassical economists call "pure theory" utilizes ideal-type concepts exclusively, and it makes certain assumptions, notably the maximizing assumptions, which almost never correspond completely with reality but rather approximate reality in varying degrees.

The ideal type is a semantical contrivance like Heisenberg's concept of a closed-off theory, because it is what Popper calls a "content-decreasing

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stratagem" to evade falsification. It is unfalsifiable, because it is protected from falsifying evidence by the stratagem of restricting its applicability in the face of contrary evidence and thus of denying its falsification. Pure economic theory with its ideal-type concepts is true where it is applicable, and it is not nonapplicable wherever it would be falsified. In other words all observed human behavior is "rational" and suitable for economic analysis wherever neoclassical economic theory applies to it, and it is "irrational" and unsuitable for economic analysis wherever the theory does not apply. If there is anything that distinguishes the ideal type thesis, it is that the evading denial of the falsifying consequence of contrary evidence is very explicit. It may also be noted that when the Weberian neoclassical economist compares his ideal type with observed behavior in order to detect irrational behavior, he is not using it as a counterinductive "detecting device" as Feyerabend advocates. When Galileo was confronted with the Aristotelian tower argument opposing the Copernican heliocentric theory, Galileo's response was to revise the language describing observation. And when Heisenberg was confronted with the apparently Newtonian track of the free electron in the Wilson cloud chamber, his response too was to revise the Newtonian language describing the observation. These are examples of counterinduction. But when the Weberian neoclassical economist is confronted with observed anomalous "irrational" behavior, no attempt is made to reconcile the reporting language of observation with the ideal-type language of neoclassical theory, much less to revise the theory. Instead the reported anomalous observations are simply excluded from economics. The Weberian regards the observed "irrational" behavior as a phenomenon to be excluded from neoclassical theory rather than as one to be investigated either for a more empirically adequate post-neoclassical economic theory or for a new test-design language.

Many contemporary economic theorists are only less explicit in their dogmatic adherence to neoclassicism with its definitive maximizing postulates. They are reluctant to dispense with the elegantly uniquely determinate mathematical solutions enabled by merely setting the first derivative to zero and checking the second derivative for a maximum inflection point. They are scandalized by the observed absence of optimizing behavior and the rejection of their maximizing postulates, because it implies that paradigmatic problems thought to have been solved elegantly after two hundred years of theoretical development in the neoclassical tradition have not actually been solved at all. Academic economists who have dutifully labored and groveled for years to earn their doctorate credentials and publish their papers in the prestigious refereed

journals, do not welcome being advised that their purportedly empirical theory depends on a content-decreasing stratagem, a self-deceiving linguistic contrivance, which makes their received theory only slightly less semantically vacuous than the formal differential calculus used to express it, and hardly more ontologically realistic than the Ayn Rand Romanticist utopian novels used to propagandize it for the nonprofessional general public.

Yet in truth not all economists are philosophically atavistic neoclassicals. In recent years the ascendancy of econometrics has made such evasion of empiricism more difficult, because the "rational" and the "irrational" are inseparably commingled in the measurement data. The econometrician constructing models from time-series historical data would prefer to make statistically acceptable models, than to dismiss large error residuals in his statistical equations as merely "irrational" behavior that can be ignored. While the ostensible practice in academia today is still the Haavelmo agenda (discussed below), in which equations are specified on the basis of neoclassical theory, a growing number of economists are evolving into closet Pragmatists. They have turned increasingly to empirical data analysis for the determination of their equation specifications, and they include in their equations even noneconomic variables such as demographic, sociological or political variables, which are never found in textbooks preaching neoclassical microeconomic or macroeconomic theory. And some economists, such as Simon, are so heretical as to reconsider even the sacrosanct maximizing rationality postulates axiomatic to the neoclassical orthodoxy.

Simon's Postulate of Bounded Rationality and "Satisficing"

In his autobiography Simon relates that in what he calls his first piece of scientific work, a study in 1935 of public recreation in the city of Milwaukee, he saw a pattern that was the seminal insight for what was to become his thesis of bounded rationality. For this study he was examining the budgeting process for the division of funds between playground maintenance, which was administered by one organization, and playground activity leadership, which was administered by another organization in the Milwaukee municipal government. He found that the actual budget allocation decision was not made as economic theory would suggest. What actually occurred was that both of the two organizations wanted more funds for their proper functions, and he generalized from his experience with this

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budgeting decision, that people bring decisions within reasonable bounds by identifying with partial goals for which their own organizational units are responsible. This insight was taken up in Simon's Ph.D. dissertation (1942), which he later published as *Administrative Behavior* (1947), the book referred to by the Royal Swedish Academy of Sciences as an "epoch-making book", when they awarded him the Nobel Memorial Prize for Economics in 1978. In his autobiography Simon says that his entire scientific output may be described as a gloss on two basic ideas contained in his *Administrative Behavior***:** (1) human beings are able to achieve only a very limited or "bounded" rationality, and (2) as a consequence of this limitation, they are prone to identify with subgoals. The first of these ideas is fundamental to Simon's critique of neoclassical rationality, and the second is fundamental to his theory of human problem solving.

In his autobiography Simon says that his "A Behavioral Model of Rational Choice" (1955) reprinted as chapter fourteen in his *Models of Man* (1987), was his first major step toward his psychological theory of bounded rationality. In that early paper he states that the neoclassical concept of rationality is in need of fairly drastic revision, because actual human behavior in making choices does not satisfy three basic assumptions underlying neoclassical maximizing rationality. Those three assumptions are: (1) a decision maker has a knowledge of the relevant aspects of his environment, which if not absolutely complete, is at least impressively clear and voluminous; (2) a decision maker has a well organized, consistent, and stable system of preferences; and (3) a decision maker has a skill in mental computing, that enables him to calculate for the alternative courses of action available to him the one course that will enable him to reach the highest achievable point in his preference scale. Then in his "Rational Choice and the Structure of the Environment" (1956) reprinted as chapter fifteen of *Models of Man*, Simon proposes replacing the neoclassical postulate of maximizing behavior with his more modest postulate that he calls "satisficing" behavior. "Satisficing" means that instead of optimizing, the decision maker's limited information and limited computational ability require that he adapt "well enough" to achieve his goals instead of optimizing.

The first chapter of his *Sciences of the Artificial* (1969) reveals that Simon identifies exactly the same things about neoclassical rationality that Weber identified as the two basic features of the ideal-type concept. Firstly like Weber's thesis of viewpoint in the ideal type, Simon calls neoclassical rationality an "abstract idealization", because it selectively directs attention to the circumstances of the decision maker's outer environment for his

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adaptive behavior. Similarly in the chapter "Task Environments" in his *Human Problem Solving* (1972) he says that it is the task that defines the "point of view" about the environment, an idea that is comparable to Weber's thesis that the ideal type contains a point of view determined by one's interests. Secondly, just as Weber said that the accentuated rationality in the ideal type is "utopian", Simon calls neoclassical rationality "heroic" to describe its unrealistic character, and later in 1983 in his *Reason in Human Affairs* again without referencing Weber, he describes optimization as "utopian." But unlike Weber, Simon does not relegate to the status of the "irrational" all the decision making that does not conform to the neoclassical ideal type of rational maximizing behavior. Instead Simon considers the empirical inadequacy of neoclassical rationality to be good reason for replacing it with his behaviorally more realistic concept of bounded rationality.

In the second chapter of his *Sciences of the Artificial* and then in his "From Substantive to Procedural Rationality" in *Models of Bounded Rationality* Simon uses the phrase "substantive rationality" for the neoclassical maximizing rationality, which considers only the decision maker's goals and outer environment. And he uses the phrase "procedural rationality" for the satisficing psychological cognitive procedures including the decision maker's limited information and limited computational abilities consisting of what Simon calls the decision maker's inner environment. The study of cognitive processes or procedural rationality is interesting only when the substantively rational response is not trivial or instantly obvious. It is usually studied in situations in which the decision maker must gather information of various kinds, and must process it in different ways to arrive at a reasonable course of action for achieving his goals.

Simon refers to the Pareto optimality described in the economists' theory of general equilibrium, which combines the individual maximizing choices of a host of substantively rational economic actors into a global optimum for the whole economic system, as the "ideal" market mechanism. Then he says that there is also a "pragmatic" market mechanism described by the Nobel laureate economist Frederich von Hayek that is more modest and believable, because it strives for a measure of procedural rationality by realistically tailoring decision-making tasks to the limited computational capabilities and localized information available to the economic decision maker, with no promise of optimization. He quotes at length a passage from Hayek's "The Uses of Knowledge in Society" in *American Economic Review* (1945), in which Hayek asks: what is the problem we wish to solve when we try to construct a rational economic order? And Hayek answers that the

economic calculus does not describe the optimization problem, since it is a problem of the utilization of knowledge that is not given to anyone in its totality. The price system is a mechanism for communicating information, and the most significant fact about it is the economy of knowledge with which it operates, that is, how little the individual participants need to know in order to be able to take the right course of action. Simon maintains that it is Hayek's "pragmatic" version which describes the markets of the real world, and that the substantive rationality of neoclassical theory is worthless, since it is not backed up by executable maximizing algorithms. He says that consumers and business firms are not maximizers, but rather are satisficers. They accept what is "good enough" because they have no choice. The rationality that they actually use is a satisficing procedural rationality. Examination of the limits of rationality leads to consideration of the price system mainly as an institution that reduces the amount of nonlocal information which economic actors must have to make "reasonable", i.e. satisficing, decisions.

Bounded Rationality, Institutionalism, and Functionalism

Simon's description of the real-world market-determined price system as pragmatic and as an institution places him in the worthy intellectual company of the American Institutionalist school of economic thought, even though he does not identify himself as such. Therefore, a few background comments about this school of economics and about its principal advocates are in order. In the "Introduction" to his *Types of Economic Theory* the Institutionalist economist Wesley Clair Mitchell says that there have been different types of economic theory, not only because there have been different types of problems, but also because there have been different conceptions of human nature. At issue is the neoclassicals' concept of human nature, which motivated them to construct a deductive theoretical economics based on the rationality postulates. The American Institutionalist School was founded as a revolt within the American economic profession, which rejected the formal and abstract deductivism in neoclassical economics and instead appealed to experience. It had its roots in the Pragmatist philosophy, the only philosophy indigenous to the United States, which itself was a revolt in the American philosophy profession, and which rejected the natural-law and utilitarian traditions in European academic philosophy.

The founding father of American Institutionalism is the iconoclastic economist and somewhat eccentric individual, Thorstein Veblen (1857-1929). In his "Why is Economics not an Evolutionary Science?" in his *The Place of Science in Modern Civilization* (1919) Veblen characterized the neoclassical economists' hedonistic psychology as describing man as a "lightening calculator" of pleasures and pains, who passively responds to his environment and is unchanged by the environment. Veblen rejected this conception of human nature and proposed instead an anthropological conception, in which the individual's psychology is formed by institutions prevailing in the community in which a man lives, and most notably institutions which evolve. He also therefore proposed that economics itself is an evolutionary science that employs a "genetic" type of theory, which describes the cumulative cultural growth of economic institutions, instead of the "taxonomic" type of theory used by neoclassical economists such as the Austrian school. He rejects the Austrian's *ad hoc* attempts to save their natural-law explanations from deviant facts by invoking "disturbing factors." He also explicitly references Charles Darwin, and rejects the German Historicist School as pre-Darwinist for offering only enumeration of data and narrative accounts instead of genetic theory.

Another noteworthy representative of American Institutionalism is John R. Commons (1862-1945). In his *Institutional Economics* (1934) Commons states explicitly that he is following the Pragmatist philosophy of Charles S. Peirce. In the second volume of this book Commons discusses Weber's ideal-type concepts, and he criticizes their fixed and unchanging character. Commons states that the utopian character of the ideal type only becomes more utopian as scientific investigation advances. Instead of the ideal type, Commons proposes the "changeable hypothesis", that takes into account new factors revealed to be relevant in the investigation, and that retires from consideration old factors found to be irrelevant. This amounts to demanding that economics be more empirical. Weber had explicitly denied that the ideal type is a hypothesis. Commons says that use of hypotheses makes less utopian the utopias that our minds create. Commons does not explicitly propose revising the maximizing assumption in the neoclassical rationality postulate; he rejects it. A typical Institutionalist, he maintains that in addition to economic interactions described by neoclassical economics there are other, namely institutional, factors that are also operative in determining the outcomes of economic transactions. In both his earlier works and again in his final work, The *Economics of Collective Action* (1950, 1970), he proposes a more adequate psychology, which he calls a "negotiational psychology" as opposed to the hedonist psychology of

the utilitarians. He also calls it an objective and behavioristic psychology instead of the subjective psychology of pain and pleasure, because it is the psychology of language, duress, coercion, persuasion, command, obedience, propaganda, and a psychology of physical, economic, and moral powers. He therefore distinguishes three types of transactions: (1) bargaining transactions, which occur in the market, and which is the type treated in neoclassical economic theory, (2) managerial transactions, which occur between levels in organizational hierarchies, and (3) rationing transactions, which are agreements about apportioning, such as occur in budgeting decisions. He says that all three types have "futurity", that is, they require some security that future outcomes occur as expected by the participants, so that expectations can operate as working rules. He sees the three types as functionally interdependent. The Institutionalist perspective focuses on the second and third types of transactions, because these represent "collective action in control of individual action", which is Commons' explicit definition of Institutionalism. Commons was particularly interested in the social control exercised by courts over the working rules in bargaining transactions. Perhaps it is not coincidental to Commons' interests that in the 1930's before the Roosevelt Administration, the courts viewed collective bargaining by labor unions as an illegal conspiracy against trade. The second and third types of transactions, however, are the ones relevant to Simon's interests.

Simon elaborates on the relation of institutions to his thesis of satisficing bounded rationality in his "Rationality as Process and as Product of Thought" (1978) reprinted in his *Models of Bounded Rationality*. He does not explicitly refer to the academic literatures of either Pragmatist philosophy or Institutionalist economics, but instead draws upon the "functionalist" type of explanation often found in the sociological literature. He references the *Encyclopedia of the Social Sciences* (1968) in which functionalism is defined as an explanation of how major social patterns operate to maintain the integration or adaptation of larger social systems. More formally stated functional explanations are about movements of a system toward stable self-maintaining equilibriums. Most notably Simon states that there is no reason to suppose that the attained equilibria are global maxima.   In other words, functional explanation describes satisficing behavior. In this paper he furthermore maintains that functional analyses are not focused on quantitative magnitudes as are found in price theory, but are focused on qualitative and structural questions, and typically on the choice among a small number of discrete institutional alternatives. Particular institutional structures or practices are seen to entail certain desirable or

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undesirable consequences. A shift in the balance of consequences, or in the awareness of them, may motivate a change in institutional arrangements. Like economic sociologists, who recognize the underlying role of economic institutions, Simon argues that economists have in fact not actually limited themselves to maximization analyses, but have utilized such qualitative functional analyses when they seek to explain institutions and behavior that lie outside the domain of price theory, distribution, and production. In his autobiography he says most of the conclusions drawn by neoclassical economists do not depend on the assumption of perfect rationality, but derive from auxiliary institutional assumptions that are required, in order to reach any conclusions at all. And in his *Reason in Human Affairs* (1983) he says that markets do not operate in a vacuum, but are part of a larger framework of social institutions, which provide the stable environment that makes rationality possible by supplying reliable patterns of events.

In "Rationality as Process..." Simon states that the characterization of an institution is almost never arrived at deductively from consideration of the function that it must perform for system survival. Functional analysis is not deductive like theoretical neoclassical economics. Rather an institution is a behavior pattern that is empirically observed, and existence of the pattern occasions the question of why it persists, that is, what function it performs. Institutions can be observed in every society, and their existence is then rationalized by the argument that its function is requisite. But Simon comments that this kind of reasoning may demonstrate that a particular behavioral pattern is a sufficient condition for performing an essential social function, but cannot demonstrate that the particular pattern is a necessary condition. Alternative patterns may be functionally equivalent, since they serve the same need. In other words there may be many alternative satisficing institutional patterns for accomplishing the same social goal.

Human Problem Solving, Cognitive Psychology and Heuristics

Simon's theory of procedural rationality is his theory of human problem solving, and it is elaborately set forth in his *Human Problem Solving* (1972) co-authored with Allen Newell. This nine-hundred page *magnum opus* took fourteen years to write. During this period Simon also wrote a briefer statement, *Sciences of the Artificial* (1969), and several articles since reprinted in his *Models of Discovery* (1977), an anthology of many of his previously published papers. Much of *Human Problem Solving* consists of detailed descriptions of problem-solving computer programs,

none of which pertain to scientific discovery. Nonetheless his views on human problem solving are relevant to methodology of science, because he considers scientific discovery to be a special case of human problem solving. At the outset of *Human Problem Solving* the two collaborating authors state that their aim is to advance understanding of how humans think by setting forth a theory of human problem solving. The concluding section of the book sets forth a general statement of their theory, which is based on the computer programs described in the body of the book and presented as empirical evidence relevant to their theory. They state that the specific opportunity which has set the course for their book is the development of a science of information processing, more recently called computer science. Their central thesis is that explanation of thinking can be accomplished by means of an information theory, and that their theory views a human as a processor of information, an information processing system. They say that such a description of the human is not just metaphorical, because an abstract concept has been developed of an information processor, which abstracts from the distinctively mechanical aspects of the computer. The authors compare the explanations in information science to the use of differential equations in other sciences such as classical physics. An information theory consisting of computer programs is dynamic like differential equations, because it describes change in a system through time. Such a theory describes the time course of behavior, characterizing each new act as a function of the immediately preceding state of the system and its environment. Given at any time the memory contents characterizing the system's state at that moment, the program determines how the memory contents will change during the next computing cycle and what the contents will be at the end of the cycle. The fundamental methodological problems of theory construction and theory testing are the same in the two types of theory. The theory is tested by providing a specific set of initial and boundary conditions for the system, by using the equations or program to predict the resulting time path, and by comparing this predicted path with the actual path of the system. The advantage of an information-processing language over the mathematical languages for formulating a theory of thinking is that an information processing language takes symbolic structures rather than numbers as values of its variables.

The information theory about human thinking and problem solving is a theory in cognitive psychology. Newell and Simon note that their theory is concerned with performance, specifically with the performance of intelligent adults in our own culture, while psychologists have traditionally been more concerned with learning. In his autobiography as well as elsewhere Simon

distinguishes cognitive psychology from both the *gestalt* and the behavioristic approaches to psychology. He rejects the black-box approach of the behaviorists and especially of B.F. Skinner, who maintains that the black box is empty.   Simon also rejects the reductionist version of behaviorism, according to which complex behavior must be explained in terms of neurological processes, and he also rejects the neurological modeling approach of the psychologists who use parallel connectionist networks or neural nets for computerized explanations.   Newell and Simon propose a theory of symbols located midway, as it were, between complex behavioral processes and neurological processes. Simon acknowledges a debt to the Gestaltists and their allies, who also recognize a layer of constructs between behavior and neurology, but Simon rejects the Gestaltists' wholistic approach to these constructs.   Simon proposes an explicitly mechanistic type of explanation of human thinking and problem solving in terms of information processing.

Simon defines human thinking as a system of elementary information processes, organized hierarchically and executed serially. Simon relies on the concept of hierarchy as a strategy for managing complexity. He defines a hierarchical system as one that is composed of interrelated subsystems, each of which in turn is hierarchical in structure down to a lowest level consisting of an elementary subsystem. In human problem solving hierarchy is determined by the organization of subgoals, which is the second idea that Simon said is basic to his entire scientific output. Hierarchical organization is common in computer systems; applications programs are written in compiler and interpreter languages such as **FORTRAN** and **BASIC**, and these languages in turn contain reserved words that are names for macros, which are subsystems in the compiler library, which in turn contain lower level subsystems, and so on down to a basic level consisting of elementary systems in binary code. For the specifically problem-solving type of human thinking Simon has analyzed information processing into a few basic concepts. The first of these is the "task environment", by which he means the problem-solving processor's outer environment as viewed by the problem solver to produce a "problem space", together with the goal that orients the problem solver to his task environment. The problem space is the inner environment consisting of the processor's internal representation of the outer task environment, and in which the problem solving activities take place. Simon maintains that there is no objective representation of the task environment independently of some processor's problem space. Furthermore it is the task or goal that defines the "point of view" about the problem-solving processor's outer environment, and that therefore defines

the problem space. Simon calls this defining process an "input translation process." Thirdly in addition to task environment and problem space, Simon introduces the concept of "method." A method is a process that bears some "rational" relation to attaining a problem solution, as formulated and seen in terms of the internal representation, which is the problem space. Here the term "rational" is understood as satisficing in the sense that a satisfactory as opposed to an optimal solution is achieved. In the mechanical processor, the method is the computer program, and most of Simon's theory of problem solving pertains to the method.

Simon distinguishes three types of method. The first is the recognition method, which can be used when the solution is already in the processor's memory, and artificial-intelligence systems using this method rely on large stores of specific information. Computer programs using this type of method contain a conditional form of statement, which Simon calls a "production." In a production, whenever the initial conditions are satisfied, the consequent action is taken. And when the conditions of several alternative productions are satisfied, the conflicts between them are resolved by priority rules. In his autobiography Simon notes that productions have become widely accepted to explain how human experts make their decisions by recognizing familiar cues directly, and that productions have been used for the "expert systems" in artificial intelligence. Experts, both human and mechanical, do much of their problem solving not by searching selectively, but simply by recognizing the relevant cues in situations similar to those experienced before. It is their wealth of experience that makes them experts. The second type of method is what Simon calls the generate-and-test method. In this method the computer system generates a problem space, and has as its goal to find or to produce a member in a subspace identified as a solution by a test. The generality and weakness of this method lies in the fact that the generation and test procedures are independent, so that the amount of search is very large. Simon typically portrays this method as requiring a search that is so large, that it cannot be carried out completely, and so must proceed in a random manner. To address this problem of innumerable possibilities the Pragmatist philosopher C.S. Peirce had advanced his logic of abduction, which postulates a natural light or instinctive genius for producing correct theories. Simon advances instead his theory of heuristics, the third type of problem-solving method, which exploits the information in the task environment as that task environment is represented internally in the processor by the problem space. In the heuristic search, unlike the generate-and-test method, there is a dependence of the search process upon the nature of the object being sought in the problem

space and the progress being made toward it. This dependence functions as a feed back that guides the search process with controlling information acquired in the process of the search itself, as the search explores the internalized task environment. This method is much more efficient than the generate-and-test method, and it explains how complex problems are solved with both human and mechanical bounded rationality.

These alternative methods represent different artificial-intelligence research programmes, software development vs hardware development, which may also be characterized as knowledge vs speed. The generate-and-test method is dependent on fast hardware; the heuristic search method is dependent on efficient software design. Researchers preferences for one or another of the methods are affected by developments in hardware technology, as well as the magnitude of the problems they select. The hardware preference has been called the "brute force" approach, and as the technology has advanced, it has enabled the implementation of artificial-intelligence systems that offer little new software but greatly improved performance for the extensive searching of very large problem spaces. For example the *Wall Street Journal* (30 April 1990) reported that a group of five Carnegie-Mellon University graduate students with IBM Corporation funding have developed a multiprocessor chess-playing system named "Deep Thought", that exhibits grand-master performance with superhuman speed. It was reported that this system does not represent any noteworthy software development either in chess-playing search heuristics or in expert chess-playing strategies. Instead it explores the huge chess-playing problem space more quickly and extensively than can the human grand master, who is limited by human bounds to his rationality.

On Scientific Discovery and Philosophy of Science

Before Simon and his colleagues at Carnegie-Mellon University had developed functioning computerized discovery systems simulating historic discoveries, Simon had written articles claiming that scientific discovery is a special case of human problem solving. In these articles he related his human problem-solving approach for discovery, to views published by various philosophers of science. The articles are reprinted in his *Models of Discovery*, where he comments in his "Introduction" that the subject of scientific discovery and of creativity generally has always been surrounded by dense mists of romanticism and downright knownothingness. In his "Scientific Discovery and the Psychology of Problem Solving" (1966)

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Simon states his thesis that scientific discovery is a form of problem solving, i.e. that the processes whereby science is carried on can be explained in terms that have been used to explain the processes of problem solving. Problem-solving thinking is an organization of elementary information processes organized hierarchically and executed serially, and consisting of processes that exhibit large amounts of highly selective trial-and-error search based on rules of thumb or heuristics. The theory of scientific discovery is a system with these characteristics, and which behaves like a scientist. Superior problem-solving scientists have more powerful heuristics, and therefore produce adequate solutions with less search or better solutions with equivalent search, as compared with less competent scientists. Science is satisficing, and to explain scientific discovery is to describe a set of processes that is sufficient and just sufficient, to account for the degrees and directions of scientific progress that have actually occurred. Furthermore, for every great success in scientific discovery there are many failures, and a theory to explain scientific discovery must predict innumerable failures for every success.

In "Scientific Discovery and the Psychology of Problem Solving" Simon also takes occasion to criticize the philosophy-of-science literature. He maintains that the philosophy literature tends to address the normative rather than the descriptive aspects of scientific methodology, and that philosophers are more concerned with how scientists ought to proceed, in order to conform with certain conceptions of logic, than with how they do in fact proceed. And, he adds, their notions of how scientists ought to proceed focuses primarily on the problem of induction. He concludes that the philosophy of science literature has little relevance to the actual behavior of scientists, and has less normative value than has been supposed. But he finds two exceptions in the philosophy of science literature: Russell Hanson and Thomas Kuhn. He says that both of these authors have made significant contributions to the psychology and sociology of scientific discovery, and that they have been quite explicit in distinguishing the process of discovery from the traditional canons of "sound" scientific method. He also says that he has made much use of the views of both of these philosophers. Simon's principal commentary on the philosophy of Hanson is his defense of Hanson against the view of Popper in "Does Scientific Discovery Have a Logic?" (1973). He notes that Popper rejects the existence of a logic of scientific discovery in Popper's *Logic of Scientific Discovery* (1934), and he says that Popper's view is opposed by Hanson in the latter's *Patterns of Discovery* (1958) and by Peirce. Peirce used the term "retroduction", which Simon says is the main subject of the theory of problem solving in both its norma-

tive and positive forms. Simon observes that Hanson made his case by historical examples of scientific discovery, and that he placed great emphasis on discovery of perceptual patterns.

In this 1973 article as well as in his earlier "The Logic of Rational Decision" (1965) Simon distinguishes heuristic search from induction, and defends the idea of a logic of scientific discovery in the sense that norms can be derived from the goals of scientific activity. He defines the logic of scientific discovery as a set of normative standards for judging the processes used to discover or test scientific theories, where the goal from which the norms are derived is that of discovering valid scientific laws. And Simon emphasizes that the heuristic strategy does not guarantee results or success, and he therefore argues that he has not smuggled any philosophical induction axiom into his formulation of a logic of discovery, and that such a logic does not depend on the solution of the problem of induction. Simon states that discovering a pattern does not involve induction or extrapolation. Induction arises only if one wishes to predict and to test whether or not the same pattern will continue to obtain when it is extrapolated. Law discovery only means finding patterns in the data that have been observed; whether or not the pattern will continue to hold for new data that are observed subsequently will be decided in the course of predictive testing of the law, and not in discovering it. It may be noted that after Simon's colleagues had created functioning discovery systems based on heuristic search, Simon often described some of those systems as using inductive search. However, in his *Scientific Discovery* Simon explicitly rejects the search for certainty associated with attempts to justify inductivism.

Simon subscribes to Popper's falsificationist thesis of scientific criticism, and in his "Ramsey Eliminibility and the Testability of Scientific Theories" (1973) reprinted in his *Models of Discovery* Simon proposed a formalization of Popper's requirement that admissible theories be falsifiable. This formalization is his "FITness" criterion, which is a neologism containing an acronym for "Fit and Irrevocable Testability." According to this requirement a theory should be admissible for consideration if and only if (1) in principle it is falsifiable by a finite set of observations, and (2) once falsified, additional observations cannot rehabilitate it. In a footnote at the end of this paper Simon adds that the FITness criterion is only a requirement for falsifiability, and that it says nothing about the disposition of a theory that has been falsified, such that the FITness criterion is compatible with what Lakatos calls "methodological falsificationism", because methodological falsificationism permits a falsified theory to be saved by modifying the auxiliary hypotheses that connect it with observables. In his

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"Methodology of Scientific Research Programmes" in *Criticism and the Growth of Knowledge* (1970) Imre Lakatos distinguished "dogmatic falsificationism" from "methodological falsificationism", and within the latter type he further distinguished "naive" and "sophisticated" subtypes. Simon's reference to auxiliary hypotheses in his footnote suggests he believed that his FITness criterion is of the "sophisticated" subtype. But he later apparently reconsidered; in *Scientific Discovery* he called his FITness criterion "naive falsification", and gave two reasons: Firstly the criterion postulates that there are wholly theory-free observation sentences, whose truth status can be determined by observations that have no theoretical presuppositions. Secondly his criterion is too strict to be applied to any theory. Simon maintains that there are no wholly theory-free observation sentences.

Simon's comments on Kuhn's philosophy are principally concerned with Kuhn's distinction between normal and revolutionary science. Kuhn maintained that the revolutionary transition is a *gestalt* switch, while Simon defends his own view that heuristic search procedures apply to revolutionary changes as well as to normal science. In his "Scientific Discovery and the Psychology of Problem Solving" Simon says that his theory of scientific discovery rests on the hypothesis that there are no qualitative differences between the processes of revolutionary science and those of normal science, between work of high creativity and journeyman work respectively. Simon points to the fact that trial and error occurs in both types of work. He argues that trial and error are most prominent in those areas of problem solving where the heuristics are least powerful, that is, are least adequate to narrow down the problem space, such that the paths of thought leading to discoveries often regarded as creative might be expected to provide even more visible evidence of trial and error than those leading to relatively routine discoveries. Later in his *Scientific Discovery* Simon develops the idea of the amount of trial-and-error search into the distinction between "strong" methods, which he says resemble normal science, and "weak" methods, which resemble revolutionary science. He identifies expert systems based principally on productions, where there may be almost no search needed for problem solving, as paradigmatic cases of strong methods exemplifying normal science.

Simon's argument that trial and error is used in all types of discovery is his defense of the heuristic method. But method is only one aspect of his theory of problem solving; there is also the definition of the problem space. He acknowledges that scientific work involves not only solving problems but also posing them, that correct question asking is as important as correct

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question answering. And he notes that Kuhn's distinction between normal and revolutionary science is relevant to the relation of question asking and question answering. In the 1966 article Simon identifies the problem space, which is the problem solver's point of view of the outer environment, with Kuhn's idea of paradigm, and he identifies the definition of the problem space with the process of problem formation. Firstly Simon notes that normal science need not pose its own questions, because its questions have already been formulated for it by the current paradigm produced by the most recent scientific revolution. The problem space is thus given by the current state of the science; the problematic case is the scientific revolution, which establishes the new paradigm. Simon argues that it is not necessary to adduce entirely new mechanisms to account for problem formulation in revolutionary science, because, as Kuhn says, the paradigms of any given revolution arise out of the normal science of the previous period. Normal science research leads to the discovery of anomalies, which are new problems that the prospective revolutionaries address. Therefore Simon argues that there is no need for a separate theory of problem formulation. He states that a theory of scientific discovery adequate to explain revolutionary as well as normal science must account not only for the origin of the problems, but also for the origins of representations, the problem spaces or paradigms. Representations arise by modification and development of previous representations as problems arise by modification and development of previous problems. A system that is to explain human problem solving and scientific discovery does not need to incorporate a highly powerful mechanism for inventing completely novel representations. Even in revolutionary science the problems and representations are rooted in the past, and are not cut out of whole cloth.

Later in his "Ramsey Eliminability..." article Simon considers another objection pertaining to the problem space in revolutionary developments. The objection is that in revolutionary science the range of alternative hypotheses that constitute the problem space or representation cannot be delimited in advance. He states that this objection rests on a commonly drawn distinction between well defined problems, which are amenable to orderly analysis such as those in normal science, and ill defined problems, which are thought to be the exclusive domain of creativity, such as those in revolutionary science. Simon argues that the force of the objection depends on the distinctions being qualitative and not just matters of degree. He replies that there is no reason to deny that revolutionary hypotheses can be the result of some kind of recursively applicable rule of generation. He cites as an example of a revolutionary discovery Mendeleev's periodic table,

which does not involve a notion of pattern more complex than that required to handle patterned letter sequences. The problem space of possible patterns in which Mendeleev was searching was of modest size, and at least half a dozen of Mendeleev's contemporaries had noticed the pattern independently of him, although they had not exploited it as systematically or as vigorously as he did. Simon concludes that before one accepts the hypothesis that revolutionary science is not subject to laws of effective search, one should await more microscopic studies than have generally been made to date of the histories of revolutionary discoveries. He says that the case of Mendeleev may pose to be not at all exceptional.

Later in "Artificial Intelligence Research Strategies in the Light of AI Models of Scientific Discovery" in *Proceedings of the Sixth International Joint Conference on Artificial Intelligence* (1979) Simon can refer to operational discovery systems. He states that discovery systems are distinguished from most other problem-solving systems in the vagueness of the tasks presented to them and of the heuristic criteria that guide the search and account for selectivity, and that because their goals are very general, it is unusual to use means-end analysis commonly used for well structured tasks and to work backward from a desired result. The discovery system solves ill structured tasks and works forward inductively from the givens of the problem and from the new concepts and variables generated from the givens. He does not reference Kuhn in this context, but the implication is that discovery systems can routinely produce revolutionary science. Then still later in his *Scientific Discovery* he reconsiders his earlier correlation of well structured problems with normal science and ill structured problems with revolutionary science. He notes that normal science is described by Kuhn as involving no development of new laws but simply of applying known laws or developing subsidiary laws that fill in the dominant paradigm. He concludes that all discovery systems that develop new laws directly from data and not from a dominant paradigm must be productive of revolutionary science.

Simon's difficulties in relating his ideas to Kuhn's originate with Kuhn's ideas, not with Simon's. The most frequent criticism of Kuhn's *Structures of Scientific Revolutions* in the philosophy of science literature is that his distinction between normal and revolutionary science is so vague, that with the exception of a few paradigmatic cases his readers could not apply the distinction to specific episodes in the history of science, unless Kuhn had identified a particular episodes himself. The attractiveness of Kuhn's book at the time of its appearance was not its unimpressive conceptual clarity; it was its welcome redirection of the philosophy

profession's interest to the history of science at a time when many philosophers of science had come to regard the Logical Positivist philosophy with hardly any less cynicism than Ovid had shown toward the ancient Greek and Roman mythology in his *Metamorphoses*. Simon's discovery systems offer analytical clarity that Kuhn could not provide, with or without the Olympian irrelevance of the Russellian symbolic logic.

Nonetheless Simon's psychological approach shares difficulties with Kuhn's sociological approach. The reaction against Kuhn's sociological approach was often based in the recognition that conformity to and deviance from a consensus paradigm may explain the behavior of scientists without explaining the success of scientists. In due course Simon's cognitive psychology agenda for philosophy of science will be carried further than Simon or his colleagues had expected, and will result in artificial-intelligence systems that are more than just discovery systems. One noteworthy example of the psychological approach can be found in the artificial-intelligence systems developed by Thagard and his associates, which are considered below. But firstly consider the discovery systems developed by Simon and his colleagues at Carnegie-Mellon University.

The Theory of Discovery Systems

Simon's principal work on discovery systems is his *Scientific Discovery: Computational Explorations of the Creative Processes* (1987) co-authored with three colleagues. In the introductory section on the theory scientific discovery he says that the central hypothesis is that the mechanisms of scientific discovery are not peculiar to that activity, but can be subsumed as special cases of the general mechanisms of problem solving. Thus the approach taken is to exhibit a series of computer systems capable of making nontrivial scientific discoveries, which are actually rediscoveries of historic scientific laws and theories including empirical generalizations. And the method of operation of these computer systems is the method of heuristic search. The theory of scientific discovery is also in his view therefore a theory in cognitive psychology. Simon says that he seeks to investigate the psychology of discovery processes, and to provide an empirically tested theory of the information-processing mechanisms that are implicated in that process. He states that an empirical test of the systems as psychological theories of human discovery processes would involve presenting the computer programs and some human subjects with identical problems, and then comparing their behaviors. The computer system can

generate a "trace" of its operations, and the human subjects can report a verbal and written protocol of their behavior, while they are solving the problem. Then the system can be tested as a theory of behavior by comparing the trace with the protocol. Simon states that the computer systems described in his book incorporate heuristic search procedures to perform the kinds of selective processes that he believes scientists use to guide them in their search for regularities in data. But he also admits that his book provides little in the way of detailed comparison with human performance. And in discussions of particular applications involving particular discoveries, he says that in some cases the historical discoveries were actually performed differently than the systems performed the rediscoveries.

The principal interest in this book is actually system design rather than psychological testing and reporting, and Simon states that he wishes to provide some foundations for a normative theory of discovery, that is to say, to write a how-to-make-discoveries book. He explains that by this he does not mean a set of rules for deriving theories conclusively from observations. Instead, he wishes to propose a set of criteria for judging the efficacy and efficiency of the processes used to discover scientific theory. Accordingly Simon sets forth a rationality postulate for the scientist: to use the best means he has available - the best heuristics - for narrowing the search down to manageable proportions, even though this effort may result in excluding good solution candidates. If the novelty of the scientific problem requires much search, this large amount of search is rational if it employs all the heuristics that are known to be applicable to the domain of the problem. Thus, his rationality postulate for the scientist is a bounded rationality postulate, not only due to the limits imposed by memory capacity and computational speed, but also due to the limit imposed by the inventory of available heuristics.

BACON and Other Discovery Systems

In his *Novum Organon* (Book I, Ch. LXI) Francis Bacon had expressed the view that with a few easily learned rules or a method it may be possible for anyone undertaking scientific research to be successful. And he proposed a method of discovery in the sciences which will leave little to the sharpness and strength of men's wits, but will bring all wits and intellects nearly to a level. For as in drawing a straight line or in inscribing an accurate circle by the unassisted hand, much depends on its steadiness and

practice, but if a rule or pair of compasses be applied, little or nothing depends upon them, so exactly is it with his method. Today Bacon's agenda is called proceduralization for mechanization, and it is appropriate therefore that a discovery system should be named **BACON**.

The **BACON** discovery system is actually a set of successive and increasingly sophisticated discovery systems that make quantitative empirical laws and theories. Given sets of observation measurements for two or more variables, **BACON** searches for functional relations among the variables. The search heuristics in earlier versions of each **BACON** computer program are carried forward into all later ones, and later versions contain heuristics that are more sophisticated than those in earlier versions. In the literature describing the **BACON** systems each successive version is identified by a numerical suffix, such as **BACON.1**. The original version, **BACON.1**, was designed and implemented by Pat Langley in 1979 as the thesis for his Ph.D. dissertation written in the Carnegie-Mellon department of psychology under the direction of Simon, and titled *Descriptive Discovery Processes: Experiments in Baconian Science*. He published descriptions of the system in "**BACON.1**: A General Discovery System" in *The Proceedings of the Second National Conference of the Canadian Society for Computational Studies in Intelligence* (1978) and as a co-author with Simon and others in *Scientific Discovery* (1987). **BACON** programs are implemented in a list processing computer language called **LISP**, and its discovery heuristics are implemented in a production-system language called **PRISM.** The system lists the observable measurement data monotonically according to the values of one of the variables, and then determines whether the values of some other variables follow the same (or the inverse) ordering. Picking one of these other variables, it searches for an invariant by considering the ratio (or the product) of these variables with the original one. If the ratio or product is not constant, it is introduced as a new variable, and the process continues the search for invariants. Examples of some of the simpler search heuristics expressed in the conditional form of a production are as follows: (1) If the values of a variable are constant, then infer that the variable always has that value. (2) If the values of two numerical variables increase together, then examine their ratio. (3) If the values of one variable increase as those of another decrease, then examine their product. The general strategy used with these heuristics is to create variables that are ratios or products, and then to treat them as data from which still other terms are created, until a constant is identified by the first heuristic.

BACON.1 has rediscovered several historically significant empirical laws including Boyle's law of gases, Kepler's third planetary law, Galileo's

law of motion of objects on inclined planes, and Ohm's law of electrical current. A similar system, **BACON.3** has rediscovered the ideal gas law and Coulomb's law of electrical current. For making these rediscoveries, Simon and his associates used measurement data actually used by the original discoverers, and published by W.F. Magie in *A Source Book in Physics* (1935). **BACON.4** is a significant improvement over earlier versions. It was developed and firstly described by Gary Bradshaw, Pat Langley, and Herbert Simon in "The Discovery of Intrinsic Properties" in *The Proceedings of the Third National Conference of the Canadian Society for Computational Studies in Intelligence* (1980), and it is also described in their 1987 book. The improvement is the ability to use nominal or symbolic variables that take only names or labels as values. For example the nominal variable "material" may take on values such as "lead", "silver", or "water." Values for numerical properties may be associated with the values of the nominal variables, such as the density of lead, which is 13.34 grams per cubic centimeter. **BACON.4** has heuristics for discovering laws involving nominal variables by postulating associated values called "intrinsic properties", by inferring a set of numerical values for the intrinsic properties for each of the postulated nominal values, and then by retrieving the numerical values when applying its numerical heuristics to discover new laws involving these nominal variables. The laws rediscovered by **BACON.4** include: (1) Ohm's law of electrical circuits, where the intrinsic properties associated with the nominal variables are voltage and resistance, (2) Archimedes law of displacement, where the intrinsic properties are density and the volume of an irregular object, (3) Black's law of specific heat, where specific heat is the intrinsic property, (4) Newton's law of gravitation, where gravitational mass is the intrinsic property, and (5) the law of conservation of momentum, where the inertial mass of objects is the intrinsic property. **BACON.4** was further enhanced so that it could rediscover the laws describing chemical reactions formulated by Dalton, Gay-Lussac, and Comizzaro. For example it rediscovered Gay-Lussac's principle that the relative densities of elements in their gaseous form are proportionate to their corresponding molecular weights. Rediscovering these laws in quantitative chemistry involved more than postulating intrinsic properties and noting recurring values. These chemists found that a set of values could be expressed as small integer multiples of one another. This procedure required a new heuristic that finds common divisors. A common divisor is a number which, when divided into a set of values, generates a set of integers. **BACON.4** uses this method of finding common divisors,

whenever a new set of dependent values is about to be assigned to an intrinsic property.

BACON.5 is the next noteworthy improvement. It uses analogical reasoning for scientific discovery. **BACON.1** through **BACON.4** are driven by data in search for regularities in the data. Furthermore the heuristics in these previous **BACON** systems are almost entirely free from theoretical presuppositions about domains from which the data are drawn. **BACON.5** incorporates a heuristic for reducing the amount of search for laws in certain special cases, in which the system is given very general theoretical postulates, and then it reasons by analogy by postulating symmetries between the unknown law and a theoretical postulate given to the system as an input. The general theoretical postulate that Simon gave to **BACON.5** is the law of conservation. The laws rediscovered by **BACON.5** using analogy with the conservation law include the law of conservation of momentum, Black's law of specific heat, and Joule's law of energy conservation.

The **BACON** discovery system was not the first system developed around Simon's principles of human problem solving with heuristics. In 1976 Douglas B. Lenat published his Ph.D. dissertation written at Stanford University and titled *AM: An Artificial Intelligence Approach to Discovery Mathematics as Heuristic Search.* Allen Newell was one of his dissertation advisors, and Lenat acknowledges that he got his ideas from Herbert Simon. Lenat has since accepted a faculty position in the computer science department of Carnegie-Mellon University. In 1977 he published "The Ubiquity of Discovery" in *The Proceedings of the Fifth International Joint Conference on Artificial Intelligence*, (IJCAI) in which he relates Simon's theory of heuristic problem solving in science and describes the specific heuristics in his **AM** discovery system. While Lenat's article includes discussion of artificial intelligence in empirical science, his **AM** system is not for empirical science, but is a computer system which develops new mathematical concepts and conjectures with these concepts. Also in the 1977 *IJCAI Proceedings* he published "Automated Theory Formation in Mathematics", which offers a more detailed description of the system's two-hundred fifty heuristics, and which also discusses his application of the **AM** system in elementary mathematics. He reports that in one hour of processing time **AM** rediscovered hundreds of common mathematical concepts including singleton sets, natural numbers, arithmetic, and also theorems such as unique factorization. In 1979 Simon published "Artificial Intelligence Research Strategies in the Light of AI Models of Scientific Discovery" in *The Proceedings of the Sixth International Joint Conference on Artificial Intelligence* in which he considers Lenat's **AM** system and

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Langley's **BACON** systems as useful for illuminating the history of the discovery process in the domain of artificial intelligence itself, and for providing some insight into the ways to proceed in future research and development aimed at new discoveries in that field. He says that AI will proceed as an empirical inquiry rather than as a theoretically deductive one, and that principles for the discipline will be inferred from the computer programs constituting the discovery systems, although he also notes that in the scientific profession the community members' work in parallel, while in the machines the work proceeds serially.

BACON created quantitative empirical laws by examination of measurement data. Simon and his associates also designed and implemented discovery systems, that are capable of creating qualitative laws from empirical data, and three such systems are described in *Scientific Discovery*. They are named **GLAUBER**, **STAHL** and **DALTON**. The **GLAUBER** discovery system is named after the eighteenth century chemist, Johann Rudolph Glauber, who contributed to the development of the acid-base theory. Langley developed the discovery system in 1983. For its historical reconstruction of the acid-base theory **GLAUBER** was given facts very similar to those known to eighteenth century chemists, before they formulated the theory of acids and bases. These facts consist of information about the tastes of various substances and the reactions in which they take part. The tastes are "sour", "bitter", and "salty." The substances are acids, alkalis and salts labeled with common names, which for purposes of convenience are the contemporary chemical names of these substances, even though **GLAUBER** makes no use of the analytical information in the modern chemical symbols. Associated with these common names for chemical substances are argument names, such as "input" and "output" that describe the roles of the chemical substances in the chemical reactions in which the substances partake. Finally the system is given names for the three abstract classes: "acid", "alkali", and "salt." When the system is executed with these inputs, it examines the chemical substances and their reactions, and then correlates the tastes to the abstract classes, and also expresses the reactions in a general law that states that acids and alkalis react to produce salts.

The second discovery system is **STAHL**, which creates a type of qualitative law that Simon calls "componential", because it describes the hidden structural components of substances. System **STAHL** is named after the German chemist, Georg Ernst Stahl, who developed the phlogiston theory of combustion. **STAHL** recreates the development of both the phlogiston and the oxygen theories of combustion. Simon states that

discovery systems should be able to arrive at laws that have been rejected later in favor of others in the history of science. And he says that since a discovery system's historical reconstruction aims at grasping the main currents of reasoning in a given epoch, then reproducing the errors that were typical of that epoch is diagnostic. Like **GLAUBER**, **STAHL** accepts qualitative facts as inputs, and generates qualitative statements as outputs. The input is a list of chemical reactions, and its initial state consists of a set of chemical substances and their reactions represented by common names and argument names, as they are in **GLAUBER**. When executed, the system generates a list of chemical elements and of the compounds in which the elements are components. The intermediate states of **STAHL**'s computation consist of transformed versions of initial reactions and inferences about the components of some of the substances. When the system begins running, it is driven by data, but after it has made conjectures about the hidden structures, it is also driven by these conjectures, which is to say, by theory. Simon concludes from the rediscovery of the phlogiston and oxygen theories by **STAHL,** that the proponents of the two theories reasoned in essentially the same ways, and that they differed mainly in their assumptions. He also applied **STAHL** to the rediscovery of Black's analysis of *magnesia alba*, and he maintains that the same principles of inference were used by chemists quite widely in their search for componential explanations of chemical substances and their reactions. The principal significance of this diversity to Simon is the demonstration that the reasoning procedures in **STAHL** are not *ad hoc*, and that **STAHL** is a general system.

The third discovery system that creates qualitative laws is **DALTON,** which is named after John Dalton. Like Dalton the chemist, the **DALTON** system does not invent the atomic theory of matter; it employs a representation that embodies the hypothesis, and that incorporates the distinction between atoms and molecules invented by Avogado. **DALTON** is a theory-driven system for reaching the conclusions about atomic weights that **BACON.4** derived in a data-driven manner. And **DALTON** creates structural laws in contrast to **STAHL,** which creates componential laws. **DALTON** is given information that is similar to what was available to chemists in 1800. The input includes a set of reactions and knowledge of the components of the chemical substances involved in each reaction. This is the type of information outputted by **STAHL**, and **DALTON** uses the same common-name/argument-name scheme of representation used by **STAHL**. **DALTON** is also told which of the substances are elements having no components other than themselves. And it knows that the number of

molecules in each chemical substance is important in the simplest form of a reaction, and that the number of atoms of each element in a given molecule is also important. **DALTON**'s goal is to use this input to develop a structural model for each reaction and for each of the substances involved in each reaction, subject to two constraints. The first constraint is that the model of a molecule of a substance must be the same for all reactions in which it is present. The second constraint is that the models of the reactions display the conservation of particles. Simon applied **DALTON** to the reaction involving the combination of hydrogen and oxygen to form water, and the system outputted a model giving a modern account of the water reaction. He also considers applying **DALTON** to elementary particle physics and to classical genetics, but he states that the current version is not adequate to this task.

Since the publication of *Scientific Discovery* Simon and his associates have continued their work on discovery systems and have pursued their work into new directions. While **BACON** and the other systems described in the 1987 book are concerned mainly with the ways in which theories can be generated from empirical data, the question of where the data come from has largely been left unanswered. In "The Process of Scientific Discovery: The Strategy of Experimentation" (1988) in *Models of Thought* Simon and Deepak Kulkarni describe their new **KEKADA** discovery system, which examines not only the process of hypothesis formation, but also the process of designing experiments and programs of observation. The **KEKADA** discovery system is constructed to simulate the sequence of experiments carried out by Hans Krebs and his colleague, Kurt Henseleit, between July 1931 and April 1932, which produced the elucidation of the chemical pathways for synthesis of urea in the liver. This discovery of the ornithine cycle was the first demonstration of the existence of a cycle in the metabolic biochemistry. Simon and Kulkarni's source for this episode is "Hans Krebs and the Discovery of the Ornithine Cycle" in *Federation Proceedings* (1980) by Frederic L. Holmes of Yale University. Holmes also made himself available to Simon and Kulkarni for consultation in 1986 when their study was in progress. The organization of **KEKADA** is based on a two-space model of learning proposed earlier by Simon and Lea in "Problem Solving and Rule Induction: A Unified View" in *Knowledge and Cognition* (1974). The system searches in an "instance space" and a "rule space", each having its own set of heuristics. The instance space is defined by the possible experiments and experimental outcomes, and it is searched by performing experiments. The rule space is defined by the hypotheses and other higher level descriptions coupled with associated measures of confidence. The

system proceeds through cycles in which it chooses an experiment from the instance space to carry out on the basis of the current state of the rule space, and the outcome of the experiment modifies the hypotheses and confidences in the rule space.

One of the distinctive characteristics of **KEKADA** is its ability to react to surprising experimental outcomes, and to attempt in response to explain the puzzling phenomenon. Prior to carrying out any experiment, expectations are formed by expectations setters, which are a type of heuristic for searching the rule space, and the expectations are associated with the experiment. The expectations consist of expected output substances of a reaction, and expected upper and lower bounds on the quantities or the rates of the outputs. If the result of the experiment violates these bounds, it is noted as a surprise. Comparison of the course of work of Krebs as described by Holmes and of the work of **KEKADA** in its simulation of the discovery reveals only minor differences, which Simon and Kulkarni say can be explained by focus of attention shifts and small differences in the initial knowledge with which Krebs and **KEKADA** started. The authors also say that a manual simulation of the path that Krebs followed in a second discovery, that of the glutamine synthesis, is wholly consistent with the theory set forth by **KEKADA**. They therefore conclude that the structure and heuristics in **KEKADA** constitute a model of discovery that is of wider applicability than the episode used to develop the system, and that the system is not *ad hoc*.

Simon's Philosophy of Science

Simon's literary corpus is rich enough to contain a philosophy of science that addresses all four of the basic questions addressed by academic professional philosophers.

Aim of Science

What philosophers of science call the aim of science may be taken as a rationality postulate for basic scientific research. Simon explicitly applies his thesis of bounded rationality developed for economics to scientific research in his autobiography in an "Afterword" titled "The Scientist as Problem Solver", although this explicit statement would not have been necessary for the attentive reader of his literary corpus. Simon describes his theory of discovery as a special case of his theory of human problem solving, because both theories are based on his theory of heuristic search.

And he views his theory of heuristic search in turn as a special case of his postulate of bounded rationality. To this metatheory one need only add that Simon's application of his postulate of bounded rationality to scientific discovery amounts to his thesis of the aim of science. The function of heuristics is to search efficiently a problem space of possible solutions, which is too large to be searched exhaustively. The limited computational ability of the scientist relative to the size of the problem space is the "computational constraint", that is the factor that bounds the scientist's rationality, constraining the scientist from aiming for anything like global rationality. The research scientist is therefore a satisficer, and the aim of the scientist is satisficing within both empirical and computational constraints.

Explanation

Simon's views on the remaining philosophical topics, explanation and criticism, may also be considered in relation to the discovery systems. Consider firstly his statements on scientific explanation including the topic of theoretical terms. The developers of the **BACON** systems make a pragmatic distinction between observation variables and theoretical variables in their systems. Simon notes that contemporary philosophers of science maintain that observation is theory-laden, and his distinction between observational and theoretical terms does not deny this semantical thesis. He calls his distinction "pragmatic", because he makes it entirely relative to the discovery system, and it is also pragmatic in the sense understood in the contemporary Pragmatist philosophy of science. Those variables that have their associated numeric values before input to the system are considered to be observational variables, while those that receive their values by the operation of the discovery system are considered to be theoretical ones. He states that in any given inquiry we can treat as observable any term whose values are obtained from an instrument that is not itself problematic in the context of that inquiry. Thus Langley considers all the values created by the **BACON** programs by multiplication or division for finding products or ratios to be theoretical terms. And Simon accordingly calls the values for nominal variables that are postulated intrinsic properties theoretical terms.

Unfortunately Simon does not follow through with this Pragmatist relativizing to problem-solving discovery systems, but reverts to the Positivist concept of explanation. In his exposition of **DALTON**, which create structural theories, Simon comments that as an area in science matures its researchers progress from "descriptions" to "explanations", and he cites Hempel's *Aspects of Scientific Explanation and Other Essays* (1965). Examples of explanations cited by Simon are the kinetic theory of

heat, which provides an explanation of both Black's law and the ideal gas law, and Dalton's atomic theory, which provides explanations for the law of multiple proportions and Gay-Lussac's law of combining volumes. He notes that each of these examples involves a structural model in which macroscopic phenomena are described in terms of their inferred components. Simon contrasts explanation to the purely phenomenological and descriptive analyses carried out by **BACON.4**, when it rediscovered the concepts of molecular and atomic weight, and assigned correct weights to many substances in its wholly data-driven manner. He affirms that **BACON.4**'s analyses involved no appeal to a particulate model of chemical elements and compounds, and that what took the place of the atomic model were the heuristics that searched for small integer ratios among corresponding properties of substances. This concept of explanation is a reversion to the hypothetical-deductive concept of explanation in which theories are said to "explain" empirical laws by deductive connection, and in which theory and empirical or descriptive generalizations are distinguished by their semantics. This is what Hanson referred to as the almanac view of science. On the Pragmatist view theory and empirical description are not distinguished semantically, but are distinguished pragmatically by their use in the problem-solving activity that is scientific research. Theory is what is proposed for empirical testing, and description is what is presumed for testing. Explanation is language that is theory after it has been empirically tested and not falsified; or one who speaks of "theoretical explanation" is merely speaking of a proposed explanation. This is the functional view of the language of science instead of the Positivist almanac view. Thus given that the discovery systems are problem-solving systems, defining "theory" and "explanation" relative to the discovery system is to define them in a manner consistent with the contemporary Pragmatist philosophy. And on this Pragmatic view the outputted laws generated by **BACON.4** are no less theoretical or explanatory than the outputs of **DALTON**.

Discovery

In addition to the physical theories that the discovery systems recreated, consideration might also be given to the behavioral and social theories that Simon and his colleagues had not attempted to address with their discovery systems. Why did this Nobel laureate economist never attempt to construct an economic theory with a discovery system? Perhaps one might ask instead: is Simon actually a Romantic in his philosophy of social science? One possible answer is that the economic theory of greatest

interest to him, his thesis of bounded rationality, does not lend itself to any discovery system like those he or his colleagues have yet designed. This is an answer in terms of technology rather than philosophy. When Simon found that behaviorism posed a philosophical impediment to his agenda for cognitive psychology, he rejected this variation on the Positivist philosophy, even though he had previously been sympathetic to it. Similarly one might expect that he should not have been deterred by any version of Romanticism; he demonstrated sufficient philosophical sophistication to distinguish empirical from ontological criteria for criticism.

Criticism

Simon's view of scientific criticism is based on his theory of heuristics and discovery systems. Philosophers of science such as Hanson, whose interests were focused on the topic of scientific discovery, found that the Positivist separation of the "context of discovery" and the "context of justification" fails to appreciate the interdependent interaction between these two functions in scientific research. He also notes this interaction between discovery and justification in *Scientific Discovery*, because it is integral to his theory of heuristics and to his discovery system designs. His principal thesis of problem solving is that the availability of evaluative tests during the successive stages of the discovery process carried out with heuristics is a major source of the efficiency of the discovery methods. Each step or group of steps of a search is evaluated in terms of the evidence it has produced, and the continuing search process is modified on the basis of the outcome of these evaluations. The confirmation of partial results accumulates and makes the confirmation of the final hypothesis coincide with its generation. Yet Simon does not fail to see the need for predictive testing by observation or experiment of the hypotheses generated by the discovery systems which only find patterns in limited available data.

Muth's Rational Expectations "Hypothesis"

Simon distinguishes three rationality postulates: the neoclassical postulate of global rationality prevailing in academic economics, his own thesis of bounded rationality, and the rational expectations hypothesis. The reader of Simon's autobiography, however, would never guess that about two decades after its first appearance, the rationality expectations hypothesis had occasioned the development of a distinctive type of discovery system, the Bayesian Vector Autoregression or **BVAR** discovery system. In fact it

is doubtful that even its creator, Robert Litterman, or his colleagues recognize the system as a discovery system, even though it does what discovery systems are intended to do: it makes theories. This irony is due to the fact that the prevailing philosophy of science in economics is Romanticism, which has led economists to view **BVAR** models as "atheoretical." But if the term "theory" is understood in the contemporary Pragmatist sense, the equations created by the **BVAR** system are economic theories. Before taking up the **BVAR** system, firstly consider the rational expectations hypothesis.

One of the distinctive aspects of Simon's autobiography is a chapter titled "On Being Argumentative." In this chapter's opening sentence Simon states that he has not avoided controversy, and he adds that he has often been embroiled in it. And on the same page he also says that he has usually announced his revolutionary intentions. But revolutionaries occasionally find others revolting against them. In the preceding chapter of his autobiography he describes a tactical retreat in the arena of faculty politics: his eventual decision to migrate from Carnegie-Mellon's Graduate School of Industrial Administration to its psychology department, which as it happens, is not an unsuitable place for his cognitive psychology. This conflict with its disappointing denouement for Simon was occasioned by the emergence of the rational expectations hypothesis, a thesis that was first formulated by a colleague, John F. Muth, and which was part of what Simon calls the ascendancy of a coalition of neoclassical economists in the Graduate School of Industrial Administration. Muth's rational expectations hypothesis, which Simon says deserves a Nobel prize even though he maintains that it is unrealistic, was set forth in a paper read to the Econometric Society in 1959, and then published in *Econometrica* (1961) under the title "Rational Expectations and the Theory of Price Movements." Muth explains that he calls his hypothesis about expectations "rational", because it is a descriptive theory of expectations, and is not a pronouncement of what business firms ought to do. The idea of rational expectations is not a pet without pedigree. It is a continuation of an approach in economics known as the Stockholm School, in which expectations play a central role, and which Muth references in his article. A brief consideration of the Stockholm School is in order, to see how the rational expectations advocates depart from it, especially in their empirical modeling.

One of the best known contributors to the Stockholm School is Bertil Ohlin, a Nobel laureate economist, who is best known for his *Interregional and International Trade* (1933), and whose elaboration on the monetary theory of Knut Wicksell anticipated the Keynesian theory in important

respects.   He called his own theory of underemployment the "Swedish theory of unused resources."   In 1949 he published his *Problem of Employment Stabilization*, which contains his own macroeconomic theory and concludes with a critique of Keynes' *General Theory* from the Stockholm School viewpoint. In his critique Ohlin draws upon a distinction between *ex ante* or forward-looking analysis and *ex post* or backward-looking analysis, firstly proposed by 1974 Nobel laureate economist Gunnar Myrdal (1898-1987), his colleague of Stockholm School persuasion and fellow critic of Keynes. Later in life Myrdal evolved his theory of *ex ante* analysis into an Institutionalist economic theory, and in his *Against the Stream* (1973) he uses it to explain a phenomenon that is problematic for Keynesian economics: "stagflation", the co-existence of economic stagnation and inflation.   However, Myrdal does not address the effect of institutional change on the structural parameters in econometric models, and apparently does not think well of econometrics.   In the first chapter, "Development of Economics: Crises, Cycles", he says that when he was still in his "theoretical stage" of thinking, i.e. pre-Institutionalist stage, he had something to do with the initiation of the Econometric Society, which he says was planned at the time as a defense organization against the advancing American Institutionalists, an advance which was halted in the economics profession by the Keynesian revolution. He says that Keynesian theory is now in crisis as a result of problems such as stagflation and structural unemployment, and that the future development of economics will be interdisciplinary and Institutionalist.

Ohlin, who is not an Institutionalist and remains a neoclassical economist, maintains that *ex post* analysis alone cannot provide an explanation in economics, because any explanation must run in terms of factors that govern actions, and actions refer to the future. Any economic explanation must therefore contain *ex ante* analysis, which consists of the expectations or plans of the actors in their economic roles. Ohlin notes that Keynes theory may be said to contain an *ex ante* analysis of investment, because it includes the marginal efficiency of capital, which is similar to Wicksell's "natural" rate of interest: the expected return from newly constructed capital. But Ohlin took exception to Keynes' exclusively *ex post* analysis of saving, in which saving is merely the residual of aggregate income net of aggregate consumption. On the Stockholm School view there must be an *ex ante* analysis of saving, and Ohlin theorizes that *ex ante* saving is determined by the difference between current consumption and the level of income in the prior period. He calls the *ex ante* saving rate the average propensity to save.   Ohlin's Stockholm School approach is

significant not only because Ohlin offers an explanation of how expectations are formed, but also because it accounts for expectations by explicit variables, the *ex ante* variables, so that their effects need not be incorporated implicitly in the statistically estimated parameters of the econometric models.

Ohlin's explanation notwithstanding, Muth blithely criticizes the Stockholm School for failing to offer an explanation of the way expectations are formed, and he advances his rational expectations hypothesis as the explanation. Muth notes two conclusions from studies of expectations measurements, which he says his rational expectations hypothesis "explains." The principal conclusion is that the averages of expectations made by economic actors in an industry are more accurate than the forecasts made with naive models, and are as accurate as elaborate equation systems, although there are considerable cross-sectional differences of opinion. The rational expectations hypothesis explains this accuracy by the thesis that expectations viewed as informed predictions of future events are essentially the same as the predictions of the relevant economic theory. Muth says that he is not asserting that the scratch work of entrepreneurs resembles a system of equations in any way, although he says that the way expectations are formed depends on the structure of the entire relevant system describing the economy. His more precise statement of his hypothesis is as follows: that expectations of firms (or, more generally, the subjective probability distribution of outcomes) tend to be distributed, for the same information set, about the prediction of the theory (or, the "objective" probability distributions of outcomes). Muth argues that if expectations were not moderately rational, then there would be opportunities for economists to make profits in commodity speculation, running a business firm, or selling information to present owners. In his discussion of price expectations, he offers an equation for determining expected price in a market, and references a paper to be published by him. The equation says that expected price is a geometrically weighted moving average of past prices. He also argues that rationality is an assumption that can be modified to adjust for systematic biases, incomplete or incorrect information, poor memory, etc., and that these deviations can be explained with analytical techniques based on rationality. The second conclusion is that reported expectations generally underestimate the extent of changes that actually take place. Like the Stockholm School, Muth's hypothesis does not assert that there are no expectations errors. He states that in the aggregate a reported expected magnitude such as a market price is an unbiased predictor of the corresponding actual magnitude except where a series of exogenous

disturbances are not independent. Muth's explanation of the reported expectations errors of underestimation is his argument that his hypothesis is not inconsistent with the fact that the expectations and actual data have different variances. Muth references Simon's "Theories of Decision-Making in Economics" in *American Economic Review* (1959), and describes Simon as saying that the assumption of rationality in economics leads to theories that are inconsistent with or inadequate to explain observed phenomena, especially as the phenomena change over time. Muth comments that his view is exactly the opposite of Simon's: dynamic economic models do not assume enough rationality.

Simon's critique of the rational expectations hypothesis is set forth in the second chapter titled "Economic Rationality" in his *Sciences of the Artificial* (1969). In the section titled "Expectations" he notes that expectations formed to deal with uncertainty may not result in a stable equilibrium or even a tendency to stable equilibrium, when the feed forward in the control system has destabilizing consequences, as when each actor is trying to anticipate the actions of others and their expectations. The stock example in economics is the speculative bubble. In the next section titled "Rational Expectations" Simon references Muth's 1961 article. He characterizes Muth's hypothesis as a proposed solution to the problem of mutual outguessing by assuming that actors form their expectations "rationally", by which is meant that the actors know the laws that govern the economic system, and that their predictions of the future position of the system are unbiased estimates of the actual equilibrium. Simon says that the rational expectations hypothesis precludes destabilizing speculative behavior. More fundamentally Simon maintains that there is no empirical evidence supporting the rational expectations hypothesis. And he doubts that business firms have either the knowledge or the computational ability that would be required to carry out the expectations strategy. He concludes that since economists have little empirical knowledge about how people do in fact form expectations about the future, it is difficult to choose at present among the models that are currently proposed by competing economic theories to account for cyclical behavior of the economy.

Ostensibly Muth proposed his rational expectations hypothesis as an explanation of two conclusions about expectations measurements. These empirical measurements should be used to provide the independent semantics and magnitudes needed for empirical testing of the rational expectations hypothesis. What might rationally have been expected of the rational expectations advocates is an attempt to construct conventional structural-equation econometric models using *ex ante* expectations data, to

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demonstrate and test their explanatory hypothesis. But neither Muth nor the rational expectations advocates took this approach. On the basis of his rational expectations hypothesis Muth shifted from an explanation of empirical measurements of reported expectations to consideration of a forecasting technique that he proposes be used by neoclassical economists. This semantical shift has had three noteworthy effects on subsequent empirical work in the rational expectations school: Firstly there was a disregard of empirical measurements of expectations, measurements that would serve as values for *ex ante* variables; then secondly there was an attack upon the conventional structural type of econometric model and the development of a new type of empirical model as an implementation of the rational expectations hypothesis but with no independently collected expectations measurements; and thirdly there evolved the design and implementation of a computerized procedure for constructing this new type of model, a computerized procedure which is a distinctive type of discovery system. This semantical shift has been consequential for econometric modeling. Haavelmo's structural-equation type of econometric model has been definitive of empirical economics for more than half a century, and it is still the prevailing practice in the economics profession. To the dismay of conventional econometricians the rational expectations advocates' attack upon the conventional structural-equation econometric model is, therefore, hardly less subversive to the *status quo* in the science, than Simon's attack on the neoclassical rationality postulate. And this outcome certainly has an ironic aspect, because the structural-equation econometric model had been advanced as the empirical implementation (at least ostensibly) of the neoclassical economic theory, while the rational expectations hypothesis has been advanced as offering greater fidelity to neoclassical theory by extending rationality to expectations. To understand such a strange turn of events, it is helpful to consider the still prevailing, conventional concept of econometric model, the structural-equation model.

Haavelmo's Structural-Equations Agenda And Its Early Critics

The authoritative statement of conventional econometric modeling is set forth in "The Probability Approach in Econometrics", initially a Ph.D. dissertation written in 1941 by Nobel laureate econometrician, Trygve Haavelmo (1911-   ), and then published as a supplement to *Econometrica* (July, 1944). *Econometrica* is the journal of the Econometric Society, which was founded in 1930, and which described itself as "an international society

for the advancement of economic theory in its relation to statistics and mathematics" and for "the unification of the theoretical-quantitative and the empirical-quantitative approach" in economics. The supplement essentially advanced certain fundamental ideas for the application of the Neyman-Pearson theory of statistical testing of mathematical hypotheses expressing neoclassical economic theory. At the time the supplement was published the society's offices were at the University of Chicago, where econometricians found themselves isolated and unwelcome. Then most economists believed that probability theory is not applicable to economic time series data, partly because the data for successive observations are not statistically independent, and partly because few economists were competent in the requisite statistical techniques. In her *History of Econometric Ideas* (1990) Mary S. Morgan writes that this introduction of probability theory into economics was a "probabilistic revolution" in econometrics, which shifted the role of econometrics from the measuring of the parameters in a theory to the testing of the theory.

Firstly Haavelmo argued that the time series data are not a set of successive observations, but are one observation with as many dimensions as there are independent variables in the model. This strategy is not mentioned in textbooks today. Haavelmo's more lasting agenda consisted of construing the econometric model as a probabilistic statement of the econometric theory, so that the theory is neither held harmless by falsifying data nor immediately and invariably falsified as soon as it is confronted with the data. He says that the model is an *a priori* hypothesis about real phenomena that states that every system of values that the economist might observe of the "true" variables, will be one that belongs to the system of numeric values which is admissible within the model. This attempt to construe the model as a third linguistic entity between theory and data leads him to develop an unusual and complicated semantical analysis. The first chapter titled "Abstract Models and Reality" sets forth his theory of the semantics of measurement variables in econometrics. Haavelmo distinguishes three types of "variables", which actually represent three separate meanings associated with each variable symbol that may occur in an empirical economic theory. The first type is the "theoretical variable", which is the meaning a variable symbol has simply by virtue of its occurring in the equations of the model, and its values are subject only to the consistency of the model as a system of one or several equations. The second type is the "true variable", which signifies an ideal experimental design that the economist could at least imagine arranging in order to measure those quantities in real economic life, that he thinks might obey the laws imposed by the model on the

corresponding theoretical variable. Haavelmo says that when theoretical variables have ordinary words or names associated with them, these words may merely be vague descriptions that the economist has learned to associate with certain phenomena, or they may signify ideal experimental designs with their descriptions of measurement procedures. But he also says that there are many indications that the economist nearly always has some such ideal experimental design "in the back of his mind", when the economist builds his theoretical models, and that in the verbal description of his model in economic terms the economist suggests explicitly or implicitly some type of measurement design to obtain the measurements for which he thinks his model would hold. Thus the theoretical and true variables are distinguished, but are not separated in the fully interpreted theory proposed for estimation and testing. And associated with the true variables there are true or ideal measurements, which are not only error free, but which are collected in accordance with an ideal experimental design. The third type of variable is the "observational variable", which describes the measurements actually used by the economist for his model construction. Haavelmo says that the economist often must be satisfied with rough and biased measures, and must dig out the measurements he needs from data that were collected for some other purpose. The true variables are those such that if their behavior should contradict a theory, the theory would be rejected as false. On the other hand if the behavior of the observational variables contradicts the theory, they leave the possibility that the economist is trying out the theory on facts for which the theory was not meant to hold. This may cause confusion, when the same names are often used for both types of variables. To test a theory against facts or to use it for prediction, either the statistical observations available must be corrected or the theory itself must be adjusted, so as to make the facts the economist considers the true variables relevant to the theory. In Haavelmo's approach to econometrics, probability distributions not only adjust for measurement errors, but also adjust for the deviations between the true and observational values due to their semantical differences.

An experienced economist, Haavelmo is adequately cognizant of the difficulties in the work that makes economics an empirical science. In contrast, most of his contemporaries in the 1940's were ivory-tower theoreticians. Today there is much more adequate data available to economists from government agencies. Nonetheless, economists still sometimes find they must use what they call "proxy" variables, which are recognized as measurements of phenomena other than what the economist is interested in explaining with his models. And sometimes the government

statistical agency will use names to identify data that describe phenomena for which the data are a proxy rather than what the data measure. For example in their *Industrial Production* (1986) the Board of Governors of the Federal Reserve System say that when their monthly production index series cannot be based on physical measures of output, such as tons of steel or assemblies of automobiles and trucks, then monthly input measures, such as hours worked or kilowatt hours of electricity consumed adjusted for the observed long-term trend and cyclical relations between input and output, are used to derive the monthly output series. Nonetheless, the Federal Reserve Board calls these proxy series "production." Except in these explicit cases involving proxy variables, however, it is questionable whether the economist has "in the back of his mind", as Haavelmo says, any specific ideal experimental design setting forth ideal measurement procedures. Most often the descriptive words associated with theoretical variable symbols in a mathematical model are vague and are just not given semantical resolution until actual measurements are associated with the model. Then the description of the actual measurement procedures supplies additional information to resolve this vagueness. Only when economists decide that the actual measurements are proxies for what they wish to investigate, such that there is more deviation involved than just errors of measurement, do they find themselves confronting an equivocation like Haavelmo's "true" and "observational" semantics instead of supplying a resolution to the vagueness in the meanings of the terms in the theory.

The second chapter titled "The Degree of Permanence of Economic Laws" contains Haavelmo's theory of scientific law in economics, and specifically his treatment of the degree of constancy or permanence in the relations among economic variables in econometric models. Nonconstancy is manifested by structural breakdown of the traditional structural-equation model, the type that Haavelmo advocates in his monograph. The rational expectations hypothesis is proposed as an explanation for structural breakdown, and the hypothesis is the basis for a new type of model that is an alternative to the structural-equation model. The **BVAR** discovery system constructs a refined version of this new type of model. Haavelmo says that the constancy in a relationship is a property of real phenomena, as the economist looks upon the phenomena from the viewpoint of a particular theory. At the very opening of his monograph he states that theoretical models are necessary to understand and explain events in real life, and that even a simple description and classification of real phenomena would probably not be possible or feasible without viewing reality through the framework of some scheme conceived *a priori*. This statement seems

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similar to Popper's thesis that there is no observation without theory, and to Hanson's characterization of observation as theory-laden. But the term "theory" in Haavelmo's monograph means specifically the neoclassical economic theory with its rationality postulate, and the basic task of his monograph is to describe his probability approach in econometrics understood as the application of Neyman-Pearson statistical inference theory to neoclassical economic theory for empirical testing.

In the first chapter of the monograph Haavelmo distinguished three types of quantitative economic relations. The first type is the definitional or accounting identity. A common example is the gross national product, which is merely the summation of its component sectors. The second type is the technical relation. The paradigmatic case of the technical relation is the production function, which relates output to inputs such as capital and labor. Technical engineering equations are more properly the tasks of other sciences, but the practice among econometricians has been to estimate production functions with the same statistical techniques that they use for all econometric equations. The third type is the relation describing economic actors. Equations of this type in econometric models are also called behavioral equations or decision functions. The behavioral equations in conventional econometric models are based on economic theory, and are not like the laws and theories developed in the natural sciences such as physics. Neoclassical economic theory purports to describe a decision-making process made by economic actors, notably consuming households and producing business firms. The econometric equation based on neoclassical theory contains independent variables that represent a set of conditions that are considered by the economic actors in relation to their motivating preference schedules or priorities as they make their best or optimized decisions, and the outcome of these optimizing decisions are represented by the dependent variable of the equation. The system of preference schedules is not explicitly contained in the equation, but Haavelmo says that if the system of preference schedules establishes a correspondence between sets of given conditions and optimized decision outcomes, such that for each set of conditions there is only one best decision outcome, then the economist may as it were jump over the middle link in the scheme, and say that the decisions of the individuals or firms are determined by the set of independent variables.

In this traditional neoclassical scheme the econometric model is based on the assumption that individual consumers' and firms' decisions to consume and to produce can be described by certain fundamental behavioral relations, and that there are also certain behavioral and institutional

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restrictions upon the actor's freedom. A particular system of such relationships with their equations statistically estimated defines one particular theoretical "structure." The problem of finding permanent economic laws becomes the problem of finding permanent structures in this sense; the failure in particular cases to solve this problem is usually manifested by an erroneous forecast with the model, and the failure is called structural breakdown. Haavelmo then considers several reasons for the structural breakdown of an econometric model. In all cases the problem is diagnosed as the absence from the model of a variable representing some operative factor that in reality has a significant effect on the dependent variable of the model, and the solution therefore consists of recognizing the missing factor and then of introducing a variable for it into the model. In the case of a model of a market one of the reasons for structural breakdown is a structural change due to the irreversibility of economic relations. It is a shift in a demand curve, such that price-quantity pairs do not represent movements along the demand curve, because the economic actors are revising their preference schedules as prices change. Haavelmo rejects claims that demand curves cannot be constructed from time series of observed price-quantity pairs, and instead says that the economist should introduce into his model variables representing the factors responsible for the revision of preference schedules. A second explanation for structural breakdown is the simplicity of the model. Economists like simple models, even though the real world is complex. Haavelmo distinguishes potential from factual influences in the real world, and says that models can be simple, because only factual influences need be accounted for in the models. But he says that economists making models may throw away elements of a theory, that would be sufficient to explain apparent structural breakdown that may occur later, because the elements do not exhibit a detectable factual influence over the time series history used to estimate the equation.

Finally a third reason for structural breakdown is the absence of a semantical property that Haavelmo calls "autonomy." Autonomous equations in a multi-equation model have an independence that is not just the formal independence of axioms in a deductive system. The semantical independence or autonomy is due to the success of an equation at identifying the preference schedules of just one social group or social role in the economy. For example the demand equation in a market model represents the decisions of buyers in the market, while the supply equation for the same price-quantity pair represents the decisions of sellers in the same market. If the supply and demand equations for a market model are autonomous, then a structural breakdown in one equation will not also occur in the other. An

autonomous equation is one that has successfully identified a fundamental behavioral relation described by economic theory.

In addition to his semantical theory and his theory of scientific law in economics, Haavelmo also gives lengthy consideration to statistical inference. One statistical topic he considers is the meaning of the phrase "to formulate theories by looking at the data." He is concerned with the problem of whether a well fitting statistically estimated model is merely a condensed description of the empirical data, i.e. whether it is *ad hoc*, or whether it is an effective test of a generalization. He maintains that how the economist happens to choose a hypothesis to be tested from within a class of *a priori* admissible theories is irrelevant, and he states that the selection may be made by inspection of the data. But he says that the class of admissible theories must be fixed *a priori* to the testing procedure, so that it is possible to calculate the power of the test and to determine the risk of accepting the hypothesis tested; he rejects the practice of selecting the whole class of admissible theories by the empirical testing process. The class of admissible theories cannot be made a function of the sample data, because then the Neyman-Pearson statistical test no longer controls the two types of errors in testing hypotheses, either the error of accepting a wrong hypothesis or the error of rejecting a true hypothesis. This curious commingling of statistical testing and the investigator's psychology in the Neyman-Pearson statistical inference theory will be ignored by the developers of the new type of model used by rational expectations advocates.

Mitchell's Institutionalist Critique

Haavelmo's agenda had its Institutionalist critics long before the Rational Expectations advocates. Morgan notes in her *History of Econometric Ideas* that Haavelmo's paper was very influential both within the Cowles Commission and with others including Herbert Simon. She also states that acceptance of Haavelmo's approach made econometrics less creative, because data were taken less seriously as a source of ideas and information for econometric models, and the theory-development role of applied econometrics was downgraded relative to the theory-testing role. She also notes that Haavelmo's approach was opposed by some economists including the Institutionalist economist, Wesley Clair Mitchell (1874-1948). Mitchell was instrumental in the founding of the prestigious National Bureau of Economic Research, where he was the Research Director for twenty-five years. A biographical memorial volume titled *Wesley Clair Mitchell: The*

Economic Scientist edited by Arthur Burns was published by the National Bureau of Economic Research in 1952. Mitchell's principal interest was the business cycle, and in 1913 he published a descriptive analysis titled *Business Cycles*.   Haavelmo's proposal to construct models based on existing economic theory may be contrasted with another paper published twenty years earlier by Mitchell in the latter's "Quantitative Analysis in Economic Theory" in *American Economic Review* (1925). Mitchell, who studied philosophy under the Pragmatist John Dewey, predicted that quantitative and statistical analyses in economics will result in a radical change in the content of economic theory from the prevailing type such as may be found in the works of Alfred Marshall. Mitchell said that instead of interpreting the data in terms of subjective motives, which are assumed as constituting an explanation and which are added to the data, quantitative economists may either just disregard motives, or more likely they may regard them as problems for investigation rather than assumed explanations and draw any conclusions about them from the data. In his "Prospects of Economics" in Tugwell's *Trend of Economics* (1924) he also said that economists will have a special predilection for the study of institutions, because institutions standardize behavior thus enabling generalizations and facilitating statistical procedure. He prognosticated in 1924 that as data becomes more available, economics will become a quantitative science that will be less concerned with puzzles about economic motives and more concerned about the objective validity of the account it gives of economic processes. While many neoclassical economists view Mitchell's approach as atheoretical, Mitchell had a very erudite knowledge of economic theories as evidenced in his *Types of Economic Theory* (1967).

Mitchell's principal work setting forth the findings from his empirical investigations is his *Measuring Business Cycles*, which was co-authored with Arthur F. Burns and published by the National Bureau in 1946. This five-hundred page over-sized book contains no regression-estimated Marshallian supply or demand equations. Instead it reports on the authors' examination of more than a thousand time series describing the business cycle in four industrialized national economies, namely the U.S., Britain, France and Germany. The authors explicitly reject the idea of testing business cycle theories, of which there were a great many. They state that they have surveyed such theories in an effort to identify which time series may be relevant to their interest, but their stated agenda is to concentrate on a systematic examination of the cyclical movements in different economic activities as measured by historical time series data, and to classify the time series with respect to their phasing and amplitude, in order to trace causal

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relations exhibited in the sequence that different economic activities represented by the time series reveal in the cycle's critical points. To accomplish this they aggregate the individual time series so that the economic activities represented are not so atomized that the cyclical behavior is obscured by idiosyncrasies of the small individual units.

The merits and deficiencies of the alternative methodologies used by the Cowles Commission group and the National Bureau were argued in the economics literature in the late 1940's. Selections from this literature have been reprinted by the American Economic Association in their *Readings in Business Cycles* (1965). Defense of Haavelmo's structural-equation approach was given by Tjalling C. Koopmans, who wrote a review of Mitchell's *Measuring Business Cycles* in the *Review of Economic Statistics* in 1947 under the title "Measurement without Theory." Koopmans compared Burns and Mitchell's findings to Kepler's laws in astronomy and he compared Haavelmo's approach to Newton's theory of gravitation. He notes that Burns and Mitchell's objective is merely to make generalizing descriptions of the business cycle, while the objective of the structural-equation approach is to develop "genuine explanations" in terms of the behavior of groups of economic agents, such as consumers, workers, entrepreneurs, etc., who with their motives for action are the ultimate determinants of the economic variables. Then he adds that unlike Newton, economists today already have a systematized body of theory of man's behavior and its motives, and that such theory is indispensable for a quantitative empirical economics. He furthermore advocates use of the Neyman-Pearson statistical inference theory, and calls Burns and Mitchell's statistical techniques "pedestrian."

The approach of Burns and Mitchell was defended by Rutledge Vining, who wrote a reply to Koopmans in the *Review of Economics and Statistics* in 1949 under the title "Koopmans on the Choice of Variables to be Studied and the Methods of Measurement." Vining argues that Burns and Mitchell's work is one of discovery, search, and hypothesis-seeking rather than one of hypothesis-testing, and that even admitting that observation is always made with some theoretical framework in mind, such exploratory work cannot be confined to theoretical preconceptions having the prescribed form that is tested by use of the Neyman-Pearson technique. He also argues that the business cycle of a given category of economic activity is a perfectly acceptable unit of analysis, and that many statistical regularities observed in population phenomena involve the behavior of social "organisms" that are distinctively more than simple algebraic aggregates of consciously economizing individuals. He says that the aggregates have an existence over

and above the existence of Koopmans' individual units and their behavior characteristics may not be deducible from the behavior characteristics of the component units.

Koopmans wrote "A Reply" in the same issue of the same journal. He admitted that hypothesis-seeking is still an unsolved problem at the very foundations of statistical theory, and that it is doubtful that all hypothesis-seeking activity can be described are formalized as a choice from a pre-assigned range of alternatives. But he stands by his criticism of Burns and Mitchell's statistical measures, because he says that science has historically progressed by restricting the range of alternative hypotheses, and he advocates crucial experiments. He maintains that crucial experiments deciding between the wave and particle theories of light in physics were beneficial to the advancement of physics before the modern quantum theory. He also continues to adhere to his view that it is necessary for economics to seek a basis in theories of individual decisions, and says that he cannot understand what Vining means by saying that the aggregate has an existence apart from its constituent components, and that it has behavior characteristics of its own that are not deducible from the behavior characteristics of the components. He maintains that individual behavior characteristics are logically equivalent to the group's, and that there is no opening wedge for essentially new group characteristics. In the same issue of the same journal Vining wrote "A Rejoinder", in which he said that it is gratuitous for anyone to specify any particular entity as necessarily the ultimate unit for a whole range of inquiry in an unexplored field of study. The question is not a matter of logic, but of fact; the choice of unit for analyses is an empirical matter.

Contemporary Pragmatist philosophers of science will recognize Vining's appeal to exclusively empirical criteria for deciding the unit of analysis as an application of Quine's principle of ontological relativity. And students of elementary logic will recognize Koopmans' reductionist requirement as an instance if the fallacy of composition, in which one attributes to a whole the properties of its components. Thus just as the properties of water waves cannot be described exclusively in terms the physical properties of the water molecules, so too for the economic waves of the business cycles cannot be describe exclusively in terms of the behavior of individuals. Both types of waves may be described as "real", even if their reality is not easily described as an "entity" as nominalists would require. As it happens in the history of post-World War II economics a reluctant pluralism has prevailed. For many years the U.S. Department of Commerce, Bureau of Economic Analysis, assumed the National Bureau's business cycle

leading-indicators agenda, and published many cyclical time series with charts in the "yellow pages" of their monthly Survey *of Current Business*, which is the Federal agency's principal monthly periodical. In 1996 the function was taken over by the Conference Board, which calculates and releases the monthly Index of Leading Indicators, which is based on Mitchell's approach. The forecasts are reported monthly in the national news media. On the other hand the Cowles Commission's structural-equation agenda has effectively conquered the curricula of academic economics. Today fifty years later in the universities empirical economics has become synonymous with "econometrics" in the sense given to it by Haavelmo.

Nevertheless the history of economics has taken its revenge on Koopmans' reductionist agenda. Had the Cowles Commission implemented their structural-equation agenda in Walrasian general equilibrium theory, the reductionist agenda would have appeared to have been realized. But the macroeconomics that was actually used for implementation was not a macroeconomics that is just an extension of Walrasian microeconomics; it was the Keynesian macroeconomics. Even before Smith's *Wealth of Nations* economists were interested in what may be called macroeconomics in the sense of a theory of the overall level of output for a national economy. With the 1871 marginalist revolution economists had developed an economic psychology based on the classical rationality postulate of maximizing behavior, which enabled economists to use the differential calculus to express their theory. And this in turn occasioned the mathematically elegant Walrasian general equilibrium theory that affirmed that the rational maximizing behavior of individual consumers and entrepreneurs would result in the maximum level of employment and output for the whole national macroeconomy. The Great Depression of the 1930's called this optimism into question, and Keynes' macroeconomic theory offered an alternative thesis of the less-than-full-employment equilibrium. This created a distinctively macroeconomic perspective, because it made the problem of determining the level of total output and employment a different one than the older problem of determining the most efficient interindustry resource allocation in response to consumer preferences.

This new perspective also brought in its train certain other less obvious novelties. Ostensibly the achievement of Keynes' theory was to explain the possibility of the less-than-full-employment equilibrium by the use of the classical economic psychology, the theory of value that explains economic behavior in terms of the maximizing rationality postulate. But supporters as well as critics of Keynes knew that there is a problem in

deriving a theory in terms of communities of individuals and groups of commodities from a basic theory set forth in terms of individuals and single commodities. In his *Keynesian Revolution* ([1947], 1966) the Keynesian advocate and 1980 Nobel laureate econometrician, Lawrence Klein (b. 1920, called this "the problem of aggregation", and he notes that Keynesians have never adequately addressed this problem. Joseph Schumpeter, a Harvard University economist critical of Keynes, was less charitable. In his review of Keynes' *General Theory* in *Journal of the American Statistical Association* (1936) he described Keynes' "Propensity to Consume" as nothing but a *deus ex machina* that is valueless if we do not understand the "mechanism" of changing situations in which consumers' expenditures fluctuate, and he goes on to say that Keynes' "Inducement to Invest", his "Multiplier", and his "Liquidity Preference", are all an Olympus of such hypotheses which should be replaced by concepts drawn from the economic processes that lie behind the surface phenomena. In other words this brilliant expositor of the Austrian school of marginalist economics regarded Keynes' theory as hardly less atheoretical than if Keynes had used data analysis. Schumpeter would settle for nothing other than a marginalist macroeconomic theory in the Romanticist tradition.

For the next quarter of a century economists attempted unsuccessfully to reduce macroeconomics to microeconomics, but econometricians did not wait for the approval of the likes of Schumpeter. Keynesian economics became the principal source of theoretical equation specifications for macroeconometric modeling. In 1955 Klein and Goldberger published their Keynesian macroeconometric model of the U.S. national economy, which later evolved into the elaborate WEFA macroeconometric model of several thousand equations. And this is not the only large Keynesian macroeconometric model; there are now many others, such as the DRI-WEFA and the Economy.com models, and they have spawned important information-consulting industry marketing to both business and government. But there are considerable differences among these large macroeconometric models, and these differences are not decided by reference to purported derivations from rationality postulates or microeconomic theory, even though econometricians ostensibly subscribe to Haavelmo's structural-equation programme and include relative prices in their equations. The criterion that is effectively operative in the choice among the many alternative business cycle models is unabashedly pragmatic; it is their forecasting performance.

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Muth's Rationalist Expectations Agenda

After Muth's papers, interest in the rational expectations hypothesis died, and the rational expectations literary corpus was entombed in the tomes of the profession's periodical literature for almost two decades. Then unstable national macroeconomic conditions including the deep recession of 1974 and the high inflation of the later 1970's created embarrassments for macroeconomic forecasters using the large structural-equation macroeconometric models based on Keynes' theory. These large models had been gratifyingly successful in the 1960's, but their structural breakdown in the 1970's occasioned a more critical attitude toward them and a proliferation of alternative views. One consequence was the disinterment and revitalization of interest in the rational expectations hypothesis. Most economists today attribute these economic events to the large increases in crude oil prices imposed by the Organization of Petroleum Exporting Countries or "OPEC." These events were also accompanied by large Federal fiscal deficits and by Federal Reserve expansionary monetary policies, and these macroeconomic policy actions became targets of criticism, in which the structural-equation type of models containing such policy variables was attacked using the rational expectations hypothesis.

1995 Nobel laureate economist Robert E. Lucas (1937-   ) criticized the traditional structural-equation type of econometric model. He was for a time at Carnegie-Mellon, and came from University of Chicago, to which he has since returned. Lucas' "Econometric Policy Evaluation: A Critique" in *The Phillips Curve and Labor Markets* (1976) states on the basis of Muth's papers, that any change in policy will systematically alter the structure of econometric models, because it changes the optimal decision rules underlying the statistically estimated structural parameters in the econometric models. Haavelmo had addressed the same type of problem in his discussion of the irreversibility of economic relations, and his prescription for all occasions of structural breakdown is the addition of missing variables. But Lucas does not even mention this remedy. Thomas J. Sargent, economist at the University of Minnesota and also an advisor to the Federal Reserve Bank of Minneapolis joined Lucas in the rational expectations critique of structural models in their jointly written "After Keynesian Macroeconomics" (1979) reprinted in their *Rational Expectations and Econometric Practice* (1981). They state that the verbal statement of Keynes' theory set forth by Keynes himself in his *General Theory* (1936) does not contain reliable prior information that certain variables should be excluded from the right-hand side of the structural equations of the

macroeconometric models based on Keynes' theory, and furthermore that neoclassical theory of optimizing behavior almost never implies either the exclusionary restrictions suggested by Keynes or those imposed by modern macroeconometric models. They maintain that the parameters identified as structural by current structural-equation macroeconometric methods are not in fact structural, and that these models have not isolated structures that remain invariant. This criticism of the structural-equation models is perhaps better described as criticisms of the structural models based on Keynesian macroeconomic theory, and they leave open the possibility that structural-equation business-cycle econometric models could nevertheless be constructed, which would not be used for policy analysis, and which are consistent with the authors' rational expectations alternative. But while Lucas and Sargent offer the non-Keynesian theory that business fluctuations are due to errors in expectations resulting from unanticipated events, they do not offer another structural-equation type of model. Events took another turn: what happened was the rejection of the use of expectations measurement data by the rational expectations advocates, and the consequent development of a kind of rational expectations macroeconometric model that is different from Haavelmo's structural-equation type of model.

Rejection of Expectations Data and Evolution of VAR Models

The rejection of the use of expectations measurement data antedates Muth's rational expectations hypothesis. In 1957 University of Chicago economist Milton Friedman set forth his permanent income hypothesis in his *Theory of the Consumption Function.* This is the thesis for which he was awarded the Noble prize twenty years later, and in his Nobel Lecture, published in *Journal of Political Economy* (1977) he expressed approval of the rational expectations hypothesis and explicitly referenced the contributions of Muth, Lucas and Sargent. In the third chapter of his book, "The Permanent Income Hypothesis", he discusses the semantics of his theory and of measurement data. He states that the magnitudes termed "permanent" are *ex ante* theoretical constructs, and that they cannot be observed directly for an individual consumer. He says that the most that can be observed are actual income expenditures and receipts during some definite period, and that these observed measurements are *ex post* empirical data, although they may be supplemented by verbal statements by the consu-mer about his future expenditures. Friedman explains that the theoretical

concept of permanent income is understood to reflect the effect of factors which the income earner regards as determining his capital value, his subjective estimate of a discounted future income stream. Friedman does not explain why the permanent income cannot be directly observed, why *ex ante* empirical reports of expectations cannot function as observations of the permanent income signified by the theoretical concept, nor does he describe the "supplementary" role of empirical *ex ante* measurements. Instead he poses a problem of establishing a correspondence between the theoretical constructs and the observed data. Thus Friedman poses an even more radical semantical dualism between theory and observational description than did Haavelmo. Friedman's strategy for resolving his correspondence problem is to use the statistician's idea of "expected value" of a probability distribution to isolate a permanent income component in the *ex post* measurement data. He calls the concept of permanent income an "analogy" to the statistical concept of expected value. The outcome of his semantical dualism is that empirical *ex ante* or reported expectations data are excluded from any consideration in his empirical analyses based on his theory. Muth does not follow Friedman's neo-Positivist dichotomizing of the semantics of theory and observation. In his rational expectations hypothesis he simply ignores the idea of establishing any correspondence by analogy between the purportedly unobservable theoretical concept and the statistical concept of expected value, and makes the statistical concept of expected value the literal meaning of "expectations." Friedman subdivides total measured income into a permanent part and a transitory part. He says that in a large group the empirical data tend to average out, so that their mean average or expected value is the permanent part, and the residual transitory part has a mean average of zero. In another statement he says that permanent income for the whole community can be regarded as a weighted average of current and past incomes adjusted by a secular trend, with the weights declining as one goes back further in time. When this type of relationship is expressed as an empirical model, it is a type known as an autoregressive model, and it is the type that is very strategic for representation of the rational expectations hypothesis in contrast to the structural-equation type of econometric model.

In 1960 Muth published "Optimal Properties of Exponentially Weighted Forecasts" in *American Statistical Association Journal.* Muth referenced this paper in his "Rational Expectations" paper, but this paper contains no reference to empirically gathered expectations data. Muth says that Friedman's determination of permanent income is vague, and he proposes instead that an exponentially weighted average of past observations of income can be interpreted as the expected value of the time series. He

develops such an autoregressive model, and shows that it produces the minimum-variance forecast for the period immediately ahead for any future time period, because it gives an estimate of the permanent part of measured income. The exponentially weighted average type of model had been used instrumentally for forecasting for production planning and inventory planning, but economists had not thought that such autoregressive models have any economic significance. Muth's identification of the statistical concept of expected value with subjective expectations in the minds of the population gave the autoregressive forecasting models a new economic relevance, but the forecasting success or failure of these models does not test the rational expectations hypothesis, because they have no relation to the neoclassical theory and its maximizing postulate with or without expectations.

Nearly two decades later there occurred the development of a more elaborate type of autoregressive model called the "vector autoregression" or "**VAR**" model set forth by Thomas J. Sargent in his "Rational Expectations, Econometric Exogeniety, and Consumption" in *Journal of Political Economy* (1978). Building on the work of Friedman, Muth and Lucas, Sargent developed a two-equation linear autoregressive model for consumption and income, in which each dependent variable is determined by multiple lagged values of both variables. This is called the "unrestricted vector autoregression" model. It implements Muth's thesis that expectations depend on the structure of the entire economic system; it says that all factors in the model enter into consideration by all economic actors in all their economic roles. The **VAR** model does not have Haavelmo's semantical property of autonomy, because there is no attempt to identify the factors considered in determining the preferences of any particular economic group, since each individual considers everything. In his "Estimating Vector Autoregressions Using Methods Not Based On Explicit Economic Theories" in *Federal Reserve Bank of Minneapolis Quarterly Review* (Summer, 1979), Sargent explains that the **VAR** model is not constructed with the same procedural limitations that must be respected for construction of the structural-equation model. Construction of the structural model requires firstly that the relevant economic theory be referenced as prior information, and assumes that no variables may be included in a particular equation other than those variables for which there is a theoretical justification. This follows from Haavelmo's premise that the probability approach in econometrics is the application of Neyman-Pearson statistical inference technique to equations having their specifications determined *a priori* by economic theory. But when the rational expectations hypothesis is

implemented with the **VAR** model, the situation changes because expectations are viewed as conditioned on past values of all variables in the system and may enter all the decision functions. This makes the opposite assumption more appropriate, namely that in general it is likely that movements of all variables affect behavior of all other variables, and all the econometrician's decisions in constructing the model are guided by the statistical properties and performance characteristics of the model rather than by *a priori* theory. He also notes in this article that **VAR** models are vulnerable to Lucas' critique, and that these models cannot be used for policy analyses. The objective of the **VAR** model is good forecasting with small mean squared errors.

Criticisms of structural-equation models similar to those of Lucas and Sargent were set forth by Christopher A. Sims, a colleague of Sargent then at the University of Minnesota and now at Yale University, in his "Macroeconomics and Reality" in *Econometrica* (1980), and Sims advocates the rational expectations hypothesis and the development of **VAR** models. He also states that the coefficients of the **VAR** models are not easily interpreted for their economic meaning, and he proposes that economic information be developed from these models by simulating the occurrence of random shocks and then observing the consequences described by the reaction of the model. Sims thus inverts the relation between economic interpretation and model construction advanced by Haavelmo: instead of beginning with the theoretical understanding and then imposing its structural restrictions on data in the process of constructing the equations of the empirical model, Sims firstly constructs the **VAR** model from the data, and then develops an understanding of economic structure from simulation analyses with the model. In the *Federal Reserve Bank of Minneapolis Quarterly Review* (Winter, 1986) Sims states that users of **VAR** models have been using these models for policy analysis in spite of caveats about the practice. Not surprisingly this policy advisor to a Federal Reserve Bank does not dismiss such efforts. He says that use of any models for policy analysis involves making economic interpretations of the models, and that predicting the effects of policy actions thus involves making assumptions for identifying a structure from the **VAR** model. But his technique of using shock simulations admits to more than one structural form for the same **VAR** model, and he offers no procedure for choosing among alternative structures. His approach is judgmental.

Litterman's BVAR Models and Discovery System

In his "Forecasting with Bayesian Vector Autoregression: Four Years of Experience" in the *1984 Proceedings of the American Statistical Association*, also written as a *Federal Reserve Bank of Minneapolis Working Paper*, Robert Litterman, at the time a staff economist for the Federal Reserve Bank of Minneapolis and who has since moved into the private sector, says that the original idea to use a **VAR** model for macroeconometric forecasting at the Minneapolis Federal Reserve Bank came from Thomas Sargent. Litterman's own involvement, which began as a research assistant at the Bank, was to write a computer program to estimate **VAR** models and to forecast with them. He reports that the initial forecasting results with this unrestricted **VAR** model were so disappointing, that a simple univariate autoregressive time series model could have done a better job, and it was evident that the unrestricted **VAR** is not successful. In his "Are Forecasting Models Usable for Policy Analysis?" Litterman noted that the failure of the unrestricted **VAR** model was the attempt to fit too many variables to too few observations. This failure led to the development of the Bayesian **VAR** model, and the Bayesian technique became the basis for Litterman's doctoral thesis titled *Techniques for Forecasting Using Vector Autoregression* (University of Minnesota, 1980).

In the Bayesian vector autoregression or "**BVAR**" model, there is a prior matrix, that is included in the formula for the ordinary least squares estimation of the coefficients of the model, and the parameters which are the elements in this prior matrix thereby influence the values of the estimated coefficients. This prior matrix is a substitute for the *a priori* imposition of economic theory in the conventional structural-equation econometric model as described by Haavelmo, and it also has the desired effect of restricting the number of variables in the model. Litterman argues that in the structural models there is rarely an attempt to justify the absence of variables on the basis of economic theory, despite the fact that a zero restriction on the excluded variable implies the existence of very certain prior information. He says that the use of such exclusionary restrictions does not allow a realistic specification of *a priori* knowledge. His Bayesian specification, on the other hand, includes all variables in the system at several time lags, but it also includes the prior matrix indicating uncertainty about the structure of the economy. Like Sargent, Litterman is critical of the adequacy of conventional macroeconomic theory, and he maintains that economists are more likely to find the regularities needed for better forecasts in the data rather than in some *a priori* economic theory.

The difficult part of constructing **BVAR** models is constructing a realistic prior matrix, and Litterman describes his procedure in his *Specifying Vector Autoregression for Macroeconomic Forecasting*, a Federal Reserve Bank of Minneapolis Staff Report published in 1984. His prior matrix, which he calls the "Minnesota prior", suggests with varying degrees of uncertainty, that all the coefficients in the model except those for the dependent variables' first lagged values are close to zero. The varying degrees of uncertainty are indicated by the standard deviations calculated from benchmark out-of-sample retrodictive forecasts made with simple univariate models, and the variation in the degree of uncertainty is assumed to decrease as the length of the time-lags increases. The parameters in the prior matrix are calculated from these standard deviations and from "hyperparameter" factors that vary along a continuum that indicates how likely the coefficients on the lagged values of the variables deviate from a prior mean of zero. One extreme of this continuum is the univariate autoregressive model, and the opposite is the multivariate unrestricted **VAR** containing all the variables in the model in each equation. By varying such hyperparameters and by making out-of-sample retrodictive forecasts, it is possible to map different prior distributions to a measure of forecasting accuracy according to how much multivariate interaction is allowed. The measure of accuracy that Litterman uses is the determinant of the logarithms of the out-of-sample retrodictive forecast errors for the whole **BVAR** model. Forecast errors measured in this manner are minimized in a search along the dimension between univariate and unrestricted **VAR** models. Litterman calls this procedure a "prior search", and it is unlike anything described by Simon in his theory of heuristic search. The procedure has been made commercially available in a computer system called by a memorable acronym, "**RATS**", which is marketed by VAR Econometrics Inc., Minneapolis, MN. This system also contains the ability to make the shock simulations of the type that Sims proposed for economic interpretation of the **BVAR** models.

Economists typically do not consider the **VAR** or **BVAR** models to be economic theories or "theoretical models." The concept of theory in economics, such as may be found in Haavelmo' paper, originates in the Romanticist philosophy of science, according to which the language of theory must describe the decision-making process in the economic actors' attempts to maximize utility or profits. In other words the semantics of the theory must describe the mental deliberations of the economic actors whose behavior the theory explains, and this amounts to the *a priori* requirement for a mentalistic ontology. The opposing view is that of the Positivists, or

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more specifically the Behaviorists, who reject all theory in this sense. Both views are similar in that they have the same concept of theory. The contemporary Pragmatist on the other hand reject all *a priori* ontological criteria for scientific criticism, whether mentalistic or antimentalistic, even when these criteria are built into such metalinguistic terms as "theory" and "observation." Contemporary Pragmatists instead define theory language on the basis of its use or function in scientific research, and not on the basis of its semantics or ontology: on the Pragmatist view theory language is that which is proposed for testing. Theory is distinguished by the hypothetical attitude of the scientist toward a proposed solution to a problem. Therefore, on the contemporary Pragmatist philosophy of science, Litterman's system is a discovery system, because it produces economic theories.

Ironically the rejection of the structural-equation type of econometric model by rational expectations advocates is a *de facto* implementation of the contemporary Pragmatist philosophy of science. Sargent described rational expectations with its greater fidelity to the maximizing postulates as a "counterrevolution" against the *ad hoc* aspects of the Keynesian revolution. But from the point of view of the prevailing Romanticist philosophy of science practiced in economics, their accomplishment in creating the **VAR**-type of model is a radical revolution in the philosophy and methodology of economics, because there is no connection between the rational expectations thesis and the **VAR**-type of model. Rational expectations play no role in the specification of the **VAR**-type of model. Empirical tests of the model could not test the rational expectations "hypothesis" even if it were an empirical hypothesis. And their exclusion of empirical expectations measurement data justifies denying that the model even describes any mental expectations experienced by the economic actors. The rational expectations hypothesis associated with the **VAR** models is merely a decorous discourse, a Romantic fig leaf giving the naked Pragmatism of the **VAR** models a dubious decency.

The criterion for scientific criticism that is actually operative in the **VAR**-type of model is perfectly empirical; it is the forecasting performance. And it is to this criterion that Litterman appeals. In *Forecasting with Bayesian Vector Autoregressions*: *Four Years of Experience* he describes the performance of a monthly national economic **BVAR** model constructed for the Federal Reserve Bank of Minneapolis and operated over the period 1981 through 1984. He reports that this **BVAR** model demonstrated superior performance in forecasting the unemployment rate and the real GNP during the 1982 recession, which was the worst recession since the Great Depression of the 1930's. The **BVAR** model made more accurate forecasts than the three leading structural models: Data Resources, Chase

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Econometrics, and Wharton Associates. However, he also reports that the **BVAR** model did not make a superior forecast of the inflation rate measures by the percent change in the GNP deflator. Thereafter Litterman continued to publish forecasts from the **BVAR** model in the Federal Reserve Bank of Minneapolis *Quarterly Review*. In the Fall, 1984, issue he forecasted that the 1984 slowdown was a short pause in the post-1982 recession, and that the national economy would exhibit above-average growth rates in 1985 and 1986. A year later in the Fall, 1985, issue he noted that his **BVAR** model forecast for 1985 was overshooting the actual growth rates for 1985, but he also states that his model was more accurate than the structural-equation models. In the Winter, 1987, issue two of his sympathetic colleagues on the Federal Reserve Bank of Minneapolis research staff, William Roberds and Richard Todd, published a critique reporting that the **BVAR** model forecasts of the real GNP and the unemployment rate were overshooting measurements of actual events, and furthermore that competing structural models had performed better for 1986. The Federal Reserve Bank of Minneapolis continues to publish forecasts from the **BVAR** model in its *Quarterly Review*. Reports in the Minneapolis Bank's *Quarterly Review* also contain descriptions of how the **BVAR** national economic model is revised as part of its continuing development. In the Fall, 1984, issue the model is described as having altogether forty-six descriptive variables and equations, but it has a "core" sector of only eight variables and equations, which receives no feedback from the remainder of the model. This core sector must make accurate forecasts, in order for the rest of the model to function accurately. When the **BVAR** model is revised, changes are made to the selection of variables in this core sector. Reliance on this small number of variables is the principal weakness of this type of model. It is not a vulnerability that is intrinsic to the **VAR**-type of model, but rather is a concession to computational limits of the computer, because construction of the Bayesian prior matrix makes great demands on the computer. In contrast the structural models typically contain hundreds of different descriptive variables interacting either simultaneously or recursively. Eventually improved computer hardware design will enable the **BVAR** models to be larger, but in the meanwhile they must perform heroic feats with very small amounts of descriptive information as they compete with the much larger structural-equation models containing much greater amounts of information.

Unlike Simon's simulations of historically significant scientific discoveries, Litterman cannot separate the merit of his computerized procedures for constructing his **BVAR** models form the scientific merit of the **BVAR** models he makes with his discovery system. Litterman is not

recreating what Russell Hanson called "almanac science", but is operating at the frontier of "research science." Furthermore, the approach of Litterman and colleagues is much more radical than that of the conventional economist, who needs only to propose a new "theory", and then apply conventional structural-equation econometric modeling techniques. The **BVAR** technique has been made commercially available for microcomputer use, but still the econometrician constructing the **BVAR** model must learn statistical techniques that he had not been taught in his professional education. Many economists fail to recognize the Pragmatic character of the **BVAR** models, and reject the technique out of hand, since they reject the rational expectations hypothesis. Nonetheless several economists working in regional economics have been experimenting with **BVAR** modeling of state economies. As of this writing such models are still used by the District Federal Reserve banks of Dallas (Gruben and Donald, 1991), Cleveland (Hoehn and Balazsy, 1985), and Richmond (Kuprianov and Lupoletti, 1984), and by the University of Connecticut (Dua and Ray, 1995). Only time will tell whether or not this new type of modeling survives much less achieves ascendancy in the economics profession.

Hickey's Metascience or "Logical Pragmatism"

Thomas J. Hickey received a master's degree in economics from the University of Notre Dame in South Bend, Indiana, where he also studied philosophy in their Ph.D. program. He found the economics faculty supportive, but he found the philosophy faculty obstructionist due to his prejudicial scientific realism. Since Descartes attempted to prove the existence of the real world, only a few pedantics have thought that realism is a conclusion. But the philosophy department chairman, a Reverend Ernan McMullin, told Hickey that were Hickey to persist in his views, he could never expect to succeed under their philosophy faculty, that he had a "bad attitude", and that if he preferred to leave he might do so. This threat was real, since the department chairman selects faculty for students' examination and dissertation boards. Hickey had no interest in a metaphysical criticism of science and even less interest in the reformist demand, which he viewed as contemptuous moral violence. The Reverend chairman also initiated an unprovoked denial that he wanted "to play God." But as the Reverend spoke, Hickey vividly recalled witnessing the Reverend's classroom behavior - shouting down students who expressed alternative ideas, and imputing views to students they had not expressed and then loudly attacking

the imputed views. Hickey's response to the reformist ultimatum was to drop his enrollment and write a patient farewell letter to the Reverend chairman, in which he stated that he would proceed independently to create his "dynamic model describing the development of science in a manner capable of facilitating the advancement of science", and that he would pursue his methodological interest as a new discipline, which he then called "empirical metascience." At this writing Hickey notes the obstructionist philosophy faculty he knew is still listed at the school's Internet web site. Furthermore he doubts even a faculty shakeout could remedy their shrill subculture.

Before leaving Notre Dame, Hickey had recognized that he needed to acquire computer-programming skills to implement his metascience agenda. Consequently after leaving he enrolled at San Jose College in San Jose, California, where he took coursework in numerical-analysis computer programming in the **FORTRAN** computer programming language. There he developed his **METAMODEL** computer discovery system, the "dynamic model" he had described in his Notre Dame farewell letter. He then published a description of his contemporary Pragmatist philosophy of science, his computational metatheory, and his **METAMODEL** discovery system in a brief seventy-five page monograph titled *Introduction to Metascience*: *An Information Science Approach to Methodology of Scientific Research*. This *Metascience* monograph was originally intended to be the thesis of his Ph.D. dissertation in philosophy of science at Notre Dame, and he has yet to find any previously published computer discovery system contributed to academic philosophy of science. Since publishing his *Metascience* monograph Hickey has also referred to metascience as "Logical Pragmatism", where the "Pragmatism" is the contemporary Pragmatist philosophy of science, and where the "Logic" is emphatically ***not*** the irrelevant Russellian "symbolic" logic, but instead consists of logics developed with computer languages and actually used in the empirical sciences. Hickey intends that his term "Logical Pragmatism" should not be taken as a proper name specifically for his philosophical views or systems, but rather should be taken in a generic sense, which includes alternative system designs and strategies, and admits to variations on the basic themes of the contemporary Pragmatist philosophy, but which essentially includes a mechanized procedural approach. More recently the phrase "computational philosophy of science" has also come into use, which also includes the alternative psychologistic approach, and thus is not specific to the contemporary Pragmatist philosophy and is an even more generic label than "Logical Pragmatism." This section reports Hickey's current statement of

his metatheory, the "Pragmatist" part of his Logical Pragmatist philosophy, and the next section describes his **METAMODEL** discovery system, his own contribution to the computational or "Logical" part of Logical Pragmatism.

Hickey's Linguistic Analysis

Hickey's metatheory may be summarized in terms of the four basic topics considered in philosophy of science: the aim of science, discovery, criticism, and explanation. But some preliminary comments are in order, to provide the integrating context supplied by the contemporary Pragmatist philosophy of language. Hickey contrasts his Logical Pragmatism to the alternative psychologistic approach, which descends from Simon and is found in the more recent efforts of Thagard. In this respect Hickey's linguistic constructionalism with computer systems locates him in the traditional orientation in twentieth-century philosophy of science. The contemporary Pragmatist philosophy of science has its origins in the "analytical" tradition, which began with the historic "linguistic turn" in twentieth-century philosophy, and which in the United States has since evolved into the contemporary Pragmatist philosophy of language. Hickey prefers a linguistic-analysis approach to the psychologistic approach for three reasons:

Firstly he believes that the psychologistic approach reveals a failure to appreciate the new Pragmatist philosophy of language, and he notes that advocates of the psychologistic approach typically include some residual Positivist ideas. He recognizes that the discovery systems must remain open to the fact that the empirical underdetermination of language limits the decidability of scientific criticism by all available evidence at any given time. His experience using his **METAMODEL** system has routinely revealed many alternative system-outputs that are equally acceptable empirically. Therefore he believes that the psychological approach may supply additional determination based on behavioral considerations that may operate within this range of empirical undecidability. But he has found that in research practice the resolution of this range of empirical undecidability is better described as more a matter of research strategy than behavioral psychology. He therefore maintains that psychological determinants are historically incidental, and that they are actually retarding if they operate outside the limits of the empirical constraint. He maintains that psychological and sociological considerations should not be included in the

aim of science as criteria for criticism and that they should be viewed in philosophy of science as purely circumstantial constraints, perhaps historical idiosyncrasies at best incidental to science and often obstructionist to scientific progress.

Secondly Hickey's metascience agenda with its Pragmatist statement of the aim of science and its linguistic constructionalism with computer systems makes no claims about representing human psychological processes, including representing the computation constraint that Simon found in intuitive human problem solving activity in the processes of theory development and discovery. Hickey treats psychological claims to date as he treats the claims made by metaphysicians and philosophers of mind: he dismisses them as speculative baggage lacking independent evidence and as gratuitously attributed to a functioning language-processing mechanized discovery system. In this regard he views the computational discovery system with the same practicality that the industrial engineer views the computerized robot on the assembly line of an automobile factory, because the engineer's purpose is not to replicate the practices of the human factory worker much less the worker's human limitations, but rather to achieve an improved productivity that justifies the financial investment in the robotic equipment and system. And such mechanized improvement may imply redesigning the relevant factory assembly-line practices initially designed for the human worker. Similarly the computational philosopher of science may - and probably will - effectively redesign the intuitive theory development practices of the human scientist, in order to exploit the productivity of mechanization and thereby to improve the discovery process. The computational philosopher of science need not understand the intuitive human discovery process, in order to produce a design yielding manifestly superior outcomes. He need only understand the characteristics of a good theory and develop a procedure whereby such theories can be produced mechanically and then observe that there are improved results produced with his mechanization implementation.

Thirdly Hickey believes that the psychological conceptualization of the discovery systems overlooks the sociocultural and historical character of scientific development. And by this he does not mean the sociological mechanisms of socialization and social control in the scientific community, which can be (and very often have been) retarding to the advancement of some sciences. He notes that the systems made by the computational psychologists do not actually operate independently of the historical and cultural environment; they too depend on the cultural environment for inputs that are specific to a historical time and state of science, including notably

the definition of the scientific problem under consideration. But the cognitive psychologists conceptualize their systems with little or no regard for the history and culture of the language-using community of the scientific professions. In sum: rather than view the artificial-intelligence discovery system as psychological investigation, Hickey views it as a language-processing constructional system operating under the regulation of the contemporary rationality postulate set forth in the Pragmatist statement of the institutionalized aim of modern science.

Therefore, working in the linguistic-analysis tradition Hickey selects as his point of departure some of Carnap's views, and modifies them in certain fundamental ways for a Pragmatist computational philosophy of science. Like Carnap, he distinguishes object language and metalanguage, and he views his **METAMODEL** discovery system as an object-language-processing system written in a metalanguage which includes notably the computer langauge used for the computer discovery system. Object language consists of the statements used by scientists for articulating their experimental designs and theories, which describe the extralinguistic real world. This language includes both colloquial discourse and the written symbols for which mathematics supplies the grammatical syntax. The metalanguage is used to describe the object language including the computerized systems procedures used to change the object language. Terms such as "explanation", "falsification", "theory", "test design", "state description", and "discovery system" are examples of metalinguistic vocabulary. The Logical Pragmatist philosopher of science, who may also be called a metascientist, uses the metalanguage in the process of formulating his theory, and it is therefore called a "metatheory." The computer language used in a discovery system is part of the metalanguage, and the system itself is part of the metatheory.

The inputs and outputs for the discovery system are called "state descriptions." A state description exhibits the state of the object language for a scientific profession at a point in time. This phrase is also borrowed from Carnap, but it has a very different meaning and purpose in Hickey's metatheory. The state description consists of all the object-language statements that are relevant to a scientific problem, that have explicit or implicit universal logical quantification, and that are believed to be true by at least some members of the cognizant scientific profession. Problems in basic science are not like engineering problems. A scientific problem is one that is solved by a new explanation having the status of what Quine calls an "empirically warranted belief", and the development of the new explanation changes the state description for a profession. A scientific profession is a

set of one or several scientists who are directing their problem-solving research efforts to the same problem, and they typically communicate with one another either informally in conversation or correspondence or formally in a shared published literature. Hickey's metatheory is not concerned with the psychology of the individual scientist; it is a theory about an individual scientific profession considered as a special language-using community performing a distinctive professional function regulated by shared institutional values.

One of the functions of the state description is to describe the semantics shared by the cognizant profession at the chosen point in time, or in the case of a cumulative state description, up to the chosen point of time. On Hickey's metatheory all the universal affirmative statements in a state description having the same descriptive term as a subject term, have predicates that describe the components of the meaning of the common subject term. In other words meanings have component parts. For example empirically warranted belief in the statement "all ravens are black" makes the phrase "black raven" semantically redundant, because the concept of blackness is already included as part of the meaning of "raven." Belief functions to give the universally affirmative statement a definitional role, but not exhaustively; they resemble what Carnap called "partial definitions." Thus a list of all the predicates in such universal affirmations about ravens would describe the parts of the meaning associated with the univocal term "raven." Each statement in the state description may be said to be both a synthetic statement, because it is believed to be empirically true, and an analytic statement, because belief in the statement also enables using it for semantical analysis. Hickey thus joins Quine's rejection of the analytic-synthetic distinction in so far as the distinction amounts to a separation of statements into two dichotomous classes, and he accepts Quine's rejection of an analytic theory of truth.   But Hickey does not reject analyticity as such, and he says that empirical statements believed to be true may be used analytically for semantical analysis. Statements need not be in categorical form, to be used for semantical analysis. It is sufficient that they have a descriptive function, that they make universal claims with or without explicit quantifiers, and that they be accepted as true. Mathematical equations containing descriptive variables are universal statements when their numerical measurement values are not assigned; so long as their equality condition is believed to obtain, they exhibit the components of the meanings associated with their measurement variables. However, mathematical statements are not part of what Carnap calls the "thing language", because they do not reference things, i.e. instantiated entities. They are better

described as statements in the measurement language, which make universal claims about measurement instances when variables are used in place of measurement values, and which may be accompanied by other statements in the thing language describing measuring procedures for obtaining numeric measurement values for their variables. Thus mathematical equations and inequalities statements are universal but reference measurement instances instead of instantiated entities. And they are particularly quantified when the values of their variables are specified by numeric values obtained by measurements.

The idea that meanings have parts is not commonplace in philosophy. As it happens the piecemeal nature of meanings has recently been proposed in neurology. In "Stroke Patients Yield Clues to Brain's Ability to Create Language" the *Wall Street Journal* (12 Oct. 1993) reports Dartmouth College neurologist Dr. Alfonso Caramazza as saying that the meaning of a word must be stored "piecemeal" in the brain. For example the meaning of "lemon" is the sum of many attributes that the brain has filed away separately, and cerebral strokes have been observed to damage an area of the brain where just one of the attributes is stored. The article notes that neurologists are presently using positron emission tomography (PET) to locate such storage areas physically in the brain. But Hickey's thesis is not a neurological or a physiological theory; it is a thesis in philosophy of language based on such obvious evidence as the fact that lexical entries in a dictionary display the parts of meanings, because the dictionary definitions are believed to be true. In Hickey's semantical thesis the "parts" of the meaning associated with a descriptive term are those features of the world that a language is capable of distinguishing at a given point in time. The smallest distinguishable features are almost never isomorphic to the descriptive terms of the language, such that there are no "primitive terms." The smallest distinguishable features are smaller than the meanings of terms, because they are components of the meaning complexes consisting of them. Hickey calls these more elementary parts "semantic values." The meanings associated with the terms are structured composites of these semantic values. There is no reason to believe that there are not always features and aspects of reality that either are not presently distinguished or are currently distinguished but not recognized in the descriptive vocabulary of a language. Adults recognize this growth in the discriminating ability in the semantics of the discourse of children, but such growth is not limited to children. Bilingual speakers perceive differences not adequately translated between languages. The student biologist learns to discriminate many features of animals noted by the professional biologist but unobserved by the layman

adult. In fact such semantical refinement often occurs at the moving frontiers of human knowledge including notably scientific knowledge. Examples may include new phenomena revealed by new observation instruments, such as the optical or electron microscope, the optical or radio telescope, or the X-ray photograph. And when such refinement occurs, the transition to new knowledge may be called "semantic incommensurability", by which is here meant the introduction of new semantic values that make prior language incapable of describing the new ontology. On Hickey's view and contrary to Kuhn's later view incommensurability is not merely a restructuring of available semantic values, which he calls "taxonomic categories" or "lexicon." Furthermore incommensurability does not imply complete discontinuity; it occasions only partial discontinuity, since continuity is supplied by the existing semantic values constituting the other parts of the meanings associated with the affected terms.

A state description may be called a static (or synchronic) analysis, since it pertains only to a point in time. A metatheory is a description of the transition from one state description to another in a science, and may be called a dynamic diachronic analysis. In computational philosophy of science a metatheory does not describe institutional change, but rather describes changes within the institution of science. Both empirical testing and theory development produce new state descriptions. A discovery system is a computer system that generates a new state description from the object-language input in the initial state description. The output is a terminal state description, which contains new theories. The comparative examination of a semantical change resulting from the transition to a new state description may be called a comparative static diachronic analysis. Such a semantical comparison between an initial and a terminal state description for the same scientific problem reveals which parts in the meanings of the descriptive terms in the statements remain unchanged, and which parts have changed due to the change in beliefs. Each descriptive term exhibits semantical continuity and semantical discontinuity. Thus Hickey finds it unnecessary to accept either the wholistic doctrine of "paradigm shift" advocated by Kuhn and Feyerabend or the wholistic *gestalt* theory of meaning used by Kuhn and Hanson.

Hickey's Functional Analysis

With these linguistic-analysis basics in mind turn now to the four basic topics common to modern philosophies of science, beginning with the aim

of science. Hickey's statement of the aim of science could be called a "rationality postulate" for science, if that phrase is not taken to mean that the goal statement is an incorrigible dogma, as it is in neoclassical economics. His thesis of the aim of science is an empirical hypothesis about the regulating institutional value system for empirical science that is responsible for scientific progress, and is based in the most noteworthy achievements in the history of modern science. The current statement of the aim of science is a statement in the contemporary Pragmatist philosophy of science, the philosophy of science that has evolved from philosophers' examination of the institutional evolution of twentieth-century physics, and that articulates its institutional views and values. At the opening of the twentieth century the prevailing institutional views and values were those of the Positivists. The lenses of Pragmatism enable the contemporary philosopher to recognize the dysfunctional effects of both Positivism and Romanticism, especially in current research practices in the behavioral and social sciences, even though the researchers in these sciences are oblivious to their institutional retardation. The contemporary Pragmatist statement of the aim of science may be expressed as follows:

> **Scientists aim to construct explanations by developing theories that satisfy the most critically empirical tests that can be applied at the current time.**

Historically scientists have accomplished great achievements with other aims in mind, and then later in retrospect the criteria they had actually employed are seen to be different. Newton for example denied that he created hypotheses, notwithstanding the hypothetical character of the laws of motion and gravitation. Thus the aim of science can also be re-expressed without referring to a conscious aim:

> **Science achieves explanations by developing theories that satisfy the most critically empirical tests that can be applied at the current time.**

The meaning of these statements is explained by the other three topics in the Pragmatist philosophy of science - discovery (i.e. theory construction), criticism (i.e. theory evaluation), and explanation - and it is based on the distinctively Pragmatist concept of "theory."

Basic research regulated by this institutionalized aim encounters various constraints, which scientists view as impediments to be overcome. But there is one constraint which is a voluntary constraint that scientists do

not view as an impediment to be overcome, but rather view with an attitude of obligation and respect as integral to the aim of science. That constraint is the empirical constraint; it is like a moral constraint in which scientists are socialized by their professional education, and which is reinforced by social controls that condition professional recognition upon conformity to its regulation of research practices. The operation of this institutional constraint is considered below in the discussion of scientific criticism. All other constraints are impediments to be overcome. Simon's "computation constraint" is an example of such an impeding constraint, as are other more circumstantial conditions such as limited financial resources. Furthermore there are two constraining impediments that are more than merely circumstantial; Hickey calls these the "cognition constraint" and the "communication constraint." These two are distinctive in that they are semantical constraints, which are integral to language and therefore to the final product of basic research science. And they are operative in basic research, because basic scientific research in a science depends upon the existing articulate beliefs in the current state of the science. The mechanical production of a new state description will produce a greater or lesser semantical change, depending on how radically or moderately the terminal state description revises the beliefs included in the initial state description. This change is a restructuring of the semantic values available in the initial state description, the input object language. And because there are no semantic values in the outputted theories that were not already in the input language, there can be no semantic incommensurability.

All resistance to learning involved in assimilating the outputted theories is due merely to change of psychological habit. Beliefs are the glue that bonds and structures the components of the meaning for each univocal descriptive term or for each of the meanings associated with an equivocal descriptive term. When a scientist develops a new theory that contradicts many previously held beliefs, he disassociates some of the component parts - semantic values - of the meanings of the descriptive terms and he re-associates and restructures those components according to the theses of a new theory. This semantical dissolution and restructuring has a disorienting and confusing effect due to linguistic habit, which Hanson called "conceptual resistance" that impedes the scientist's theory-developmental work. This resistance is actually a psychological one, but its impeding effect on reconceptualization is the cognition constraint. Similarly when the scientist has created a radically new theory, his colleagues to whom he attempts to communicate his new theory also experience the disorientation and confusion as they achieve the dissolution and reconstruction of their

semantics associated with habitually familiar terms. The impediment or psychological resistance that they encounter in this learning experience due to this semantical change is the communication constraint. A computerized discovery system has no psychological habits and therefore is not impeded by any cognition constraint, but the scientists who read its output may have to overcome a severe communication constraint in their attempt to "communicate" with the machine by assimilating its outputs.

Consider next the topic of scientific explanation. An explanation is the fruitful outcome of successful work in accordance with the regulating institutionalized aim of science: it is a theory that has been tested and not falsified. It is a belief which had been a theory prior to its testing, which has since been tested by the most critically empirical test available at the time, and which has not been falsified by any test to date. Such a universal statement may also be called a scientific "law". A theory in turn is a set of one or several related statements having explicit or implicit universal logical quantification, and that is proposed for testing. A law a theory that is no longer a theory, because the successful test outcome has removed its exceptionally hypothetical status relative to all other empirical universal statements accepted for the test. The law statement operates in an explanation as a premise in a deduction that concludes either to descriptions of particular events or to other universal statements. Such derived universal statements must be tested, and prior to testing they may reduce the law back to the status of a theory by enabling the further testing. The law could later be used as a test-design statement for defining another problem in another state description. Laws and therefore explanations with laws are not permanent. New test designs that resolve the vagueness in the semantics of current test-design statements thereby enabling improved measurements or observational techniques, will occasion reversion of the explanatory law to the status of a theory for retesting, and the result could be falsification. This definition of "theory" as universal statements proposed for testing and this distinction between theory and law are based in a Pragmatic definition of "theory" in terms of the use or function of theory in critical testing under the institutional regulation of the aim of science.

This contemporary Pragmatist definition of theory language in terms of its function is opposed to the earlier Romanticist and Positivist definitions in terms of some preferred semantics and ontology. The Romanticist definition still prevails in many social sciences, where "theory" language is has a semantics that describes an ontology consisting of mental states. The Positivist view defines "theory" in contrast to "observation" language, which is also alleged to have a specific semantical content such as sense

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perceptions, phenomena, or sense data. Both the Romanticist and Positivist views of the aim of science represent anachronistic institutional views and values for empirical science. Hickey believes that philosophers who continue to accept the Positivist philosophy have done so because they have not rejected the semantical distinction between observation and theory language. The contemporary Pragmatist may define "observation" language in contrast to theory in the sense that the universally quantified test-design statements supply the vocabulary and semantics for observational description. This observation vocabulary conceptualizes and articulates the perceptual experiences involved in the test situation including the report of the test outcome. But the Pragmatist distinction is not a semantical distinction; Pragmatists do not recognize any inherently observational semantics. The Pragmatist distinction is based on the strategic functions of the test-design and theory statements in the empirical test - which can and do change.

Theory language is defined pragmatically, but theories are individuated semantically. Theories are individuated for either of two reasons: Firstly two theory expressions that address different problems are different theories in the sense that they are different theories relative to different scientific professions. What is theory for one profession is not so for some other; theories are proposed solutions for a problem, and each profession is defined in terms of the scientific problem that it is addressing. What is conventionally called "a science" is actually many scientific professions. Secondly two theory expressions that address the same problem but make contrary claims are different theories, and they are different theories for the same scientific profession. This is similar to Popper's criterion for identifying different theories in crucial experiments. He says that they address the same problem in the sense that they share the same test-design statements, the language that characterizes the problematic phenomenon that the tested theories propose to explain. In an active science there may be many alternative theories in the same state description, since for each theory there need be only one member of the profession who has sufficient confidence in the theory to propose it for testing. In practice there are typically one or several minority views and a majority view, with the minority supporting the new upstart such as Einstein's theory in 1919 at the time of Eddington's eclipse experiment.

The topic of explanation pertains to the synchronic perspective, since it depends on the state of beliefs and test outcomes at a point in time. The topics of criticism and discovery pertain to the diachronic perspective, since they involve change in scientific beliefs between two points in time. These

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two functions are performed by the research scientist within the regulating institutional matrix defined by the aim of science, and they are the practices that have the effect of changing the state of the object language and therefore its state description. Both of these functions are operative in a discovery system, which produces scientific change. In the dynamic perspective consider firstly scientific criticism by empirical testing. Contemporary Pragmatist philosophy of science admits only empirical criteria for scientific criticism, and it excludes prior ontological criteria, including those required by the Positivist and Romanticist philosophies. Hickey thus joins Quine's rejection of any metaphysics or "first philosophy", which would impose any nonempirical criteria for scientific criticism. And he therefore also joins Quine's doctrines of ontological relativity and scientific realism, which were practiced by Galileo, Einstein and Heisenberg (but not Bohr), when these historic physicists affirmed that the real world is as their empirically tested and nonfalsified theories describe it. Hickey maintains a relativistic thesis of the semantics and ontology of language implied by the artifactual character of meaning, but he does not maintain a relativist thesis of truth, save for the banal fact that truth is a property of statements and is therefore relative to what is said.

The empirical criterion operates in the test of a scientific theory. At the time of the test of a theory all the statements in the state description may be viewed a segregated dichotomously into two classes: those that are proposed for testing and those that are presumed for testing. The former are the theory statements. And there may be more than one theory. The latter, the statements presumed true for testing the theory, are the test-design statements. Theory statements are included in the state description, because at least one member of the profession, presumably the proponent of the theory, believes that his theory is true. But the test-design statements are accepted as true by all the members of the profession, since these statements supply the semantics that characterize the problematic phenomena independently of any theory, identify the cognizant profession, and define the object language that is relevant and thus included in the state description. Execution of the empirical test in accordance with the previously agreed test-design changes the state description for the cognizant profession, when it eliminates one or several theories by a falsifying test outcome. By prior agreement the test-design statements are those that will be regarded as true in the event of falsification. Regardless of the test outcome, these statements contribute parts of the meanings of the descriptive terms common to both the test design and theory statements. But the parts of the meanings contributed by the theory statements change depending on whether or not the theory was

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believed true by the particular scientist before the test, and depending on whether or not the theory was falsified by the test. The advocates of the theory believed in it before the test, and therefore believed that its statements supplied a true characterization of the problematic phenomenon in addition to the definitive characterization supplied independently by the test-design statements. Both test design and theory statements contribute parts to the meaning of each univocal term common to them, until falsification makes at least one term equivocal. The falsifying test outcome motivates the proponents and advocates to reconsider, such that the semantics of their theory is no longer thought to supply a characterization of the problematic phenomenon.

However, in the event of falsification some of the theory's advocates may choose to reconsider their prior agreement about the defining role of the test-design statements. Other members of the profession may dismiss this behavior as prejudicial or foolishly stubborn. But even if the response to a falsifying test outcome is merely a stratagem to evade falsification, so long as the execution of the test is not questioned, reconsideration of the test design creates a role reversal between theory and test design. It redefines the problem into a new one and in effect creates a new state description, in which the falsified theory in the old state description assumes the role of test-design statements in the new one. Observation language is merely a change of quantification of some of the universal test-design statements, such that the reconsideration of the test design in response to falsification, which redefines the semantics and reverses the relation between theory and test design, creates a new observation language. This reversal is enabled by the artifactual character of the semantics of language, which was noted by Duhem in his thesis of physical theory, when he stated that a falsifying test does not locate the error that caused the falsifying outcome.

Furthermore reconsideration is not an irresponsible evasion of the empirical constraint discipline, when the recalcitrant advocates propose a new theory for the new problem, a theory that purports to explain why the old test design should be rejected. In fact this is the outcome of Feyerabend's counterinduction thesis, which he illustrated with Galileo's creation of a new observation language for the Copernican theory to extend the Copernican theory to redescribe the observations used as objections. The Copernican theory and its extensions became a new observation language, and Feyerabend is correct in saying that Galileo had created his own observation language.

The thesis of artifactuality is contained in Quine's "Two Dogmas of Empiricism", where he stated that it is possible to preserve the truth of any

statement by redistributing truth-values. Unlike Duhem, Quine does not limit the artifactual character of language to physical theory, and he therefore admits to no restriction on redistribution of truth-values. But while anything can be reconsidered, not everything actually is reconsidered, and there continues to exist semantical continuity due to more remote and unchanged beliefs. Complete semantic incommensurability could never occur, even when new semantic values are introduced. The web of beliefs is not a logically complete axiomatic system in which every provable theorem has been derived. It is a cultural artifact – connected but perpetually fluctuating and frayed. The propagation of semantical change is damped by vagueness, logical inconsistencies, undetected implications and continuing alterations.

Furthermore the test-design statements may be modified for reasons other than a falsifying test outcome. They may be refined by the addition of statements describing new test procedures that offer more accurate measurements or more refined observation techniques, so that the testing may be more critical. Feyerabend notes that this outcome may result from developments in "auxiliary sciences." These new test-design statements have the effect of reducing the vagueness in the semantics of the descriptive terms in the test-design statements. All descriptive language is always vague, and vagueness can never be completely eliminated, but it can in principle always be reduced. Vagueness occurs to the extent that descriptive terms have not been related to one another in universal affirmations or negations believed to be true. Refining the test design has the effect of resolving some of the vagueness in the descriptive terms in the test-design statements, and the outcome of the consequently more critical test may be the falsification of previously tested and nonfalsified theories.

Finally, turn to the topic of scientific discovery or theory development. The critical elimination of theories from the state description by empirical testing requires consideration of the state description at one point in time. But for the constructional introduction of new theories into the state description, it is necessary to consider the historically accumulated object language from both falsified and nonfalsified theories in many past state descriptions for a given scientific problem. This is because falsified theories have scrap value; their constituent descriptive vocabulary can be salvaged for new theory construction. In some circumstances the construction of new theories can be predicted and therefore effected by use of the salvaged object language in a cumulative state description. Hickey distinguishes three types of theory construction with the objective of identifying those circumstances: (1) theory extension, (2) theory elaboration, and (3) theory revision.

Given a new state description with its statements of test design that identify its scientific problem, the first type of theory construction that the cognizant profession will attempt for a new problem is theory extension. This initial conservative response to falsification suggests Quine's principle of "minimum mutilation." The existing beliefs give phenomena what Hanson called their "intelligibility", and scientists are reluctant to sacrifice intelligibility by disturbing their current beliefs. Furthermore language habits are strong, and they motivate minimizing semantic mutilation. Theory extension creates minimal disturbance to current beliefs, and it consists of using the statements of an explanation already accepted as a solution for another problem, and then extending that explanation to address this current problem, perhaps because the current problem is viewed as a special case of the solved problem. This extension is something more than just a logical transformation. It may consist of relating the explanation to the terms or variables in the test-design statements for the current problem by the addition of new statements, and these new relating statements constitute the new theory, which is tested and may be falsified. Falsification of these new relating statements would not affect the validity of the employed explanation as an explanation of the problem that it had already solved. If successive attempts at theory extension fail to solve the current scientific problem, then some of the members of the cognizant profession will become more willing to depart from the existing stock of accepted explanations. But theory extension may also employ analogy with some currently accepted but unrelated explanation. The resulting reorganization in the science in which the new analogy is applied may produce a new theory that seems quite revolutionary to the affected profession.

Theory elaboration is the next most conservative approach. It offers minimal deviance from accepted explanation, and it involves a modification to some previously proposed but since falsified theory for the problem. The falsification is typically recent and is motivated in an attempt to save the falsified theory. The modification consists of the introduction of some new descriptive term or variable as a "correcting factor" or "hidden variable", that will change the previously proposed and since falsified theory thereby transforming it into a new theory. It may also occasion introduction of new semantic values, and thus create semantic incommensurability. This effort does not "save" the falsified theory, but instead produces a new one, since the modification changes the theory's claim and its test outcome. Different members may propose different correcting factors as strategic in their theories, but their theories will typically display a recognizable similarity to the extent that they are basically modifications of shared older beliefs.

Empirical testing may result in persistent falsification of theories produced in this conservative manner. Some members of the profession will therefore become more willing to deviate more radically, and their theory construction will make new theories that bear increasingly less similarity to past theories produced by theory extension or theory elaboration. As the permutations permitted to theory construction become greater, the only remaining control on the exponentially increasing number of constructional possibilities is the size of the descriptive vocabulary in the state description. But this size approaches a limit, as the persistent failure of theory elaboration provides reason to expect that the solution to the current problem does not consist in the further search for more still hidden correcting factors, but instead consists in restructuring statements containing a selection from the descriptive vocabulary already in the cumulative state description, the last vestige of continuity with the past supplied by the test-design language and the only remaining available language.

Hickey calls this third type "theory revision", and he maintains that as increasing numbers of researchers abandon theory elaboration in favor of theory revision, the prospects increase for producing an empirically satisfactory explanatory solution by the mechanized theory revision of the object language available in the cumulative state description. The key idea in this strategy for mechanizing theory development is that the descriptive vocabulary that serves as input has been identified, is small, and is available. Hickey notes that the conditions occasioning increased use of the strategy of theory revision might resemble something similar to what Kuhn called a "crisis", and also that theory revision produces a much more radically new and different theory, that would readily be called "revolutionary." Hickey maintains that the principle of minimal mutilation dictates that the introduction of new semantic values does not typically occur during theory revision, and that the introduction of new semantic values occurs prior to theory revision. Therefore since no new semantic values are involved, there is typically no semantic incommensurability in revolutionary transitions. Ironically revisionary theory development is most often viewed as the most mysteriously muse-inspired type, while according to Hickey's metatheory the availability of object-language input from a cumulative state description makes it the type that is most easily mechanized. Mechanization takes the mystery out of musing.

Hickey does not accept Kuhn's early thesis that every scientific revolution is a wholistic gestalt switch to a new "paradigm" producing an institutional change. Nor does he accept Feyerabend's radical historicist thesis that there are semantically incommensurable revolutionary

developments involving Whorfian covert categories, even when the new theory uses a new patterning mathematics, or his thesis that science should be in a state of perpetual revolutionary change. In Hickey's metatheory of semantical description the semantical continuity through theory revision is exhibited in the unchanged semantical contribution to the descriptive vocabulary made by the test-design statements, if as Popper says, one "sticks to the problem." And the semantical discontinuity is exhibited by the semantical contribution to the descriptive vocabulary by the radically new statements constituting the new theory. Due to the semantical continuity, even the most radical scientific revolution does not create a completely new world view that is semantically incommensurable with the past and that *ipso facto* constitutes an institutional change. Thus there is no semantical basis for maintaining that radical change in theory necessitates institutional change in the science, although historically it has on a few occasions produced such change. The extent of semantical restructuring in the new theory produced by theory revision produces a correspondingly high degree of cognition constraint for the inventor working with no discovery system, and a comparably high degree of communication constraint for the profession with or without a discovery system.

Furthermore, the contemporary Pragmatist philosophy of science with its theses of semantic relativism and scientific realism liberates theory from any particular semantics and ontology. This is the institutional change belatedly recognized by philosophers of science when confronted with the development of the quantum theory, although due recognition must be given to Popper, who earlier concluded that science is "subjectless", when he was confronted with the development of the relativity theory. When the Romanticist and Positivist philosophies of science prevailed, on the other hand, they attempted to make all future scientific theory metaphysically bound to the prevailing theory's distinctive semantics and to the ontology its semantics described, thereby giving that theory institutional status. Any revision of theory therefore actually required an institutional change in the views and values in the affected science. The philosophies of science advanced by Kuhn and Feyerabend describe institutional views and values that characterize earlier periods in the history of physics, when science's institutional views, as Hanson noted, defined such concepts of explanation and causality. Pragmatism avoids this outcome by making ontological commitment depend exclusively upon empirical adequacy, rather than including any ontology in the criteria for scientific criticism. This practice of scientific realism simply means that even the more obdurate physicists and philosophers of science have learned something. Of course institutional

change will continue to occur in sciences in which Pragmatism prevails, because it is impossible to predict what the post-Pragmatist philosophy of science will look like. But in those sciences that have not yet matured institutionally the adoption of the contemporary Pragmatist philosophy of science will produce an institutional change: in due course psychology will drop Positivist Behaviorism and sociology and neoclassical economics will outgrow their retarding Romanticism. Then they will have achieved the maturity they envy in other sciences.

Hickey's METAMODEL Discovery System

Hickey is the first philosopher of science to design and create an artificial-intelligence discovery system for philosophy of science, although he is reluctant to call his system "artificial-intelligence", since no one knows what "natural intelligence" means, and since he furthermore makes no psychological claims about his system design. His **METAMODEL** discovery system constructed while at San Jose College, San Jose, CA, antedates Simon's applications of his problem-solving theory of heuristic search to the problem of scientific discovery by about ten years, and Hickey's system has an original design that is not the same as the heuristic-search discovery system design used by Simon and his colleagues at Carnegie-Mellon in the 1980's or by their later followers including Thagard. In his autobiography Simon distinguishes three types of discovery systems: expert systems, generate-and-test systems, and heuristic-search systems. Unlike Simon's heuristic-search type, Hickey's generative grammar most closely resembles the generate-and-test type of system. The generate-and-test procedure in the **METAMODEL** discovery system does not proceed through a lengthy sequence of dependent decision points. Instead the design is a combinatorial procedure that generates and tests independently a very large number of structured nonredundant combinations of language elements. The **METAMODEL** is an exhaustive cognitive exploration of revisionary theory-constructional possibilities that are latent in the input state description. The principal disadvantage of the generate-and-test design is its extensive utilization of computer resources in comparison to the heuristic-search design. On the other hand the principal advantage is that unlike heuristic search, it does not risk overlooking or preemptively excluding theories that are worthy of consideration. In other words it is not a satisficing system, but rather is an optimizing system that outputs a small number of constructionally generated and empirically tested theories. As the

computer hardware technology continues to improve, the trade-off between efficiency and thoroughness will continue to move in the direction of thoroughness. Hickey's **METAMODEL** system is designed exclusively for creating longitudinal models.

Hickey's *Introduction to Metascience* is divided into two parts. The first part is an exposition of his metatheory, as described above in its essentials. The second part sets forth the design of his **METAMODEL** discovery system together with a description of an application of the system to the trade cycle specialty in economics in 1936, the year in which John M. Keynes published his *General Theory of Employment, Interest and Money*. The **METAMODEL** performs revisionary theory construction to reconstruct the development of Keynes theory, an episode now known as the "Keynesian Revolution" in economics. The applicability of the **METAMODEL**'s revisionary theory construction for the rational reconstruction is already known in retrospect by the fact that, as Lawrence Klein says in his *Keynesian Revolution* (1966, [1947]), all the important parts of Keynes theory can be found in the works of one or another of his predecessors. The **METAMODEL** discovery system has an input and an output state description, and Hickey firstly describes the cumulative input state description containing the object language given to the system. The test-design statements are not explicitly displayed in the input state description, since they do not change through the execution of the discovery system. They consist of statements describing the phenomena symbolized by the descriptive variables occurring in the trade cycle theories that had been proposed by economists up to 1936, together with the statements describing the measurement procedures for collecting the associated data. The measurement data are those representing the U.S. national economy, which were originally published at the time in annual issues of the U.S. Department of Commerce *Statistical Abstract*, and since reprinted in their *Historical Statistics of the United States* (1958). Hickey searched both the books and the periodical literature of the economics profession for the interwar years prior to 1937, which pertained to the trade cycle problem. The American Economic Association's *Index of Economic Journals* was a useful bibliographic source, which also revealed that the number of journal articles fluctuated in close correlation with the national average unemployment rate with a lag of two years. This examination of the relevant professional literature yielded ten economic theories of the national trade cycle, which he translated into mathematical form. The ten theories were those of J.A. Hobson, Irving Fisher, Foster and Catchings, J.M. Clark, F.A. von Hayek, R.G. Hawtrey, Gusatv Cassel, Gunnar Myrdal, Johan

Akerman, and A.C. Pigou. The descriptive vocabulary occurring in these theories was a highly redundant, and yielded a set consisting of eighteen variables. The data for these variables are annual time series for the period 1921 through 1934, which were available to any economist in 1936. These time series data were converted to index numbers of period-to-period change rates, and together with variable names including one time lag are the input to the **METAMODEL** discovery system for the historical simulation. The output state description was expected to contain an econometric model of Keynes theory constructed by the discovery system. Therefore Keynes' theory like the other theories was translated into mathematical form. The theory is actually a static theory, but it was made dynamic by including considerations contained in an appendix to the *General Theory* titled "Notes on the trade cycle", in which Keynes explicitly applies his theory of income determination to the phenomenon of the trade cycle. Keynes theory contains ten variables and seven equations with three exogenous variables. All ten variables occur in more than one of the preceding trade cycle theories, and most in several of them. There is no question that all the variables needed for a recognizably Keynesian theory are available in the existing literature in 1936.

The **METAMODEL** contains two initial designations that must be made prior to execution of the discovery system in the computer. Firstly the user must designate which descriptive variables among the current-valued variables are the problematic variables, i.e. those that identify the problem the theory is to solve and also the cognizant profession. In the application to the trade cycle problem, the problematic variables are aggregate employment and aggregate real income for the national economy. Every macroeconometric model printed in the output state description generated by the system will contain these problematic variables and the equations determining their numeric values. Secondly the user must designate which among the current-valued variables are exogenous variables. These variables have their values determined for a generated model and not by it; the values are determined independently by economic policy decisions. The exogenous variables designated in the trade cycle application are Federal real aggregate fiscal expenditures, Federal real aggregate fiscal tax revenues, and the Federal Reserve's measure of the aggregate nominal money stock. These two types of designations together with other information such as the number of observations in the time series data are entered into a control record that is immediately read when the system is executed. The control record is followed by records containing the character names of the input

variables with separate identifiers for current values and lagged-valued variables, and then the time series data records follow.

The **METAMODEL** discovery system is a **FORTRAN** computer program having an architecture consisting of a main program, **SLECTR**, and two called subroutines, **REGRES** and **SOLVER.** **SLECTR** is the combinatorial procedure that selects nonredundant combinations of language elements. It contains a switch, which is initially set to the open state. When the switch is open, **SLECTR** selects combinations of time series from the input file initially read by the system, and for each selection it calls the **REGRES** subroutine. **REGRES** is an ordinary-least-squares-regression procedure that statistically estimates an intercept and coefficients thereby constructing an equation for the selection of variables passed to it by **SLECTR.** If the estimated equation does not have a satisfactory R-squared coefficient-of-multiple-determination statistic associated with it, according to a minimum value given to the system and stored in its control record, then control is returned to **SLECTR**. But if the R-squared statistic is satisfactory, the equation is stored as a record in an accumulation file before control is returned to **SLECTR**. After **SELCTR** has made all its selections for nonredundant combinations of as many as six variables, the switch is closed, and **SELCTR** repeats its combinatorial procedure.

With the switch closed **SELECTR** makes selections of estimated equations from the accumulation file previously generated by **REGRES,** and for each selection it calls subroutine **SOLVER**. **SOLVER** solves the multi-equation model as a simultaneous-equation model, and then executes the model to generate a reconstruction of the historical data. In order to accomplish this, there are certain criteria that any selection of equations must satisfy, and **SOLVER** checks for these conditions. Firstly the combination of equations constituting the model must contain equations that determine the two designated problematic variables. Secondly the model must be uniquely determined, such that there are as many current-valued endogenous variables as there are equations. Thirdly the model must be recursive, such that there is at least one current-valued variable for each lagged-valued variable describing the same phenomenon. Fourthly the model must be a minimal statement, such that it contains no current-valued variables except the problematic variables, that occurs but once in the model and is not needed to evaluate a lagged-valued variable describing the same phenomenon. When **SOLVER** finds a combination that does not satisfy all these criteria, it returns control to **SLECTR** for another combination of equations. Models that do satisfy all these criteria are capable of being solved, and **SOLVER** solves and iterates the model both to recreate the

history with synthetic data for the years 1921 through 1933, and to make a one-period out-of-sample postdictive forecast for the year 1934. The control record for the system also contains a minimum error for the forecasts of the problematic variables, and the final test for the model is for its forecast accuracy. Each model that also satisfies this criterion is outputted to the printer and printed in conventional mathematical form with each equation listed together with its associated R-squared statistic. The output also lists the synthetic data generated by the iteration of the model together with the forecast values for all its endogenous variables. The computing equipment available at the time the **METAMODEL** discovery system was created did not permit a complete operation of the system, but partial runs demonstrated that the system would generate a satisfactory Keynesian model.

There are many and various artificial-intelligence discovery system designs, but Hickey's design was motivated by his objective of using the techniques and formalisms that are actually used by econometricians. Unlike the Logical Positivists, who relied on symbolic logic to represent the language of science, Hickey wanted to use the ordinary language prevailing in the science for which he developed his discovery system. Thus his system uses the ordinary-least-squares regression statistical estimation technique for estimating the parameters of equations that are assembled into first-degree, higher-order difference equation systems. Two-stage-least-squares can be applied to the outputted models if they are not just identified. The truly noteworthy difference between Hickey and the conventional neoclassical economists using Haavelmo's agenda is their respective philosophies of science. The neoclassicals practice the Romantic philosophy of science, while Hickey is a Pragmatist. Hickey's combination of conventional econometric modeling techniques with the contemporary Pragmatist philosophy of science has proved to be very fruitful for Hickey's professional research work; he has made his living as a research economist for thirty years with his **METAMODEL** system.

Four years after designing and testing his **METAMODEL** discovery system with Keynes' theory in economics, Hickey had occasion and opportunity to use the system to address a contemporary problem in social science. At that time he was a profit analyst in the analysis and statistics bureau of the finance department of United States Steel Corporation, the largest domestic steel manufacturer. He had completed a conventionally Keynesian quarterly macroeconometric forecasting model, but found that the model was not performing satisfactorily. This occurred during the years following the large increase in crude oil prices imposed in 1973 by the Organization of Petroleum Exporting Countries (OPEC), and no

macroeconometric models available at the time had the consequences of this shock in the sample data available to estimate the models statistically. Many economists reacted to the structural breakdown of their models with patience, and waited the generation of new data. Others, however, believed that more than oil prices were at fault, and that there are more basic reasons for dissatisfaction with their models. One such group as mentioned above was the rational expectations economists, and they had their distinctive agenda.

Hickey also believed that more was involved than inadequate sample data. But unlike the rational expectations advocates he views the phenomenon of structural breakdown in the same manner as did Haavelmo, who maintained that the problem is remedied by introducing into the model new variables representing missing factors, the absence of which had caused the breakdown. But unlike Haavelmo, Hickey agrees with the Institutionalist economists that neoclassical economics limits economic explanation to an excessively small number of factors, and that it assumes incorrectly that all the other complexities in the real world are irrelevant. Furthermore Hickey is not philosophically sympathetic to the Romanticism in neoclassical economics, and he prefers the explicitly Pragmatic orientation of the Institutionalist economists. However, Institutionalists did not make econometric models; they were usually more interested in the historical evolution of economic institutions. Hickey ventured beyond conventional Institutionalists and decided to integrate functionalist sociology into his econometric model, even though functionalists do not make econometric models either. Functionalism in sociology is the thesis that all institutions of a national society are interrelated. Therefore he used his **METAMODEL** discovery system to investigate how variables representing each of the five basic institutions of the American society can be related by statistically estimated equations of the type used in econometric models. Both the sociology in the model generated with the discovery system and the truculent philosophical rejection by the Romantic sociologists to his use of the discovery system, are discussed in the sections below. An important complicating fact that is operative in the sociologists' rejection of Hickey's work, is that his system does not use the statistical and mathematical techniques that might be called the ordinary language of the sociologists; instead his system generated models for which the sociologists' education leaves them professionally incompetent and technically unprepared.

One noteworthy consideration in the present context is the modifications he made to his **METAMODEL** system, which enabled him to run it as an integrated system. In later years he had access to much better

computer equipment than the machine he used to develop the **METAMODEL**, but some modifications to the design of the system were nevertheless needed. One modification made the system store in the accumulation file only one equation of all those constructed for the same dependent variable and having the same number of independent variables. The one saved for further processing is that having the highest R-squared statistic. The effect of this modification is to reduce significantly the number of equations available for selection by **SOLVER**, and therefore to reduce the number of models generated for testing for output. However, this modification has been eliminated from the commercial version of the **METAMODEL**, which now runs without subroutine **SOLVER**. Nonetheless, the fact that in any case the system generates many alternative equations and models that are empirically acceptable is an example of the contemporary Pragmatist's thesis of empirical underdetermination of language and of scientific pluralism. For Romanticist and Positivist philosophers of science, this is an argument against development of hypotheses by data analysis, and an argument for invoking some prior ontology with its concept of causality. But for the contemporary Pragmatist, pluralism simply a routine fact of life in basic scientific research, just as it was for Einstein who called such pluralism an "embarrassment of riches."

A second modification is the introduction of a new test criterion in subroutine **SOLVER** that tests for the simulation of an inflection point in the synthetic data. A model assembled by **SOLVER** that cannot simulate in the synthetic data the occurrence in the actual data of an inflection point, is rejected by **SOLVER** and is not sent to the printer for display. The modified version of the **METAMODEL** was executed to make macrosociological models with eleven current-valued input variables, and each was allowed two lagged-valued variables. The total number of equations estimated and stored by **REGRES** for further processing by **SOLVER** was thirteen, and the total number of macrosociological models generated and critically accepted by **SOLVER** for output was three models. As it happens, two of the three models were actually the same model for reasons that **SOLVER** cannot detect, and so the total number of models actually outputted was only two. In response to inquiries during the first year of publication of *Introduction to Metascience* Hickey released a source-code listing of the **FORTRAN** statements of the **METAMODEL**, and issued it as a supplement to the monograph. This supplement also contained a listing of the input data for the sociological application and a list of the printed output models generated with the modified version of the discovery system. These functionalist macrosociometric models generated by the

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METAMODEL discovery system were intended to be used as a guide for integrating sociological, demographic, and human ecological factors into an integrated macrosociodemographic-econometric model of the U.S. national society.

However, circumstances precluded Hickey's accomplishing this more ambitious objective until the 1980's, when he was the Deputy Director of Economic Analysis and Senior Economist for the Division of Economic Analysis of the Indiana Department of Commerce. The **METAMODEL** discovery system as originally designed was inadequate to such a project, and it was necessary to revise the design. Subroutine **SOLVER** was the principal limitation; it could only make models with as many as twelve equations, while the integrated model required a number in the range of one hundred equations. Consequently Hickey designed and wrote a new **METAMODEL** discovery system that performed only the functions of **SLECTR** and **REGRES** in the old system. The new system can accept as many input variables as the installation's computer and its **FORTRAN** compiler can handle. A description of the resulting integrated macromodel was published in the state agency's *Perspectives on the Indiana Economy* (March, 1985). Later in the September 1985 issue Hickey published "The Pragmatic Turn in the Economics Profession and in the Division of Economic Analysis of the Indiana Department of Commerce", in which he described the new **METAMODEL** and compared it with some **VAR** models and the **BVAR** system constructed by the rational expectations advocates. The United States Department of Commerce has issued him a registered copyright for both the original and the commercial versions of his **METAMODEL** discovery system.

Hickey has used the commercial version of the **METAMODEL** system for many other econometric and sociodemographic modeling projects for various employers and clients including USX/United States Steel Corporation, State of Indiana/Department of Commerce, BAT(UK)/Brown and Williamson Company, Pepsi/Quaker Oats Company, Altria/Kraft Foods Company, Allstate Insurance Company, and TransUnion LLC. Monthly, quarterly, and annual versions of the system exist, and are used for both quantitative market analysis and for quantitative risk analysis. The **METAMODEL** system has been licensed perpetually to TransUnion for their consumer credit risk analyses using their proprietary TrenData aggregated quarterly time series extracted from their huge national database of consumer credit files. They use the models generated by the discovery system to forecast payment delinquency rates, bankruptcy filings, average balances and other consumer borrower characteristics that constitute risk

exposure for lenders, especially during the contractionary phase of the business cycle. Hickey has also used the system to discover the underlying sociological and demographic factors responsible for the secular long-term market dynamics of food products and other nondurable consumer goods.

It might also be noted about these market analyses that much of the success of the **METAMODEL** system is due to Hickey's Institutionalist approach in economics. A review of the membership roster of the National Association of Business Economists (NABE) reveals that economists in private industry are almost never employed in the consumer nonfinancial services and consumer nondurable goods sectors of the economy that lie outside the financial, commodity, or cyclical industrial sectors. This is due to the education offered by the graduate schools that is restricted to neoclassical economics, which has become a kind of a Romanticist ideology having the status of an orthodox theology. Employers in the consumer nondurable goods and nonfinancial services sectors, whose output accounts for approximately half of the U.S. national Gross Domestic Product, have no need for neoclassical orthodoxy. They have no need for macroeconomic aggregate income theory of the business cycle, and very limited need for microeconomic relative price theory of commodities. Microeconomic theory treats all industries as commodities in which there is only price competition to the exclusion of all franchise or branded products where advertising and other forms of nonprice competition prevail. And it treats aggregate income as the only aggregate factor to the exclusion of the many underlying sociodemographic factors considered by the Institutionalist economist. The doctrinairism of the neoclassical academic economists is costing their graduates a very high opportunity cost in lost employment opportunities. And it has also created an occupational vacuum which Institutionalist economists like Hickey have not hesitated to exploit financially.

From 1978 to 1982 Hickey submitted a paper describing his macrosociometric model developed with his **METAMODEL** system to several sociological journals. The paper was acceptable on empirical grounds. But the prevailing philosophy of science in academic sociology is still Romanticism, and since Hickey is a Pragmatist, the editors of all the journals rejected the paper for publication. Romanticism, an early philosophy of science, is still alive and well in both American academic sociology, which is still neo-Parsonian, and in neoclassical economics. Therefore before turning to Hickey's macro-sociometric model, consider firstly the Romantic philosophy of science prevailing in social science today.

Parsons' Romantic Sociology

Twentieth-century sociology and twentieth-century physics offer the philosopher of science a striking contrast. Physics saw revolutionary developments with the relativity theory and quantum theory, and these in turn occasioned the repudiation of Positivism, the nineteenth-century philosophy of science, by both the physicists and the philosophers of science. Sociology on the other hand saw no advancements like the developments in physics, and attempted to rework both Positivism and Romanticism, which contemporary philosophers of science view as anachronistic. The result has been the intellectual stagnation of sociology and the decline of its academic profession. This section examines the reworking of the nineteenth-century philosophies of Romanticism and Positivism by two sociologists, whose names are associated with these efforts in twentieth-century American sociology. The first and most influential of these is the Harvard University Romantic sociologist, Talcott Parsons. Parson's Romantic philosophy of science is very uncongenial to such modern ideas as computerized discovery systems, but his philosophy is still widely practiced and is enforced by the editors of the periodical literature of academic sociology. This overview of Parsonian Romanticism is also included here to explain its hostility to artificial intelligence.

Talcott Parsons (1902-1979) was a professor at Harvard University from 1927 until his retirement in 1973. He wrote an intellectual autobiography, "On Building Social System Theory", in *The Twentieth-Century Sciences* (1970). He had majored in philosophy at Amherst University, where he was also influenced by the Institutionalist economist, Walton Hamilton, and he studied under the anthropologist, Bronislaw Malinowski, at the London School of Economics. Parsons received his doctorate from the University of Heidelberg, where he was influenced by the views of Max Weber of Heidelberg, even though Parsons attended Heidelberg after Weber's death. Parsons' principal work is his *Structure of Social Action: A Study in Social Theory with Special Reference to a Group of Recent European Writers* (1937), an eight-hundred page that examines the social theories of four writers: Alfred Marshall, Vilfredo Pareto, Emile Durkheim, and Max Weber. This *magnum opus* is a historical study in philosophy of social science. Its thesis is that social theory has evolved beyond Positivism by an "immanent" process of development within the body of social theory, and that the outcome has been a "convergence" to a type of theory that Parsons calls the "voluntaristic theory of action." The

voluntaristic theory of social action encompasses its own philosophy of science which has evolved with it, and which in turn describes the evolution of the voluntaristic theory of action set forth in the book.

The principal figure among the four social theorists considered is Weber, whose social theory and *verstehen* philosophy of scientific criticism is represented in Parsons' work as part of an immanent development culminating in Parsons' own voluntaristic theory of action. In the present context what Weber said is of less importance than what Parsons understood and rendered Weber as having said, since it was Parsons who was the principal influence on American sociologists. In summary Weber starts with the concept of action, which he defines as any human attitude or activity, to which the actor or actors associate a subjective meaning. "Social action" in turn is action, which according to its subjective meaning to the actors involves the attitudes and actions of others, and is oriented to them in its course. Finally, sociology is the science which attempts the interpretative understanding, i.e. *verstehen*, of social action, in order to arrive at a causal explanation of its course and effects. The *verstehen* explanation is in terms of a motivation, which he defines as a meaning complex which to the actor or to the observer appears to be an adequate ground for his attitudes or acts. A correct causal interpretation of action is one in which both the outward course and the motive are correctly grasped, and in which their relation to each other is "understandable" to the sociologist in the sense of verstehen. The object of *verstehen* in Weber's methodology is to uncover the motivations that cause action.

This philosophy of science is Romantic in two respects: Firstly it requires that the language of explanation contain vocabulary that references an ontology consisting of subjective experiences of the social actors, and it defines the term "theory" in social science specifically as language describing this ontology. Secondly it requires the *verstehen* or introspectively based "understanding" of the motives described by statements referencing this ontology, as a criterion for scientific criticism, and defines "causal explanation" in terms of this *verstehen* imputation of subjective motives for observed behavior. The requirement of *verstehen* may be called a strong version of the Romantic philosophy of social science, since some Romantic social scientists accept a weaker version, in which social science explanation has the subjective ontology but is not required to satisfy the *verstehen* criterion, because the *verstehen* explanations based on the social scientist's empathy have been known to differ widely from one social scientist to another. Some Romantic social scientists who accept the weaker thesis do not believe that the social scientist should have to be able

to find an explanation convincing by reference to his personal or imaginatively vicarious experience. Historically the philosophy of science that evolved in reaction to the Romantic philosophy is Positivism. The Positivist (or Behaviorist) philosophy of science requires the exclusion of the subjective experience required by Romantic philosophy, and either redefines the meaning of "theory" to exclude any mentalist ontology or more typically just forbids all "theory." Finally the contemporary Pragmatist philosophy of science, which has evolved as a criticism of Positivism after the Second World War, rejects the thesis common to both the Romanticist and the Positivist philosophies, that ontological considerations either must or may not function as criteria for scientific criticism, and it defines "theory" by reference to its function in empirical testing rather than to any ontology.

Now consider the Parsonian neo-Weberian Romantic philosophy of science in greater detail. Weber's philosophy of social science is a variation on the distinction between natural science and social science, that originated with the Kantian philosophical separation of the phenomenal and noumenal domains, and that gave rise to the Hegelian historicist view of explanation. Unlike the German Historicists, however, Weber does not reject the use of universal laws in social science. He notes that in practical daily social life people use generalizations to make reasonably reliable predictions of the reactions of other persons to a given situation, and that they succeed by imputing motives to men, by "interpreting" men's actions and words as expressions of motives. He maintains that social scientists similarly use their access to this subjective aspect of human action, and that this access carries an immediate evidence or certainty. The natural and social sciences, therefore, differ in that the former rely on observation of external regularities or *begreifen*, while the latter have the benefit of the internal or subjective knowledge of subjective motives or *verstehen*, which are not present in the sense data of events considered in natural science. Weber postulated different aims for the natural and social sciences. On Weber's view the aim of natural science is the formulation of universally applicable general laws, while the aim of social science is description of the individual uniqueness of an actual or possible historical individual. Weber thus views social science as a historical science, while also admitting its use of general laws. Parsons rejects this correlation of natural and social science to the analytical and the historical respectively; he maintains that both natural and social science are analytical. Also in Weber's view there is a selectivity that every scientist brings to his subject, and he says that this selectivity is determined by the interest of the scientist; the basis for selectivity is the relevance of the subject matter to the values of the scientist. Furthermore, it may be noted

that Weber maintains that this value relevance is not the same as value judgments, and that scientific criticism is objective.   While recognizing Weber's thesis of value relevance, Parsons says that Weber did not lay sufficient emphasis on the fact that what is experienced is determined by a conceptual scheme, and that conceptual schemes are inherent in the structure of language. It might be said that Parsons thus anticipated in important respects the contemporary Pragmatist theory of observation two decades before the Pragmatist philosophers took it over from the physicists. Parsons says that the principle of value relevance applies to both natural and social sciences making them both analytical instead of historical sciences, and that the difference between the two types is therefore only in their subject matter and not in their logic.

While Parsons may have anticipated the contemporary Pragmatists' philosophy of observation, he had nothing like their metatheory of evidence. He notes that for Weber *verstehen* is not just a matter of immediate intuition; Weber subordinates the immediate evidence from *verstehen* to other considerations: verstehen must be "checked" by reference to a logically consistent system of concepts, which Parsons says is equivalent to the situation in the natural sciences, where immediate sense perception of natural events must be incorporated in a system of theoretical knowledge, because what is experienced is always determined by the general conceptual schemes that are already developed. Parsons says that subordination of *verstehen* to a conceptual scheme precludes uncontrolled allegations, and he affirms that Weber had a very deep and strong ethical feeling on this point. Parson's neo-Weberian Romanticism has had a retarding influence in sociology.   The editorial practices prevailing today in the academic sociological journals is that each Romantic sociologist functioning as a referee uses this subjective criterion of "meaningfulness" to advance his own conceptual schemes and to suppress publication of alternative schemes proposed by other sociologists. The result has been a caricature of scientific criticism that employs a fantasizing wizardry exhibiting such disregard for empirical evidence, that it could have startled even Baum's grand illusionist, the Wizard of Oz.   Today the iconoclastic Pragmatist philosopher of science draws back the curtain of self-delusion and exposes the Romanticists' "mechanisms."

Weber also takes up the question of how to establish the existence of a validly imputed causal relationship between certain features in the historical individual case and the empirical facts that existed before the historical event. His procedure involves the practice of historical revisionism by means of thought experiments, in which historical events are viewed as

cases to which general laws may be applied. Weber calls these cases "ideal types." He sets forth as a principal criterion for the correct formulation of an ideal type that the combination of features used in it should be such that taken together they are meaningful, that they "make sense." Parsons explains this to mean that they must adequately describe a potentially concrete entity, an objectively possible case, in terms of the action frame of reference. Two types of laws are involved in this process, both of which may occur in either the natural or social sciences; they are empirical generalizations and analytical laws. The problem of adequate causal explanation in social science is one of adequate causal imputation to make analytical laws and also involves the relation of empirical generalizations to analytical laws. In social science the elements related by the general laws may be ideal-type units, such as bureaucracy, or they may be more general theoretical categories, such as the rationality of action. The statements of general law which relate these elements may be either empirical generalizations or analytical laws. The former laws are judgments of the probable behavior under certain given circumstances of the type element. The latter are statements of general modes of interaction among the elements and are known by *verstehen*. Interestingly Parsons says that it is perfectly possible for adequate judgments of causal imputation to be arrived at in terms of type units and empirical generalizations alone, i.e. without *verstehen*. But as historical cases become more complex, adequacy of explanation may require resort to more explicit formulations of the cases as ideal types containing ideal-type units related by *verstehen*. But if this approach in turn is not adequate, it may become necessary to resort to more generalized theoretical categories and laws. The less general statements are not dispensed with in this progression from empirical generalizations to analytical laws to more general analytical theory, but the analytical laws serve as an important check on the formulations of the empirical generalizations. Parsons says that the degree to which it is necessary to push forward from empirical generalizations to analytical laws in order to attain adequate explanation, is relative to the given empirical problem at hand. He says that this process may involve probabilistic judgments, when it is necessary to make a very complex judgment of causal imputation, as in the relation of the Protestant ethic to modern capitalism. The historical individual, such as capitalism, must be analyzed into a large number of type-units, each of which is subjected to judgments of probability as to its line of development under the relevant circumstances. In this probabilistic sense Weber speaks of adequacy, when the great majority of the causally relevant type units, such as the Protestant ethic, that might have influenced a given

historical individual are favorable to the particular thesis about its development.

Parsons advances his own methodological thesis including an architectonic scheme for the sciences based on his own ontological thesis. Throughout the book he opposes the "reification" of any particular analytical theory, and particularly the reification by Positivists of either classical physics or classical economics. He considers reification to be fallacious and objectionable because it is a "monistic" realism, which requires that all realistic scientific theories be reduced to one if they are not to be regarded as fictional. Parsons proposes his own ontological thesis, which he calls "analytical realism", according to which the general concepts of science are not fictional but adequately grasp aspects of the objective external world. Some earlier philosophers had called this type of realism "perspectivism." This is the realism he affirms for those concepts in analytical laws that are ideal-type units, concepts that he calls analytical elements and that Weber had regarded as fictional. Parsons consequently rejects any reductionist view of the relation between natural and social sciences and explicitly affirms an organicist thesis of emergent properties. This emergentism is the consequence of value relevance, and it is the basis for the frame-of-reference thesis and for Parsons' architectonic for the sciences. Parsons identifies three reference frames that he calls the three great classes of theoretical systems: the systems of nature, the systems of action, and the systems of culture. Parsons says the first two pertain to processes in time and are therefore empirical, while the systems of culture pertain to eternal objects such as art forms and ideas. Examples of sciences of culture are logic, mathematics, and systems of jurisprudence, and Parsons chooses not to consider this type in his book. The empirical analytical sciences are divided into natural sciences and sciences of action. The latter are distinguished negatively by the irrelevance of the spatial frame of reference, and positively by the indispensability of the subjective aspect, i.e. *verstehen*, which is irrelevant to the natural sciences.

The action frame of reference is fundamental to social sciences. It consists in the irreducible framework of relations among analytical elements consisting of ideal-type units and is implied in the conception of these units. Common to all theoretical systems or sciences sharing the action frame of reference are structural elements consisting of ends, means, conditions, and norms. In the relations there is a normative orientation of action and a subjective point of view. These considerations are as basic to the action frame as the space-time aspect is for the framework used for physics. The sciences of action include the social sciences, which Parsons subdivides into

economics, politics and sociology, according to three defining emergent properties. The defining emergent property for economics is economic rationality, that for politics is "coercive rationality", and that for sociology is "common-value integration" which Parsons finds in the works of the four authors examined in his *Structure of Social Action*. Thus he defines sociology as the science that attempts to develop an analytical theory of action systems, in so far as these systems can be understood in terms of the property of common-value integration. These defining properties are emergent, because an attempt to analyze the system further results in the disappearance of these properties. Neither economic rationality nor common-value integration is a property of unit acts in an action system apart from their organic relations to other acts in the same action system, and the action system furthermore must be adequately complex so these properties can be observed. Consider further Parsons' ontology: Parsons says that value relevance applies equally to both social and natural science, and he rejects any implication of complete relativism by the thesis of value relevance. Following Weber he limits relativism to specific modes of its application within the action frame of reference and he excludes it from applying to the action frame itself. The reader will note that this exclusion is a completely ad *hoc* limitation. Furthermore Parsons maintains that all different conceptual schemes proceeding from different values or interests must be translatable into one another or into some wider scheme, so that the whole position is not overthrown by skepticism. This too is *ad hoc*; the history of science does not reveal such reductionism, and it is not implied by Parsons' analytical realism. Parsons is unprepared to accept the contemporary Pragmatist ontological relativity and theoretical pluralism, because he thinks such a view implies skepticism. He says that the development of scientific knowledge is to be regarded as a process of asymptotic approach to a limit, which can never actually be achieved.

In 1951 Parsons published his principal contribution to theoretical sociology, the *Social System*. This work is his implementation at a rather abstract level of the *verstehen* procedure of causal explanation, the vicarious imputation of motivations for social action. In the *Social System* he calls this implementation of *verstehen* "motivational analysis" and "dynamic analysis." Motivated behavior is action that is oriented to the attainment of gratifications or to the avoidance of depredations according to the actor's expectations as defined by the value system in the social culture. Parsons thus sets forth his "fundamental dynamic theorem of sociology": the stability of any social system depends on the integration of a common value pattern into the motivating need dispositions of the personalities of the members of

the social system. This integration is achieved by institutionalization. An institution is a cluster of interdependent role patterns, which are integrated into the personalities of the social members by motivational processes or "mechanisms" called socialization. And tendencies to deviance from these role patterns are counteracted by mechanisms called social control. These integrating mechanisms of socialization and social control produce tendencies to social equilibrium. The motivational processes operate to create and maintain social structures such as roles and institutions, and these structures in turn operate to satisfy the functional prerequisites of the social system. Parsons identifies four basic institutional role clusters, which have associated collectivities of social members, and which have their basis in four corresponding functional prerequisites for a social system. They are: (1) the family, which functions to control sex relations and to perform the socialization of new members, (2) the economy, which functions to organize the instrumental achievement roles and the stratification of the society, (3) politics, which functions to organize the roles pertaining to power, force, and territoriality, and finally (4) religion, which functions to integrate value orientations with cognitive orientations and personality. Parsons refers to his sociological theory as structural-functional. The motivational dynamics induces voluntary conformity to prevailing role patterns and thereby produces a tendency to social equilibrium. Changes produced by this tendency are changes within the existing structures of the social system. But there are also changes of the structures of the social system, which is referred to by the phrase "social change." Parsons says that a general theory of the processes of change of social systems is not possible at present, because such a theory would require a complete knowledge of the laws of the motivational processes of the system. He therefore says that the theory of change of the structure of social systems must be a theory of particular subprocesses of change within such systems, and not of the overall processes of change of the system as a system. And in this context he affirms that it is possible to have knowledge in the form of empirical generalizations that certain changes do in fact occur under certain conditions. But he still maintains that an action theory of social change must include the motivational analyses, and may not merely be a system of empirical generalizations.

Habermas on Weber

Weber's problematic views on the aim(s) of social science continue to exercise social scientists and philosophers of the social sciences. In "The Dualism of the Natural and Cultural Sciences" in his *On the Logic of the Social Sciences* (1988) Jurgen Habermas discusses an ambiguity in Weber's corpus about the problem of irrational purposeful action. This book contains a clear rendering of Weber's philosophy of social science. Ideally social science should be a combination of explanatory empirical uniformities found in the natural sciences and interpretative or hermeneutic understanding of meaning and motivations found in the cultural sciences. When the social actor chooses means that are adequate to realize his motivating purpose, the sociologist can grasp the meaning and motive of the actor and also relate the actors' behavior and its outcome in valid empirical explanations. But when the social actor's choice of means is not effective and therefore not rational, the sociologist may be able to observe an explanatory empirical uniformity in observed behavior, but not be able to impute a valid interpretative understanding. In his *Economy and Society* Weber admitted that research might discover noninterpretable uniformities underlying what appears to be meaningful action. This inconsistency gave rise to Weber's ambiguity in his attempt to relate empirical explanation and interpretative understanding. On the one hand in "Science as a Vocation" Weber values the practical and informative nature of valid empirical explanations for social policy and planning, when he says that they supply knowledge of the technique by which one masters life – external things as well as social action – through calculations. In this context Weber was willing to recognize empirical explanations without interpretative understanding, and the role of the interpretation of subjective meaning is merely to open the way to the empirical social facts. Thus Habermas says that in the context of the controversy over value judgments Weber subordinates the requirement for interpretative understanding to the requirement for empirical explanation. On the other hand he says that in other contexts Weber maintains that cultural science cannot exhaust its interest in empirical uniformities, because sociology has an aim that is different from that of natural science, and Weber was unwilling to give sociology the status of a natural science of society. In "Objectivity in Social Science" in *The Methodology of the Social Sciences* Weber views the empirical laws as only preparatory to the aim of making their basis and nature understandable, which he says is autonomous to the empirical investigation. Like most Romantics Weber had a Positivist idea of the natural sciences, but his ambiguity about method principally

originates in the conflicting aims of social science as empirical and as cultural investigations. This dualism noted by Habermas might be called "Weber's dilemma", and German Romantic that he is, Habermas, who also views natural science through the lenses of Positivist philosophy, opts for interpretative understanding. Irrational purposeful action is not exceptional. Social actors often fail to realize the consequences of their motivated actions, and may even have other consequences in mind. Merton examined at length the irrelevance of subjective motivations to objective consequences.

Merton's Critique of Parsons

Robert K. Merton (1910-2003), had studied under Parsons at Harvard University, where he received his doctorate in sociology in 1936. He was later appointed chairman of the department of sociology at Columbia University.   His dissertation, *Science, Technology, and Society in Seventeenth-Century England*, marked the beginning of his career-long interest in sociology of science. His papers in sociology of science written and published between 1935 and 1972 are reprinted in his *Sociology of Science: Theoretical and Empirical Investigations* (1973). While Merton's interest in science is noteworthy, his views in sociology of science are beyond the scope of this history. Here the focus of interest is Merton's *Social Theory and Social Structure* (1949, 1968), where he departs from Parsons' Romanticism with his own rendering of the functionalist type of explanation for sociology, and develops his own concept of scientific sociological theory.   He believes that functional analysis is the most promising yet the least codified of contemporary orientations to problems of sociological interpretation. He does not claim to have invented this type of sociological explanation, and he offers several examples of it in the literature of sociology; he says that his major concern in this book is its "codification" by developing a "paradigm" for it. He notes that sociologists often use the term "function" as it is used in mathematics to describe interdependence, but he is not thereby proposing a mathematical type of sociological theory. In fact he explicitly states that his purpose is to codify the procedures of qualitative analysis in sociology.

Merton says that the concept of social function refers to observable objective consequences and not to subjective dispositions such as aims, motives, or purposes, and that the consequences of interest are those for the larger structures in which the functions are contained. The concept of

function involves the standpoint of the observer and not necessarily that of the participant. He says that failure to distinguish between the objective sociological consequence and the subjective disposition inevitably leads to confusion. This is because the subjective disposition may but need not coincide with the objective consequence, since the two may vary independently. This concept of functional analysis occasions Merton's distinction between "manifest" function and "latent" function. Manifest functions are those that have objective consequences contributing to the adjustment and adaptation of the social system, and which are intended and recognized by the participants in the social system. Correlatively latent functions are defined as those objective consequences contributing to the adjustment or adaptation of the social system, and which are not intended or recognized by the participants in the social system. As an example Merton says that criminal punishment has manifest consequences for the criminal and latent functions for the community.

Merton's distinction is clearly valid, and has been recognized by others independently. For example William H. McNeill, who is not a sociologist but a historian of medicine, illustrates what sociologists would call "latent functions" in his *Plagues and People* (1977), a historical study in epidemiology. McNeill writes that a recent large-scale outbreak of bubonic plague, also known in earlier Europe as the "Black Death", occurred in Manchuria in 1911. Investigators discovered that the disease had been contracted from marmots, which are large burrowing rodents with skins that commanded a good price on the international fur market. The indigenous nomad tribesmen of the steppe region, where these animals live, had mythic explanations to justify epidemiologically sound rules for dealing with the risk of bubonic infection from the marmots. The tribesmen believed that departed ancestors might be reincarnated as marmots. Trapping was taboo; a marmot could only be shot, and an animal that moved sluggishly was untouchable. And if the marmot colony showed signs of sickness, custom required that human community to strike its tents and move away to avoid misfortune. Such customary practices and proscriptions reduced the possibility of human infection with plague to minor proportions. But in 1911 inexpert Chinese emigrants, who knew nothing of the tribesmen's "superstitions", hunted the marmot for their furs, trapping both sick and healthy animals indiscriminately. The result was that plague broke out among the Chinese and then spread along the newly constructed railroad lines of Manchuria. In this case the manifest function, as least to the nomads, is the proper treatment of possible reincarnated ancestors, while the

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latent function is a hygienic hunting practice that protected the hunter from a serious contagion.

Merton describes heuristic purposes for his distinction between manifest and latent functions. The distinction not only precludes confusion between motive and function, which he notes may be unrelated to each other, but it also aids the sociological interpretation of many social practices, that are regarded by observers as merely ignorant "superstitions", yet still persist even though their manifest purposes are clearly not achieved. And it also directs the sociologist's inquiries beyond the manifest or intended aspects of behavior to discover its generally unrecognized consequences. Merton thus affirms that the discovery of latent functions represents significant increments in sociological knowledge, because they represent greater departures from "commonsense" knowledge about social life. This is more philosophically sophisticated than the *verstehen* requirement that hypotheses "make sense." Furthermore he notes that the concept of latent function has significance for social policy or social "engineering." He sets forth a basic theorem, which may be called Merton's theorem of social engineering; it says that any attempt to eliminate an existing social structure without providing adequate alternative structures for fulfilling the functions previously fulfilled by the abolished organization is doomed to failure. More generally Merton's theorem says that to seek social change without due recognition of the latent functions performed by the social organization undergoing change, is to indulge in social ritual rather than social engineering.

Like Habermas' discussion of irrational purposeful action, Merton's thesis of latent functions reveals the inadequacy of the Parsonian Romantic concept of theory based on motivational analyses, but Merton furthermore recognized that a new concept of sociological theory is needed, although he does not adopt the Positivist's complete rejection of Romanticism. In his discussion of functionalism he says that in preparation for developing a functionalist explanation a fully circumstantial account of meanings, i.e. the cognitive and affective significance attached to a behavior pattern, goes far toward suggesting appropriate lines for a functional analysis. Had he been less sympathetic to the Romantics, he might have followed through to the conclusion that the distinction between manifest and latent functions contributes nothing to the explanatory value of the functionalist explanation, since its explanatory value consists not in a functional factor being either manifest or latent but in its being consequential for other factors to be explained. And this implies that the manifest-latent distinction is informative only for Romantics, who need to be told that motivational

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analysis is not adequate for explanation in social science, except as one among many possible heuristic devices for developing functionalist hypotheses.

Merton's attack on Parsonian sociology is not a frontal assault on Romanticism, but is part of his own agenda for sociological research. The attack is directed explicitly at the all-inclusive type of system building practiced by many sociologists including notably Parsons. His principal objection to these all-inclusive systems is that they are too vague to be tested empirically, and he refers to them as general orientations toward sociological analysis rather than "theories." The agenda that he advocates for future research in sociology is the development of what he calls "theories of the middle range", theories that he says are somewhere between minor but necessary empirical generalizations or working hypotheses on the one hand and the Parsonian-like all-inclusive systems on the other. Unlike the Romantics, who define theory in terms of the semantics of a vocabulary referring to subjective meanings and motives of social actors, Merton defines theory in terms of its logical structure. He explicitly defines "theory" for both natural and social sciences as a logically interconnected set of propositions from which empirical generalizations can be derived. In another statement he says theory is a set of assumptions from which empirical generalizations are derived. And referencing Lundberg's "Concept of Law in the Social Sciences" he says a scientific law is a statement of invariance that has been derived from a theory. He distinguishes theory from the empirical generalization saying that the latter is an isolated proposition summarizing observed uniformities of relationships between two or more variables. In the history of science there have been significant single-equation theories, such as Newton's theory of gravitation. But Merton does not state explicitly whether or not he intends by his definition to exclude from the domain of theory language the single-equation theories that are found in many sciences.

Referencing Whorf, Merton notes that the empirical researcher's perceptions are fixed by his conceptual apparatus, and that the researcher will draw different consequences for empirical research as his conceptual framework changes. However, Merton does not seem to recognize that this control of language over perception undermines his distinction between theory and empirical generalization, since this semantical control operates by the linguistic context of empirical generalizations, which means that empirical generalizations are never isolated. His distinction is therefore unsustainable. Had he approached this problem by an analysis with an adequate and contemporary philosophy of language, he might have seen that

his distinction incurs the same difficulty that both the Romantics and the Positivists encounter, when they purport to distinguish theory from a semantically isolated observation language. The semantics of observational description is not isolated from that of theory; semantics, logical syntax, and belief are interdependent. The only sustainable basis for distinguishing theory from nontheory language is the pragmatics of language, the functions it performs in basic research. As it happens, Merton comments on the functions of theory for empirical research. But his comments presume his distinction between theory and empirical generalizations, and are not definitive of a distinction between theory and nontheory language. Furthermore his list of functions are not applicable to the modern quantum theory, and more generally are not sufficiently universal in the practice of scientific research to serve as defining characteristics of theory language. On the contemporary Pragmatist philosophy of science the only characteristic that distinguishes theory from nontheory language is that the former is proposed for testing, while the latter is presumed for testing.

It may be noted here by way of a postscript to this discussion of Merton, that some economists also recognize what Merton calls "latent functions", even if the economists have no particular name for it. 1976 Nobel laureate economist Milton Friedman's "Methodology of Positive Economics" (1952), reprinted in his *Essays in Positive Economics* (1953), is one of the more popular methodological papers written by an economist for economists in the post-World War II era. A contemporary philosopher of science would likely view this paper as an effort to de-Romanticize neoclassical economics. Although this paper sets forth a somewhat naive semantical thesis, its semantical metatheory is more sophisticated than the neo-Positivist view in Friedman's *Theory of the Consumption Function*, and his phrase "positive economics" here does not mean Positivist economics. Like the Pragmatists, Friedman says that the only relevant test of the validity of a hypothesis is comparison of its predictions with experience; he thus accepts no ontological criteria in his view of scientific criticism, including the Romantics' mentalistic criteria involving descriptions of motivations. He explicitly rejects objections to the rationality postulates or to any other assumptions employed by economic theory, including the objections of the Institutionalist economists, when they are not based on the predictive performance of the theory. For example he notes that businessmen do not actually calculate marginal cost or marginal revenues and solve a system of simultaneous equations as do economists, and that businessmen seldom do as they report when asked about the factors affecting their decisions. But Friedman says that businessmen must act *as if* they have compared marginal

costs and marginal revenues, because they will not remain in business if their behavior is not consistent with the theory of rational and informed maximization of returns. In philosophers' terms, this means the economist is not a Romantic examining what the entrepreneur thinks, but rather is a Pragmatist examining the consequences of what he does. Or, in Merton's terms: it is the functional consequences that are relevant, and the motives are latently functional when their unintended consequence is satisfaction of the marginalist conditions set forth in neoclassical economics.

Lundberg's Positivist Sociology

Parsonian Romanticism has not been without its critics. Not surprisingly the science that was founded by the founder of Positivism, namely Auguste Comte, has offered new Positivist critics to oppose Parson's latter-day variant of Romanticism. The principal protagonist in this critical role, who was contemporary to Parsons, was George Lundberg (1895-1966). As it happens, Lundberg's criticisms did not effectively persuade American sociologists, and post-World War II sociology took the Parsonian path. Nonetheless a brief rendering of Lundberg's criticism will describe the philosophy which for many years American academic sociologists viewed as their principal philosophical alternative to Parsons. Lundberg traces his philosophical heritage to Comte. In his "Contemporary Positivism in Sociology" in *American Sociological Review* (1939) Lundberg gives three quotations from Comte's *Positivist Philosophy*, that he says suggest the principal survivals and modifications of Comte's work that may be regarded as contemporary Positivism in sociology. The first quotation is a statement of the principal aim of science, according to which the business of science is to analyze accurately the circumstances of phenomena, to connect them in invariable natural laws according to the relation of succession and resemblance, and to reduce such laws to the smallest possible number. The second quotation sets forth a secondary aim of science, namely to review existing sciences to show that they have a unity of method and a homogeneity of doctrine. The third quotation affirms the importance of observation and rejects the view that the sciences of human behavior should attempt to study facts of inner experience. Lundberg finds himself at variance with Parsons, and he quotes anti-Positivist comments from a lengthy footnote in Parsons' *Structure of Social Action*, in which Parsons states that all Positivisms are untenable for both empirical and methodological reasons.

Lundberg wrote several methodological works. His principal philosophical work is a monograph of about one-hundred fifty pages titled *Foundations of Sociology* (1939), which includes his views set forth in a previous papers including one titled "Concept of Law in the Social Sciences" published in *Philosophy of Science* (1938). The 1964 edition of the *Foundations* monograph contains an "Epilogue" as a new chapter, in which Lundberg maintains that the Parsonian approach to sociology is converging toward the Positivist view. In 1929 he wrote *Social Research: A Study in Methods of Gathering Data*, which he extensively revised in 1942. In 1947 he wrote *Can Science Save Us?* and in 1953 he co-authored *Sociology*, a textbook in seven parts with a methodological discussion constituting the first part, and that went through four editions.

Lundberg was very impressed by the successes of natural science especially in comparison to sociology, and he stated that the history of science consists largely of the account of the gradual expansion of the realms of the natural and physical at the expense of the mental and the spiritual. His agenda for sociology therefore is to realize success in sociology by imitating the methods of the natural sciences. The philosophical understanding of natural science during the time of his active career was the Positivist philosophy, which also prevailed in academic philosophy of science at the time. But the classical Machian Positivism implemented in the natural sciences with its phenomenalist ontology is not easily adapted to behavioral and social sciences, and Lundberg therefore developed his own Pickwickian Positivism for sociology. Lundberg's epistemological view has similarities to the classical British empiricists, Locke, Berkeley and Hume, and also to the early Positivists such as Mach. These philosophers started with the thesis that what the human mind knows immediately is its own ideas, sensations, or sense impressions. This is a subjectivist view that occasions the question of how the human mind knows the external or extramental real world. One answer to this problem is the copy theory of knowledge, according to which ideas reveal reality, since they are copies of reality. Another answer is that there is no external world consisting of material substances, such that the ideas themselves become reified, and the result is an idealist and solipsistic thesis, such as Berkeley's *esse est percipi*, "to be is to be perceived." Lundberg also has a subjectivist theory of knowledge, but he has his own solution to the problem of knowledge of reality. Lundberg maintains that the immediate data of all sciences are symbols, by which he means human responses to whatever arouses the responses. And he also calls these responses sensory experience. His subjectivist philosophy of knowledge is nonrealist, because it makes

subjective experience instead of extramental reality an object of knowledge rather than making experience constitutive of knowledge. He then goes on to say that the nature of that which evoked these human responses must be "inferred" from these immediate data which are our sensory experience; we infer both the existence and the characteristics of anything from these responses. In his Positivism there are apparently some extramental realities beyond the phenomena. Furthermore this "inference" of the characteristics of reality is not a deductive inference, but consists of operationalist definitions. In his discussion of measurement Lundberg says that since Einstein, physicists have blatantly declared that space is that which is measured by a ruler, that time is that which is measured by a clock, and force is that which is measured by pointers across a dial. A thing is that which evokes a certain type of human response represented by measurement symbols. There is an ironic aspect to Lundberg's epistemological subjectivism, because he uses it to refute the view that the subject matter of social science is subjective, arguing that distinctions between what is subjective and what is objective is not given in the data. Thus objectivity is not given in things, but in those ways of responding that can be corroborated by other persons. He seems unaware that corroboration to establish objectivity or intersubjectivity is itself quite problematic for any subjectivist philosophy of knowledge.

The most distinctive aspect of Lundberg's version of Positivism is his rejection of the naturalistic philosophy of the semantics of language. In discussing quantification he rejects any distinction between natural and artificial units for measurement, and he denies that scientists measure the behavior of some things but not the being, quality or quantity of others. He argues that like physicists, sociologists must recognize that all units are artificial linguistic constructs symbolizing human responses to aspects of the universe relevant to particular problems. Lundberg also implicitly recognizes that his semantical view affirms an ontological relativity, when he says the human knower infers the "nature" of phenomena from his symbolic responses. But his ontological relativity contrasts with that of the contemporary Pragmatists, who are realists and who maintain that the human knower knows reality directly and not indirectly by any kind of inference. Lundberg's rejection of the naturalistic philosophy of the semantics of language absolves him from any need to characterize the observational basis of science. He thus evades a difficult problem for a social or behavioral science attempting to implement the phenomenalist thesis of the Positivist physicist or chemist. Human social behavior is not easily or productively described in terms of phenomenal shapes, colors, sounds, or other

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purportedly elementary sense data. In contrast the Vienna Circle sociologist, Otto Neurath, was *ad hoc* in his attempt to accomplish the same thing, when he simply announced that a "thing language" as opposed to a phenomenalist language is admissible in Logical Positivism. More importantly Lundberg's artifactual thesis of semantics is strategic to his agenda for rejecting the view that sociology has a distinctive subject matter, i.e. distinctive in its subjective nature, since human knowledge does not immediately apprehend the nature of things. But rejection of the naturalistic semantics undercuts Lundberg's agenda of eliminating vocabulary conventionally referencing subjective experience as opposed to observably objective behavior. His philosophy of the semantics of language does not admit the distinction he tries to enforce as a condition for a scientific sociology.

Lundberg offers several statements of the aim of science. In one statement he says that the primary function of all science is to formulate the sequences that are observable in any phenomena, in order to be able to predict their recurrence. In another he says that the goal of all science is the formulation of valid and verifiable principles as laws comprehending with the greatest parsimony all the phenomena of that aspect of the cosmos which is under consideration. He defines a scientific law in turn as a verifiable generalization within measurable degrees of accuracy of how certain events occur under stated conditions, and he defines a theory as a deductive system of laws. A central thesis in Lundberg's agenda for a natural science approach to sociology is that scientific law in social science means exactly what it means in natural sciences. He therefore rejects any distinctive type of scientific law based on *verstehen*, and he says that understanding in his sense is not a method of research, but rather is the end to which the methods aim. Lundberg's philosophy of scientific criticism is verificationist, and in his textbook he defined a law as a verified hypothesis.

Lundberg offers several statements on the nature of scientific explanation, the topic in which he is most fundamentally at variance with the Romantic sociologists. In one brief statement he says that something is explained or understood, when the situation is reduced to elements and correlations among the elements, which are so familiar that they are accepted as a matter of course, and curiosity is then put to rest. He defines an "element" as any component that is not in need of explanation or of further analysis. Another of his statements is given in terms of his thesis of frames of reference. Problematic data are said to be explained when they are incorporated into previously established habit systems of response, which constitute frames of reference. When this is accomplished, the new observations are said to have "meaning" and to be "understood." Consistent

with his rejection of naturalistic semantics he says that frames of reference are not inherent in the universe, but are pure constructions for our convenience. He states that the scientist's interest in a problem requiring a response defines the categories in terms of which he reports his experience. When he seeks an explanation, he seeks to associate data reporting the problematic experience with what he already knows, i.e. the familiar, described by his established habit systems of response, which is the relevant frame of reference.

The frame of reference Lundberg considers appropriate for a natural science of social phenomena is behaviorism. In his *Foundations* he references a passage from Robert K. Merton's "Durkheim's Division of Labor" in *American Journal of Sociology* (1934), a relatively early work in Merton's literary corpus, in which Merton states that on the Positivist thesis, which says that science deals only with empirical facts, a science of social phenomena becomes impossible, since it relegates to limbo all ends, i.e. subjective anticipations of future occurrences. Lundberg says that this view fails to recognize that anticipated ends in the sense of conscious prevision exist as words or other symbols to which the organism responds, just as it does to other stimuli to action. In the behavioristic framework words are entities that are just as objective as physical things. No relevant data, even those designated by such words as "mind" or "spiritual" are excluded from science, if these words are manifest in human behavior of any observable kind. Like most Positivists Lundberg is unaware that the meaning of "observable" is philosophically quite problematic. Later in his *Can Science Save Us?* (1947, 1961) he further comments about the word "motives" in relation to frames of reference. He says that it is a word used to designate those circumstances to which it seems reasonable to attribute an occurrence, and that therefore it can have different meanings depending on the frame of reference in which it is used. Lundberg believes that of all reference frames the scientific frame of reference has proved to be the most successful for human adjustment to the environment.

The type of explanation that he explicitly advocates for sociology is what he calls the "field" type, which he also calls relational and situational, and which he opposes to types that refer to unexplained innate traits of social agents. He compares the idea of field to the idea of space as it is used in geography and ecology. The geographer describes behavior in terms of symbolic indices such as birth rates, death rates, and delinquency rates, for a geographical region, and then he correlates these indices. The transition from an ecological map representing delinquency rates as gradients to an organizational or functional representation for sociology involves a

transition from a geographical to a social "space" and from a pictorial to a more abstract symbolic representation such as functional equations relating measurements. In "Social Bookkeeping", the concluding chapter of his *Social Research*, Lundberg notes that national demographic statistics have routinely been collected, and that social scientists have made successful objective generalizations on the basis of these data. He maintains that quantitative sociological laws can be just as objective as demographic generalizations.

In the concluding "Epilogue" chapter of the 1964 edition of his *Foundations* Lundberg describes similarities between Parsons' sociology and that of Stuart Dodd. Lundberg takes Dodd's work to be exemplary of the natural science approach in sociology. Dodd was chairman of the Sociology Department at the American University in Beruit, Lebanon. Dodd describes his *Dimensions of Society: A Quantitative Systematics for the Social Sciences* (1942) as a "companion volume" to Lundberg's *Foundations,* which Dodd reports he had sent to Lundberg for prepublication criticism. This distinctive book and its sequel, *Systematic Social Science: A Dimensional Sociology* (1947), set forth a social theory called the *S*-theory, which implements Lundberg's philosophy of science. Dodd's 1942 text contains a distinctive notational system for elaborately describing social "situations" in terms of four "dimensions": the demographic, the cultural, the ecological, and the temporal. The 1947 text contains representations for eleven social institutions. But the symbols in this notational system serve principally as a kind of shorthand, and seem not to be subject to mathematical computation or transformation, as are theories in natural science. American sociologists did not accept Dodd's *S*-theory or his approach. However, even if the *S*-theory had been mathematical as is, say, Newtonian mechanics or contemporary mathematical economics, the academic sociologists would not have accepted it anyhow, because they are too incompetent in mathematics to assimilate it.

Parsons and Lundberg offer surprising ironies in their attempts at philosophy of science. Each for reasons of his own surpassed the naturalistic thesis of the semantics of language that is common to both the Positivist and the Romanticist traditions in philosophy, and in this respect each had surpassed the academic philosophers of science who were contemporary to them in the 1930's and 1940's. Both of them affirm the artifactual thesis of semantics, the view that the semantics of language is a cultural artifact rather than a product of nature. In this respect these social scientists enjoy the benefit of a professional perspective uncommon at the time to the academic philosophers preoccupied with the philosophy of

physics. Ironically, however, neither Parsons nor Lundberg exploited the implications of their philosophically superior view of semantics, because each brought his own agenda to his ersatz philosophizing efforts, which in each case is incompatible with the artifactual-semantics thesis and the realistic epistemology.

Lundberg arrived at his artifactual-semantics thesis at the expense of realism, because he carried forward a subjectivist epistemology from the Positivist philosophy. And his fidelity to Positivism cost him any basis for the objectivity that he thought justifies his natural-science agenda for social science. Historically the Positivist basis for objectivity with the subjectivist epistemology is the naturalistic-semantics thesis of language. The copy theory of knowledge is an old example of a strategy for objectivity with the subjectivist phenomenalist epistemology. Bridgman's operationalist definition is a more contemporary case, which ironically Lundberg calls upon as the basis for his view that the gap between the subjective responses constituting sensory experience and the objective real world is mediated by an inferential process consisting of operationalist definitions. Lundberg may not have realized that both operationalist definitions and the Positivist concept of observation are based on the naturalistic view of semantics.

Parsons arrived at his artifactual-semantics thesis in a more sophisticated manner, when he said that all observation is in terms of a conceptual scheme, and when he said that there is a relativity or selectivity in the conceptual scheme resulting from the value relevance or interest of the scientist. This relativism is consistent with the artifactual-semantics thesis, and is not consistent with the naturalistic-semantics that says the information in concepts is absolutely fixed and predetermined by nature. Furthermore Parsons' approach to the artifactual-semantics thesis is consistent with his realistic epistemology, which he calls "analytical realism." Analytical realism enables scientific observation to describe aspects of the real world with semantics supplied by the value-relevant conceptual scheme. In these noted respects Parsons' philosophy of science is truly post-Positivist, as he had claimed. But there is a problem, which he attempted to finesse: the artifactual-semantics thesis cannot support his agenda for a voluntaristic theory of social action. This agenda requires a naturalistic-semantics thesis that would enable Parsons to say that such aspects of reality as ends, norms, or motives are not observable in human behavior, but are causes that must be supplied by imputation by the social scientist by reflection on his own experience, that is by *verstehen*. In order to implement his agenda, Parsons says that the relativism introduced by value relevance obtains within the frames of reference for the natural sciences and for voluntaristic action, but

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does not obtain between them; and on this basis he distinguishes empirical generalizations about human behavior appropriate for natural sciences from the "analytical laws" appropriate to the action frameworks formed by *verstehen*. This thesis is a completely *ad hoc* addendum that is inconsistent with the artifactual-semantics thesis for language.

The claim made by Parsons that ends, norms, and motives are not observable is erroneous, and it is not erroneous due to behaviorism, as Lundberg maintains. Contrary to Lundberg behaviorism is also dependent on a naturalistic-semantics thesis of language. It is erroneous because, as Parsons says, all observation is in terms of a conceptual scheme, and this means that there is an intellectual component in observation supplied by the linguistic context constituting the conceptual scheme. Contemporary Pragmatists, such as Hanson, have expressed this by saying that observation is "theory-laden." Einstein asserted the same thesis when he told Heisenberg that theory decides what the physicist can observe. The electron was observable by Heisenberg in the Wilson cloud chamber because his quantum theory supplied the conceptual scheme that supplied elementary intelligibility for the phenomenon to be observably recognizable as an electron's track. Similarly the *verstehen* interpretation supplied by the Romantic sociologist is no less contributing to the semantics of the language describing observed human behavior than the quantum theory is to the semantics of the observation report of the vapor tracks in the Wilson cloud chamber. Parsons noted that Weber required that the causal imputation by *verstehen* be checked by reference to a logically consistent system of concepts, which Parsons says is equivalent to the situation in the natural sciences where immediate sense perception must be incorporated into a system of theoretical knowledge. On the Pragmatist view, however, it is the whole theoretical system of beliefs including the *verstehen* analytical laws that is "checked" by empirical testing.

Both Weber and Parsons seem to have failed to see that there can be no requirement for the *verstehen* concept of causality in the sciences of human behavior, just as there is no requirement for the Newtonian or Aristotelian concepts of causality in physics. Weber's and Parsons' attempt to impose such a requirement as a condition for causal explanation in social science, is now recognized to be a fallacy: the fallacy of demanding ontological criteria for scientific criticism. On the contemporary Pragmatist philosophy of science only empirical criteria may operate in scientific criticism. The artifactual-semantics thesis makes all ontologies as dispensable as the empirical theories whose semantics describe those ontologies, and it makes all theories subject only to empirical criticism

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without regard to how improbable or counterintuitive empirically adequate theories may seem to the individual scientist.

The METAMODEL System Applied to Sociology

In 1976, five years after Hickey left Notre Dame and three years after he developed his **METAMODEL** discovery system, he used the system to develop a macrosociometric theory of the American national society with historical time series data describing fifty years of American history. In order to display the Romanticist philosophy of science that still prevails in American academic sociology, this section firstly summarizes Hickey's functionalist macrosociometric theory, and secondly examines the responses of the editors and referees of four academic sociological journals to which Hickey had submitted his paper setting forth his macrosociological model and findings. The editors of all four sociological journals refused to publish Hickey's paper. Hickey has retained all the original correspondences from these editors. In his paper Hickey described his discovery-system generated macrosociometric model as a "quantitative functionalist theory of macrosocial change", and he contrasted it with Parsons' structural-functionalist approach, which Hickey called "classical functionalism." Classical functionalism is a social-psychological theory in the Romantic tradition concerned with the institutionalization of patterns of value orientations. It explains social order and stability by the analysis of motivational processes or "integrative mechanisms" of socialization and social control, which integrate social actors' need dispositions into cultural patterns that always include value orientations. Parsons called this process of integration the fundamental dynamic theorem of sociology and the core phenomenon of the dynamics of the social system. When this stability or equilibrium is extended throughout the macrosociety, the result is called consensus equilibrium.

Hickey noted that the classical functionalist theory does not explain social change. As Parsons stated, the institutionalization of cultural-value orientations by these integrative mechanisms relate to social change only as forces of resistance except to the degree that the macrosociety is described as malintegrated, in which case these mechanisms legitimate deviant behavior. Hickey thus maintains that Parsons' paradigm of motivational processes is not an adequate theoretical basis for the analysis of macrosocial change, and he rejects the reductionist dogma that a macrosociological theory must be built up from a social-psychological or microsociological

analysis of motivational processes, as had been envisioned by classical functionalists. Hickey also invokes the definition of functionalism in terms of consequences, and references the distinction between manifest and latent functions proposed by Merton, to enable sociological theory to explain the unintended and unforeseen consequences of social actors' behavior that cannot be explained in terms of conscious motivational processes. Furthermore as a Pragmatist, Hickey maintains that sociological theory like any scientific theory permits but does not require a Romantic ontology for a valid sociological explanation. Accordingly while his basis for his selection of distinctively sociological variables in his theory is that these variables reference the fact that the behavior of the social actors is voluntary group associational behavior that reveals cultural values distinctive of particular social institutions, he admits to no requirement that he structure his equations on the basis of any postulated motivations, nor does he admit to the more extreme demand of some Romantics that such motivations be empathetically based and imputed to social actors in accordance with the *verstehen* criterion for sociological explanation and criticism.

Hickey's approach is not only an alternative to the reductionist social psychology of classical functionalism that still prevails in sociology, it is also an alternative to attempts to reduce demographic and sociological phenomena to economic motives, as has been proposed by 1992 Nobel laureate economist, Gary S. Becker (1930-   ), a University of Chicago professor of economics and sociology. Becker is a Romantic economist, who employs the neoclassical economists' rationality postulate to explain such sociological phenomena as marriage, divorce, education and crime, and also demographic phenomena. He maintains that marriage, family size, education, etc. are economic decisions, because they involve incentives. His theory-of-choice approach based on rationality postulates assumes the calculating attitude (for which he actually develops calculations), that sociologists contrast to the attitude of respect and of voluntary conformity that is institutionalized by socialization and social control. Furthermore, while Hickey's approach is related to the Institutionalist tradition in economics, he goes beyond Institutionalist economics to include consideration of all of the five basic institutions in the macrosociety, and he models the functional relations among them. Hickey uses the distinctively sociological perspective for the explanation of these sociological phenomena; it is not reducible to neoclassical economic theory, nor is it merely an extension of Institutionalist economics. Hickey does not exclude or reject the effects of economic conditions from his explanatory equations;

in fact he explicitly includes economic variables. He proposes a distinctively macrosociological perspective that includes macroeconomic conditions.

The variables in Hickey's theory fall into two broad classes: (1) the institutional variables representing types of groups associated with each of the five basic social institutions, and (2) noninstitutional factors. The institutional variables are sociologically relevant, because they are *per capita* rates having numerators describing aggregate voluntary group-associational behavior, thereby making the *per capita* rates measures of voluntary consensus within the macrosociety. They measure degrees of consensus on undefined scales of more-or-less about the institutionalized cultural value systems distinctive of each of the five basic types of institutional groups. Consensus represents the extent of integration of the members of society about the institutional values, integration that is necessary for the type of group to function and to continue in existence. A decline in consensus about the institutional values results in an increase in the incidence of dissolution of the associated type of group. The five basic institutional groups are the family or domestic type of group, the church or religious type of group, the school or educational type of group, the business enterprise or economic type of group, and the law-governed macrosociety itself. The family group is represented by the *per capita* marriage rate and also by the *per capita* divorce rate. These two variables describe new family formation and dissolution respectively. The religious type of group is represented by the *per capita* rate of religious affiliation. The educational institutional group is represented by the percent of seventeen/eighteen-year olds that graduate from high school. The economic type of group is represented by the *per capita* new business enterprise formation net of voluntary dissolutions. The institution of government is not represented by any political group but by the reciprocal of the *per capita* homicide rate; this variable describes the rate of voluntary conformity with the minimum conditions codified into criminal law for membership in good standing in the macrosociety. Except for the divorce rate, increases in the *per capita* rates for all the institutional variables represent increased consensus about their distinctive cultural value systems.

The noninstitutional variables identify factors that have been proposed in the sociological literature as relevant to macrosocial change. Demographic change is represented by the crude birth rate. Technological innovation is represented by the *per capita* number of patent applications for inventions. Macroeconomic business cycle conditions together with longer secular economic trends are represented by the *per capita* real income measured by the constant-dollar gross national product. Military

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mobilization during wartime is represented by the *per capita* number of armed forces active duty personnel. Mass communications media is represented by personal consumption expenditures *per capita* for newspapers, books, periodicals and cinema, plus business income to radio and television broadcasting firms. Ecological change is represented both for internal migration due to urbanization represented by the percent of the population living on farms, the most important internal migration at the time, and for international immigration represented by the *per capita* rate of foreign immigration.

The data are drawn from the U.S. Commerce Department's *Historical Statistics of the United States* (1976) and annual issues of *Statistical Abstract of the United States.* The historical time series are from 1920 through 1972. Firstly the data are either aggregated into four-year periods before *per capita* rates are calculated or are four-year averages of *per capita* rates calculated by the source. Then these four-year *per capita* rates are transformed into period-to-period change rates to enhance sensitivity of the equations and eliminate collinearity, and then the change ratios are transformed into index numbers having the last historical period, which is the first out-of-sample forecast period, as the base period. The quantitative theory is a functionalist theory not only in the sense that it is expressed in mathematical functions, but also in Merton's sense of functionalism, because it describes the interdependence of the types of institutional groups and the consequences of their interaction for the macrosociety as a whole as represented by the whole system of equations. The mathematical model is a recursive, first-degree, higher-order difference equation system.

This mathematically expressed macrosociological theory is used to examine social change in the U.S. national macrosociety with both static and dynamic analyses. Consider firstly the functionalist static analysis. The objective of a static analysis is to determine whether or not there is a stable equilibrium, that is, a solution in which the numeric value of each variable is the same for an indefinite number of successive time periods through which the model may be iterated. Since the numeric values are change rates of *per capita* rates, the mathematical equilibrium solution is one of constant change rates of the *per capita* rates for all of the variables. However these constant change rates may be positive, zero, or negative, and this is not necessarily a macrosocial equilibrium. The classical-functionalist-consensus equilibrium extended to the scope of the macrosociety is represented by constant *per capita* rates for all the institutional variables, and these *per capita* rates must be high if not actually near their maximum values to represent a high degree of consensus and macrosocial integration. Since constancy of such high *per*

capita rates implies zero growth of the change rates, nonexistence of an equilibrium solution consisting of zero change rates implies the nonexistence of a classical consensus stable equilibrium. As it happens, examination of the equilibrium solution of the model reveals that a mathematically consistent zero-growth solution for all the institutional variables does not exist, and therefore reveals that consensus equilibrium is not possible. In classical functionalist terms this means that the interinstitutional system of cultural value orientations of the American national society is inconsistent or "malintegrated."

Consider next the dynamic analyses, which consist of iterating the model to examine its response properties. The findings from three simulation experiments were described in the paper. In the first experiment the model is iterated with all its exogenous variables zero growth of their *per capita* rates. The iteration propagates a time path, which oscillates with increasing amplitude generates an intergenerational twenty-eight-year cycle. Examination of the structure of the model reveals that the equations determining the change rates of the *per capita* birth and marriage rates are interacting to create the cycle, and therefore that the cycle reflects changes in the national demographic profile, i.e. the age composition of the population. The explosive instability, however, is due to the constancy of the *per capita* real aggregate income variable occurring in the marriage rate equation, which represents fewer marriages in the Great Depression. The exogenous status of the economic sector means that there can be neither a dampening feedback on *per capita* real income from the exploding birth rates, nor any effect from productivity improvements resulting from technology improvements represented by the patents for inventions. Hickey maintains that any macrosociological model should be integrated into a model of economic growth, just as the contemporary Institutionalist economist will maintain that conventional neoclassical econometric models of economic growth should be integrated into a macrosociological model of institutional change.

In the second experiment and in all succeeding experiments this demographic cycle is eliminated by removing the birth rate equation from the model and by making the birth rate exogenously determined with an assigned constant zero-growth rate. When the model thus modified is iterated, it generates a damped oscillating path which is also intergenerational, and which converges into a stable equilibrium of constant growth rates for all its endogenous variables including the institutional variables. The negative feed back producing the dampening effect involves an interaction between the equations determining the homicide rate and the

high school graduation rate. A sustained decline in voluntary compliance with the minimum conditions for social order as codified into criminal law, i.e. an increase in the homicide rate, occasions in due course a corrective reaction that involves the socializing function of the educational institution.

However, the resulting growth equilibrium does not necessarily result in a movement toward consensus for all the types of institutional groups. Additional simulations reveal that only when the growth of the exogenous real *per capita* income is made to occur at a rate of no less than four and one-half percent compounded annually, does the equilibrium solution result in positive growth rates for all of the institutional variables excluding the divorce rate. This is the minimum annual rate of *per capita* real economic growth required for the dampening negative feed back to stabilize the national macrosociety in an equilibrium growth toward macrosocial consensus integration. It is also the minimum annual growth rate for a full-employment economy. But the static analysis revealed that this state of affairs can only be temporary, since the cultural value system is malintegrated. Furthermore, historically a four and one-half percent annual growth rate for real *per capita* income has not been sustainable by the U.S. macroeconomy, because it typically results in destabilizing inflation rates.

The third experiment consists of shock simulations in which the model is given an unrealistically large one-period increase after it is iterated sufficiently to settle into its equilibrium growth path. The limits imposed in reality by the *per capita* rates are disregarded in the simulations, to exhibit the dynamic properties of the model. In each simulation the shock consists of a permanent doubling of the *per capita* rate for one selected noninstitutional variable. In all but one case the shock propagates a damped oscillating path that settles back into a stable equilibrium solution. The exceptional case is the *per capita* urban residence rate, and the outcome of the shock is a nonoscillating explosive destabilization of the macrosociety. In this latter case the equations for both the birth rate and the urban residence rate have been removed from the model, such that the population growth cannot be accommodated by internal migration, and the macrosociety is disturbed beyond the stabilizing capacity of its interinstitutional integrative mechanisms.

In summary there are four findings from both the static and dynamic analyses. Firstly the interinstitutional cultural value system of the American national macrosociety is malintegrated, such that Parsonian macrosocial consensus equilibrium is not possible. Secondly if demographic cycles are exogenously determined, the national society exhibits a nonsustainable tendency to macrosocial consensus when *per capita* real income grows at a

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minimum rate of four and one-half percent compounded annually. Thirdly this tendency to consensus equilibrium at this economic growth rate stabilizes in growth equilibrium, because the interinstitutional cultural value system contains relationships that create intergenerational negative feed back integrative mechanisms, which involve a corrective reaction to criminal social disorder and operates through the socializing functions of the educational institution. Finally internal migration, an ecological adjustment to population growth, is necessary for the institutional integrative mechanisms to be effective. Such is a summary of Hickey's findings.

A few years later Hickey developed a larger model based on the above described macrosociometric model, which integrated the sociological and macroeconomic sectors of the nation into one model. Hickey's description of the model and findings from it were published with the title "The Indiana Economic Growth Model" in the periodical *Perspectives on the Indiana Economy* (March, 1985) published by the Indiana Department of Commerce. The model contained over one hundred equations and was instrumental to the economic development planning by the government of the state of Indiana during the eight years of the administration of Governor Robert Orr.

A Pragmatist Critique of Academic Sociology's *Weltanschauung*

This section consists of Hickey's criticism of the referee reviews and consequent decisions by the editors of four sociological journals to reject the paper. Contrary to these editors Hickey regards his paper as worthy of publication. His reports of the sociologists' attempts at scientific criticism are based on the correspondences from the editors, which Hickey has retained. This sample of seven referee criticisms from three academic sociology journals is not a random sample. It is a selected sample made by the journal editors, who presumably chose the critics they deemed best for the topic of the paper. And it is consistently representative of academic sociology's institutionalized values and German Romantic *Weltanschauung*. In this respect it is noteworthy that virtually none of these referee criticisms of Hickey's paper are empirical, but rather are attempted criticisms in philosophy of science. Hickey's basic rejoinders set forth herein are firstly that sociologists are technically inadequate to the Hickey's mathematical modeling, secondly that they are ignorant of the contemporary Pragmatist philosophy of science, and thirdly that they reveal obstructionism.

Consider the sociologists' technical inadequacies. Before constructing his national macrosociological theory with his **METAMODEL** discovery

system, Hickey undertook an extensive search of the academic sociological literature to determine what factors should govern his selection of the manifestly sociologically relevant time series data as inputs to the discovery system. He also wanted to find some example of the writing style used in sociology for reporting findings from such modeling analyses. In his literature search he could find no precedent for his dynamic macrosociometric model. Empirical work in sociology consists exclusively of survey research using written questionnaires and/or oral interviews. And the purpose of the surveys is to examine social-psychological hypotheses. Furthermore the survey research findings are summarized in tables, but are not analyzed with any statistically estimated models. One consequence of this condition is that any sociologist selected by an editor to be a critic could not reference any previously published equation that is empirically superior to any equations explaining the time series data used by Hickey.

A second consequence of the unprecedented character of Hickey's macrosociometric model is that it reveals that academic sociologists are not educationally prepared to work with higher-order difference equation systems, such as those constituting Hickey's model. Hickey's professional education is in economics, and since the publication in 1939 of "Interactions between the Multiplier Analysis and the Principles of Acceleration" in *Review of Economics and Statistics* by Nobel laureate economist Paul Samuelson, such difference equations have become a staple technique in mathematical economics and econometrics. And Hickey's use of the technique of shock simulations was introduced into economics in 1933 by the University of Oslo, Norway, economist Ragnar Frisch in his "Propagation Problems and Impulse Problems in Dynamic Economics" in Economic *Essays in Honor of Gustav Cassel*. Ironically Hickey's macrosociometric model is not a simultaneous-equation model, and any reasonably bright high school student could replicate Hickey's findings using Hickey's model with index numbers having any arbitrary but uniform base year and by iterating the model in a computer spreadsheet program. And any undergraduate who was sufficiently motivated to search back issues of the *U.S. Statistical Abstract* in a public library or a college library, could replicate the estimation of Hickey's model in a computer spread sheet's multiple regression routine. But these techniques are not taught in the curriculum of the Ph.D. sociologists. Thus the referees were suspicious and dismayed by the findings drawn from the simulation and shock analyses in Hickey's paper. The outcome was that the sociologists deemed by the editors to be sufficiently reputable as to be worthy to function as referees for his journal, showed themselves to be incompetent in the formal techniques

in Hickey's paper. And it may be added that the editors who rejected Hickey's paper gave no evidence that they are any less technically ignorant. Hickey comments that it may be human to reject what one does not understand, but it is not professional.

Consider next the sociologists' philosophical inadequacies. When people do not know what to do, they do what they know, whatever it may be; and what the critics of Hickey's paper know is a reductionist social-psychological Romanticism, which even today still requires *verstehen* criticism. The referees selected by the editors to whom Hickey sent his paper did not announce explicitly that they practice *verstehen* verification. But just as critics of papers in mature sciences do not announce that they practice empirical criticism, so too sociologists simply go about practicing criticism unreflectively according to the institutionalized value standards that they had learned in their educational experience and that are approved by their colleagues. These editor-selected sociologists used language that makes apparent their *verstehen* practice by the rhetoric and vocabulary in the stated reasons they set forth as criticisms. They criticized Hickey's equations because they do not "make sense", because they are "counterintuitive", "meaningless", "bizarre", "surprising", etc. thereby making apparent their practice of *verstehen* criticism.

Sociologists may use the *verstehen* for *a priori* criticism either before or after testing. It operates before testing when it controls discovery. The sociologist empathetically formulates on the basis of his own personal reality a hypothesis about the mental states that motivate the social actors' behavior that he may or may not intend to investigate by survey research, with the result that hypotheses that do not satisfy the *a priori verstehen* criterion are excluded from consideration for empirical testing. And *verstehen* operates after testing when the criticizing sociologist is confronted with a report of findings from another sociologist's empirical work, and when the report leads the criticizing sociologist to reject out of hand an unexpected but empirically valid finding with which he cannot empathize on the basis of his personal or vicarious experience.

Consider finally the consequent sociological obstructionism. Sociologists like to view themselves as the professional experts in all matters sociological. As experts they earn their livelihoods as academic professors of their subject in recognized universities, award the universities' credentials, and condescendingly deem all others who might discourse on the subject to be laymen and amateurs who lack the academic instruction that professional sociologists market. Therefore should the submitting layman employ mathematical and statistical techniques in which the

academic sociologists are incompetent, then the submission is viewed as an embarrassing expose of the professionals' inferiority. Consequently the submission of such a paper by an outsider like Hickey, a philosopher and econometrician, is not welcomed by the academic sociological journals. Were sociology a modern science with institutionalized empiricist value standards, these editors and their referees would have damaged their professional reputations by dismissing Hickey's paper. As it happens in the same year that Hickey began submitting his paper to these sociological journals, the editor of the *Journal of the American Society of Information Science* (Jan. 1978, Vol. 29, No. 1, p. 3.) stated in his "Editor's Notes" that referees sometimes use the peer review process as a means of attacking a point of view, and object to the content of a submitted paper. He said that often rather than rejecting a paper so treated, it would be better to publish the submitted paper with the reviewer's comments.

In his autobiography, *Work and Academic Politics* (2002), William H. Form who was the *American Sociological Review* editor to whom Hickey had submitted his paper, portrays academic sociology as a mediaeval guild and himself as a journeyman in the guild. A guild is a kind of trade association of craftsmen or merchants that flourished in Europe between the 11th and 16th centuries, and that was formed for the mutual aid and advancement of its members by monopolizing its trade. Based on the attempted criticisms by editor-selected sample of referees Hickey concludes that American academic sociology is much worse than a mediaeval guild operating in restraint of trade; he believes that the philosophy of science enforced in American academic sociology is so inbred that its information pool is as degenerate as the gene pool of an incestuous hereditary dynasty.

Sociological Methods and Research

The first academic sociological journal to which Hickey sent his paper was *Sociological Methods and Research* published by Sage Publications, Inc. This journal did not acknowledge receipt of the paper, but Hickey's retained U.S. Postal Service receipt documents that the paper was received on 18 December 1978, the date that Hickey uses to document his priority, although in fact his macrosociological model was actually created in the latter half of 1976. On 22 May 1979 Hickey received a letter rejecting the paper for publication from the editor, a Mr. George W. Bohrnstedt of Indiana University. With the letter were enclosed criticisms by two referees, both of whom offered a recitation of their Romanticist philosophy of science.

The first referee described Hickey's model as a reification of the worst type, and ridiculed Hickey's artificial-intelligence discovery system as a self-cooking program. This rhetoric is symptomatic of Romanticism and also suggests a Luddite mentality. This critic also described Hickey's model as value-based modeling, and said that it is inferior to a demographic accounting framework advocated by a sociologist named Kenneth C. Land. Land had proposed a modeling approach in his "A General Framework for Building Dynamic Social Indicator Models: Including an Analysis of Changes in Crime Rates and Police Expenditures" in *American Journal of Sociology* (1976). Land's ideas have their origin in a 1971 technical report, *Demographic Accounting and Model Building*, written by a Professor Richard Stone, and published by the Organization for Economic Cooperation and Development. Conceptually the demographic accounting system is analogous to a perpetual inventory accounting system as might be found in a retailing business; it has beginning and ending inventory stocks, and inflows and outflows explaining the changes in stocks over an accounting period. In the demographic system the stock variables represent population head counts with the inflows due to births or immigration and the outflows due to deaths and emigration. Stone also describes how the accounting system may be expressed as a matrix with the inflows and outflows treated analogously to the economist's input-output models with the rows representing inflows, the columns representing outflows, and the cells representing transition coefficients calculated by dividing the aggregates in each row by its row total. Since these transition coefficients will change from period to period, there is an additional problem of projecting these changes if the model is to be used for any kind of policy analysis. Land's paper proposed using the econometric type of models statistically estimated over the time series of transition coefficients, which he furthermore says can be interpreted as measures of opportunities for social benefits. He therefore calls this the opportunity-structure approach, which he says is based on ideas originally proposed by the sociologist William F. Ogburn. Land's approach seems interesting and might be fruitful, if and when it is ever carried out. However the equation set forth in his paper is not estimated over vectors of transition coefficients from any demographic model. In any event it is not clearly an alternative to Hickey's, since his value-based approach might be used to model the changes in the transition coefficients. But Hickey is not indebted to this approach, and he was unwilling to be conscripted to the support of this agenda as a condition for publication. In fact he referenced it in future versions of his paper to distinguish his work. The second referee

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selected by Bohrnstedt started his criticism by saying that he can't quite figure out whether or not the paper is a "put on”.

Hickey decided that *Sociological Methods and Research* is not a suitable publication for his paper, because he concluded that the model is beyond the competence of the editor and his selected referees, and he did not send the editor any replies. He did not know at the time that referees for other sociological journals would offer even more dogmatically Romantic criticisms. Nor did he know at the time that Bohrnstedt is philosophically opposed to the contemporary Pragmatist philosophy of science until Bohrnstedt, Knoke and (later) Mee later published an undergraduate textbook titled *Statistics for Social Data Analysis*, which virtually echoed the philosophy of science expressed in Bohrnstedt's selected referees. The authors would limit modeling to a testing role, and advocate a version of Haavelmo's structural-equation agenda with its romantic ontology. Like Haavelmo they distinguish unobserved conceptual variables and observable indicators, a gratuitously equivocating semantical dualism. And they propose criteria for identifying causality prior to statistical modeling and empirical testing.

American Journal of Sociology

The second sociological journal to which Hickey sent his paper is the *American Journal of Sociology*, (AJS) which was edited at the time by a Mr. Edward O. Laumann at the University of Chicago. The journal acknowledged receipt of Hickey's paper on 19 October 1979, and on 21 November 1979 Hickey received a rejection letter from the editor together with statements of reasons for rejection written by two referees.

These criticisms were even more Romantic than those from *Sociological Methods and Research*. The first referee rejected Land's opportunity-structure concept saying it is a clumsy abstraction that is too vague to illuminate anything about social change that is not obvious. And then obviousness notwithstanding he says in the next paragraph that he is simply not convinced about anything reported in the paper, and that the outcome is bizarre. He demanded substantively informed investigations that specify "concrete behavioral mechanisms." This is an argot for a Romantic sociology that is a social psychology. He also claimed that there is a "burgeoning" literature reporting analyses of the various influences of trends in factors and speculated about factors that he thinks may be useful for modeling, but he offered no citations. Hickey replied that the critic should

do his own modeling and should not attempt to make Hickey or any other author his research assistant as a condition for publication.

Laumann's second referee dismissed Hickey's interpretation of the *per capita* rates as theoretical hocus-. Hickey's used of *per capita* rates of voluntary associational behavior relative to institutional groups to reflect degrees of consensus about the respective institutional cultural values. He believes that there may be an analogy between his treatment of cultural values and Nobel laureate economist Paul Samuelson's treatment of economic values in the latter's doctrine of revealed preference set forth in "A Note on the Pure Theory of Consumer's Behavior" (1938) and in "Consumption Theory in Terms of Revealed Preference" (1948). Both papers are reprinted in *The Collected Papers of Paul A. Samuelson* (1966, Vol. 1). Samuelson rejected the Austrian school's Romantic concept of utility as an introspectively known psychological experience of economic value, and instead describes consumer demand in terms of observed market behavior revealing consumer preference patterns. And just as a commodity's per unit price measures economic value in the publicly observable market transaction even though the price does not characterize the economic value except in association with the identified group product, so too the *per capita* rate measures institutional value in the publicly observable group associational behavior even though the *per capita* rate does not characterize the social value except in association with the identified group. Another analogy that is more familiar is a political election outcome: a landslide election outcome is a measure of a high degree of political consensus, even though the election returns do not characterize the mandate that the winning candidate brings to public office. This critic also attacked Hickey's use of his discovery system, and he said that the computer program cannot replace the complexity of a scientist's intuition. Another Luddite sociologist! Hickey replied that the manner in which a theory is created is irrelevant to its empirical validity, and referenced the early Pragmatist philosopher, William James, who tersely said of worthy scientific theories: "By their fruits ye shall know them, and not by their roots."

Hickey submitted his rebuttals to Laumann, and on 30 July 1980 he received another rejection letter with a brief criticism from a third referee enclosed. The third critic identified as an internal reviewer, indicated that he had read the criticisms written by the two other referees and the rebuttals submitted by Hickey. In his own criticism this third critic very briefly summarized the other referees' objections, and then maintained that Hickey does not understand the fundamental objection to the paper: the need for specific mechanisms.

The Chicago University web site identifies Laumann as a 1964 Ph.D. sociology graduate of Harvard University, which at the time was under the influence of Parsonian Romanticism. Laumann is no less a Romantic than his selected referees, and Hickey believes was probably one of them. In his *Sexual Organization of the City* (2004) based on a local Chicago survey, and in his *Sex, Love, and Health in America* (2001), *Social Organization of Sexuality* (1994), and *Sex in America* (1994) all based on a larger national survey, Laumann seeks interpretative understanding of his survey respondents' sexual attitudes and behaviors. His romantic approach is most explicitly set forth in the chapter titled "Normative Orientations toward Sexuality" in his *Social Organization of Sexuality*. Laumann's chairmanship of the Chicago University's Sociology Department was preceded by that of William F. Ogburn, who was a Positivist. During Ogburn's tenure there were vigorous methodological debates in the university's sociology department. Ogburn rejected the Romantic concept of sociology, and maintained that any discipline that does not imitate the methods of the physical sciences is not truly a science. He advocated quantitative analysis in social science, demanded that the language of sociological explanation refer only to observables features of human behavior, and rejected the Romantics' reference to subjective experience. The Romantics won in these methodology debates, and Laumann's views are on the winning side. He became chairman of the department and editor of the journal. His selection of referees and rejection of Hickey's paper suggests he is no more sympathetic to contemporary Pragmatism than to Positivism.

American Sociological Review

The third academic sociological journal to which Hickey sent his paper was *American Sociological Review,* (ASR) the journal of the American Sociological Association (ASA), which was edited by a Mr. William H. Form at the University of Illinois, Urbana. Form acknowledged receipt of Hickey's paper on 13 March 1981. On 10 April Hickey received a rejection letter signed by Form with two referee criticisms enclosed. The first referee criticism was typical of the others. He focused on the idea of theory, which he distinguishes from the idea of model. He purports that Hickey's model needs a theory underlying the causal assertions embodied in the equations, and says that while Hickey's model satisfies statistical criteria, it does not make substantive sense. All this is from the Romantic argot. He said that Hickey had distorted the methodological views of Kenneth Land, which are set forth in Land's "Formal Theory" in *Sociological Methodology:*

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1971, a book bearing an *imprimatur* identifying it as an official publication of the American Sociological Association. In this paper Land distinguishes theories from models, and proposes relating them with a logical schema advanced for the natural sciences by the Logical Positivist philosopher Carl Hempel. This philosophical eclecticism by Land is possible, because both Romantics and Positivists distinguish theory and observation language semantically, even though they reverse the role of semantics: the Positivists such as Hempel believe that theory is meaningless unless it is logically related to observation language, while the Romantics such as this critic believe that empirical models are meaningless unless they are related to mentalistic theory. He also says that the equations are puzzling, that demographers will be amazed, and that criminologists will be surprised. These objections are not only irrelevant but also dubious; for example examination of the U.S. Federal Bureau of Investigation's *Uniform Crime Statistics* reveals that the factors in Hickey's equation would not be surprising to the criminologist. More importantly making familiarity a criterion for scientific criticism results in stagnation, and sociology has been stagnated by its Romantic philosophy of science for decades.

The second referee selected by Form wrote an arrogantly dismissive critique consisting of only six sentences, a cynical caricature of criticism. He wrote that the metatheoretical considerations in the paper do not motivate the actual analyses effectively, that little useful theory is involved, that the particular analyses are similarly little motivated, and that none of them reflect the usually long traditions of research attempting to explain the variables involved. He also calls the paper an empiricist venture that is utterly ineffectively related to the empirical traditions involved in explaining the equations. He then attempts criticism about correlations and trends that reveals his inexperience with data analysis, and he concludes that these correlations should not without much more thought be made a basis for causal inference. Ironically these reasons for rejection actually would be reasons for acceptance in a mature and productive scientific profession. Deviation from tradition is never a reason for rejection of a paper, and the critic's description of Hickey's paper as an empiricist venture reveals that irrelevant criteria are actually operative, because science is fundamentally nothing other than an empiricist venture. Hickey calls this phobic anti-empiricism the "sociologists' disease". Furthermore the critic's demanding much more thought is irresponsibly uninformative.

In view of such rejection Hickey is not surprised to have discovered later that Form has his own alternative approach to the subject of Hickey's paper, which involves no modeling. Form described his own approach in his

"Institutional Analysis: An Organizational Approach" (1990), which he also summarized later in his autobiography, *Work and Academic Politics*. His organizational approach was his style of sociology long before he received Hickey's submission. In the 1990 article Form references his earlier *Industry, Labor and Community* (1960) as an illustration of his organizational approach, and it is also recognizably similar to that found in the last chapters, "Industry and the Community" and "Industry and Society" in his *Industrial Sociology* (1951). "Institutional Analysis: An Organizational Approach" is not a report of new empirical findings but rather is a proposal, which to date has not advanced beyond the status of a programmatic agenda. Form defines an institution as a number of interacting organizations (complexes) whose boundaries are measured by the extent of their contacts. He says that norms, rules, regulations, and laws are regularities that emerge from these interactions, and that institutional analysis explains how these regularities emerge, function, and change. Therefore he says his organizational approach to institutional analysis requires that norms, values, ends, and related concepts should not be used as independent variables in institutional analysis, that he is reversing the traditional approach, and that his procedure avoids a circularity where they are asserted to exist and then used to explain behavior. Form's rejection of circularity suggests that he had no exposure to simultaneous-equation models. Form uses the term independent variables, but he does not propose any modeling, much less actually do it. And he offers no empirical basis for his excluding institutional values from the role of independent variables.

In his autobiography Form wrote that unlike his predecessor editors he read every manuscript submitted to the ASR, and wrote his own internal review for every submission. After viewing Form's work style Hickey believes that upon reading Hickey's submission Form found the paper's alternative technical approach unappealing if not actually threatening to his own discursive organizational approach, because Hickey has found no evidence that Form possesses the technical skills for competing with the modeling and simulation approach in Hickey's paper. In retrospect it is not surprising to Hickey that Form rejected the paper.

Hickey submitted his rebuttals to Form on 6 May 1981. In his reply Hickey referred Form to an elementary textbook in econometrics, and also enclosed a brief annotated bibliography of the relevant philosophy of science literature for the edification of Form and his selected critics. Hickey promptly received a drop-dead letter in reply, in which Form told Hickey that apparently Hickey does not understand the folkways of his profession, that it is not normative for an article to be resubmitted once it is rejected, and

that if this were not the practice, Form would spend his life re-reviewing the same manuscript. Ironically even in 1981 this was probably true of Hickey's paper even disregarding Form's advanced age (he was born in 1917). Even today the typical Ph.D. sociologist lacks the technical education for performing the construction and simulation techniques used by Hickey in his first-degree higher-order difference-equation empirical model much less use artificial intelligence.

Comments

These seven *Sturm und Drang*-style criticisms consistently reveal the domination of the German Romantic philosophy of science in American academic sociology making it more of a humanities literature than an empirical science. Is this consequential? The practice of an anachronistic philosophy of science yields a retarded science, and academic sociology is a backward science *in extremis*, if it can even be called a science instead of a humanities literature. In "Sociology's Long Decade in the Wilderness" the *New York Times* (28 May 1989) reports that universities such as the University of Rochester in New York and Washington University in St. Louis, Missouri, have disbanded their sociology departments, and that the National Science Foundation has drastically cut back funding for sociological research. A graphic display in the article indicates that since the mid-1970's the number of bachelors degrees awarded with majors in sociology has declined by nearly eighty percent, the number of sociology masters degrees by sixty percent, and the number of sociology doctorate degrees by forty percent. Demand for Ph.D. degrees is influenced by many factors not specific to sociology, such as cyclical and secular changes in economic conditions, and changes in population size and demographic profile. But the effects of these and other extraneous factors can be filtered by relating the number of sociology doctorates to the number of doctorates in other fields. Data for earned doctorates in various sciences are available from the United States Department of Education, Office of Educational Research and Improvement. Time series plots of the percent of earned doctorates in sociology both relative to the number of earned doctorates in economics and relative to the number of earned doctorates in physics corroborate the reported decline of academic sociology, and validate the accuracy of Mr. Joseph Berger's reporting for the *New York Times*.

Berger's article also quotes a Mr. Stephen Buff, identified in the article as the assistant executive director of the American Sociological Association, as saying that sociology suffers from not being well defined in

the public mind, and that sociology is confused either with social work or with socialism. But contrary to Mr. Buff's explanation public opinion is not operative in these decisions made against academic sociology. Decisions to enroll or not to enroll in graduate schools of sociology are made by students with undergraduate majors in sociology; decisions to support or close sociology graduate schools are made by knowledgeable university administrators; and the funding decisions of the National Science Foundation are made by staff members who are among the best informed in the nation. The cause of these unfavorable decisions originates within academic sociology; it does not lie with an ignorant general public. The article also quotes a more realistic opinion by a professor Egon Mayer, a Brooklyn College sociologist, who said that sociologists are still teaching the same sort of thing that they taught in the 1960's and 1970's, but are not as convinced now that it is worth teaching, and are not quite sure what it should be replaced with. In Hickey's view sociologists will never know what is worth teaching, until they discard their Romantic dogmatism and adopt the contemporary Pragmatist philosophy of science.

These reports are not encouraging to any young man or woman with the option of graduate-level studies in pursuit of a career in academic sociology. As a Ph.D. graduate in sociology he (or she, of course) would find that there is little demand for what he has to teach, and may expect that he might have to pursue another occupation to earn a living. And were he lucky enough to find employment on the faculty of a university that still has a sociology department, but formulated a view that is contrary to the dominant Romanticism, he would find that he cannot get published in the academic literature. His submitted paper would be rejected for reasons that are a caricature of empirical criticism by an editor who cannot distinguish contrary evidence from the contrary opinions expressed by his selected referees. And were the submitting sociologist so audacious as to presume to submit rebuttals to the comments of the pontificating referees, he may find himself reading a drop-dead letter from the editor saying that apparently he does not understand the folkways of the profession, that it is not normative for an article to be resubmitted once it is rejected, and that if this were not the practice the editor would spend his life time re-reviewing the same manuscript.

The effect of sociology's Romantic dogmatism is not limited to academia. It includes the profession's demonstrated impotence to serve as a guide for the formulation of effective social policy. The same *New York Times* article also cites disillusionment resulting from the failures of the Great Society programs of the 1960s', and reports that sociologists today

find they have lost Federal funding, must scale down their projects, forsake new inquiries, and disguise their work as anything-but-sociology. Similarly in his *Limits of Social Policy* (1988) Nathan Glazer, Harvard University professor of sociology and formerly an urban sociologist in the Federal Housing and Home Finance Agency during the Administration of John F. Kennedy, writes that the optimistic vision of sociology guiding policy by use of its knowledge of the fine structure of society of how policy impinges on family, neighborhood, and community has faded considerably. Glazer observes that in trying to deal with the breakdown of traditional structures, particularly the family, social policies have weakened them further and have made matters worse. He cites as one noteworthy example the welfare system, which he says undergoes continual expansion and correction with input from social scientists, but which nonetheless damages the family, encourages family breakup, and encourages fathers to abandon children, even though many of the changes in the system were designed to overcome just these untoward effects. He notes that these outcomes have occasioned the rejection of social engineering, which he describes as the capacity of human foresight using subtly graduated incentives and disincentives and sharply focused programs, to affect human behavior and to improve the human condition. Glazer maintains that the most significant limitation of the effectiveness of the social policies formulated and implemented in the 1960's is lack of knowledge.

However, sociology's failure in the crucible of real-world social policy is not due merely to a lack of knowledge that could be remedied by more research in conformity with the Romantic philosophy of science. Sociologists will never understand these symptoms of failure, until they recognize the pathogen infecting their professional culture: the Romantic dogmatism that operates in their criteria for scientific criticism and that imposes *a priori* restrictions on their theorizing. As it happens the eighth chapter of Glazer's book "'Superstitions' and Social Policy" could well be taken as an expose of sociologists' failure to recognize latent functions, and it amounts to a vindication of Merton's theorem of social engineering. As long as academic sociologists accept only theories that reduce to a motivational social psychology, much less to Romantic theories that "make sense" in compliance with the *verstehen* criterion; as long as they reject Romantically inexplicable latent functions and suppress publication of empirically superior theories that seem surprising or bizarre relative to the sociologist's *verstehen*; and most importantly as long as contemporary Pragmatism remains a *terra incognita* to the sociologists - sociologists will continue to be incapable of contributing to effective social policy, much less

establishing their profession as a well functioning and modern empirical science instead of a philosophically retrograde academic occupation.

Twentieth-century physics too had its failures, but when physicists formulated the relativity and quantum theories to remedy these failures, they found that they had to unbundle the ontological commitments which they had conventionally bundled together with the empirical criteria for scientific criticism, and then they had to make a decision about which type of criteria would operate as rules of evidence. Their acceptance of the startlingly counterintuitive but empirically superior relativity and quantum theories led them to discard all ontological criteria including but not limited to those which they recognized as held over from the Newtonian physics, even though the Newtonian ontology had come to define what constitutes causal explanation and what "makes sense" in physics. Had the physicists dogmatically adhered to the old Newtonian ontology, neither relativity theory nor quantum theory could have been accepted. Instead physicists accepted theories exclusively on the basis of their empirical test outcomes. This is Pragmatism as it evolved in the practice of basic research in the Galileo-Einstein-Heisenberg tradition, and then as it was later articulated firstly by the physicists and then more systematically by the contemporary Pragmatist philosophers of science. The optimism of the Great Society social programs to which Glazer referred, has long ago passed into history, even as sociologists continue to bundle their ontological commitments to Romanticism into their criteria for scientific criticism. Glazer's use of the term optimism in his *Limits of Social Policy* is an understatement; today only a government of incorrigibly naive Candides would again entrust the philosophically naive sociologists with a guiding role in the formulation of social policy. Before these Panglossian professors of sociology can restore their credibility with real-world social policy administrators, they must overcome their anachronistic Romanticism and accept the contemporary Pragmatism, which rejects *a priori* commitment to any ontology as a criterion for scientific criticism.

Social Indicators Research

There was one more sociological journal to which Hickey had sent his paper, which cannot be treated as the others discussed above, because the editor refused to inform Hickey of the scientific criticisms given as reasons for rejection. This journal is *Social Indicators Research* edited by a Mr. Alex C. Michalos. Michalos is identified on the journal's stationery as Director of the Social Indicators Research Programme at the University of Geulph in

Ontario, Canada, and the publisher is identified as D. Reidel Publishing Company, Dordrecht, Holland, and Boston, U.S.A. Michalos acknowledged his receipt of Hickey's manuscript in a letter dated 19 January 1982. In a letter to Hickey dated 4 February 1982 Michalos said that he had received a very unfavorable review of the manuscript and would not be able to publish it. He added that usually he has specific comments from a reviewer to send to authors, but that in this case the reviewer pretty well threw up his hands. In response to a letter from Hickey dated 12 February demanding two written referee comments, Michalos wrote a letter to Hickey dated 22 February 1982 replying that sometimes his reviewers are brutal, and that when the first reviewer is exceptionally critical, as in the case of Hickey's manuscript, he does not go to a second reviewer. He concluded by saying that he had sent Hickey all he had. In Hickey's view no critic is above criticism, and he wonders what enabled this single referee to exercise such a controlling influence over this editor. The most recent edition of the *National Faculty Directory* lists Michalos as a faculty member of the Department of Philosophy at the University of Guelph. Having a professional philosopher as editor of a sociological journal might have been a singularly fortunate circumstance both for *Social Indicators Research* and for academic sociology. Instead what Hickey actually encountered is an editorial practice that resembles the comparably absurd judicial practice portrayed in Franz Kafka's *The Trial*, in which the accused is arrested, tried, condemned and executed without ever having been informed of the charges brought against him. Hickey has no idea what Michalos actually teaches his philosophy students in the Department of Philosophy at the University of Geulph. But Hickey believes that both the students in Michalos' philosophy classroom and readers of his journal would be much better served, were Michalos to accept the decidedly non-Kafkaesque contemporary Pragmatist philosophy of science, and both teach contemporary philosophy of science in his classroom and implement it in his editorial decisions.

Conclusion

Hickey's submission to the four sociology journals was not intentionally a Trojan horse. But for all editors working for journals serving pseudoscientific professions like American academic sociology, this author offers a paraphrase of Homer's rendering of the advice belatedly issued by the unfortunate Trojans: "Beware of philosophers bearing contributions." Perhaps some day an indignant and principled academic sociologist will be inspired to establish a sociology journal of rejected papers, which would

publish not only the papers rejected by the orthodox sociological journals but also the shrill and strident reasons for rejection sent to the rejected authors together with the author's replies. Science after all is inherently public, and the reasons for rejection written by referees are attempts at scientific criticism, even if they are disreputably incompetent attempts. Such a practice of expose would introduce a badly needed sense of responsibility into the published literature of this backwater academic occupation by making the editors publicly answerable for the incompetent decisions they would otherwise be able to hide like incompetent physicians who believe they can bury their fatal mistakes. One beneficial outcome would be a high turnover of the incompetents - firstly the referees and then eventually the editors who selected these referees and accepted their opinions.

But the greatest threat to the sociology journals' attempted suppression of information is the Internet. The journals' gate guards can no longer protect the careers of ensconced academicians from new ideas. The Internet will have the same effect on sociology's publication censorship that it has had on tyrants' political censorship. Now contributors can circumvent the obstructionism. Disingenuous lip service to academic freedom will be replaced by the Internet's effective and unrestricted freedom to distribute and access new ideas and contributions. American academic sociology has a long long long way to travel before it can honestly claim to have evolved into a modern empirical science, but the information highway may speed this eventual development.

The "Last Sociologist"

In March 2001 Lawrence Summers, formerly Treasury Secretary under President Clinton and a Harvard Ph.D. graduate in economics who received tenure at Harvard at the remarkably young age of twenty-eight years, was appointed Harvard University's twenty-seventh president. His has not been a caretaker administration; in his first year his changes occasioned no little controversy. In "Roiling His Faculty, New Harvard President Reroutes Tenure Track" the *Wall Street Journal* (11 Jan. 2002) reported that Summers has attempted to make tenure accessible to younger faculty members and to avoid extinct volcanoes, those graybeard professors who receive tenure due to past accomplishments, but whose productive years are behind them. The threatening implications of Summers' administrative changes for Harvard's social science departments including sociology have

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not been overlooked. One critical faculty member is quoted by the *Wall Street Journal* as saying that a prejudice for younger over older candidates amounts to a prejudice for mathematical and statistical approaches - such as those reflected by Summers' own area of economics - over historical or philosophical approaches. Thus it appears that American academic social scientists are finally - in the twenty-first century - being dragged out of their murky misty Romanticism albeit kicking and screaming, but not without rear-guard resistance.

Another example of such resistance is a *New York Times* OP-ED-page article (19 May 2002) titled "The Last Sociologist" by Harvard sociology professor Orlando Patterson. Essentially Patterson's article is a defense of the Romantic dualism between the natural and social sciences with its doctrine that sociology is the interpretative understanding of culture. He complains that in their anxiety to achieve the status of economics, contemporary sociologists have adopted a style of scholarship that mimics the methodology and language of the natural sciences, which he describes as a style that focuses on building models, formulating laws, and testing hypotheses based on data generated by measurement. He claims that for most areas of social life - especially those areas in which the general public is interested - the methods of natural science are not only inappropriate but are also distorting. Patterson illustrates the kind of scholarship he approves for sociology by referencing such books as *The Lonely Crowd* by David Riesman, Patterson's mentor whom he describes as discarded and forgotten by his discipline of sociology, and *The Sociology of Everyday Life* by Erving Goffman, a Reisman contemporary. Patterson writes that these authors followed in an earlier tradition, and that their style of sociology was driven firstly by the significance of the subject and secondly by an epistemological emphasis on understanding the nature and meaning of social behavior. He says that this understanding is of a type that can only emerge from the interplay of the author's own views with those of the people being studied. Patterson laments that today sociologists eschew any explanation of human values, meanings, and beliefs due to ambiguities and judgment. He says that sociologists writing today about culture disdain as reactionary any attempt to demonstrate how culture explains behavior, while their models emphasize the organizational aspects of culture, with the result that little or nothing is learned from sociology about literature, art, music, or religion even by those who purport to study these areas.

Patterson's complaints notwithstanding sociology is becoming an empirical social science capable of making predictions with quantitative models like the econometric models of empirical economics. Society needs

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and wants an empirical science of society that enables forecasting and policy, and this achievement requires subordinating the Romantic mentalistic criteria to the Pragmatic empirical criteria. American academic sociology might finally graduate to the status of an empirical science were Patterson actually the last Romantic sociologist. But Patterson's OP-ED comments notwithstanding he is not the "last sociologist" meaning the last Romantic sociologist of culture. American academic sociology has a long, long, long road ahead of it before it graduates to the status of a modern empirical science. Examination of recently published articles in the four journals, to which Hickey had sent his macro-sociometric model twenty-five years ago, reveals that editors and referees are still Romantics. There now appear a few statistical models, but the authors are still required to supplement their statistical models with descriptions of motivations that supply the required understanding, so they "make sense."

Nonetheless changes at Harvard are in progress thanks in no small part to inexorable attrition. The *Wall Street Journal* article reported that Summers' hiring policies have the support of Harvard's governing board, and that hiring is an area that could prove to be his most enduring legacy. And given that Harvard is the cradle of both the classical and contemporary variations of Pragmatism, Summers' influence augers well for academic sociology at Harvard. Then eventually the professors and practitioners of sociology, the science of conformism, will follow Harvard's lead, just as they did when Parsons was the Pied Piper from Harvard.

American academic sociology is still a philosophically retrograde prescientific academic profession. But happily not every American academic sociologist is a philosophical simian that drags his knuckles as he walks. Immediately below the reader will find a description of a computerized artificial-intelligence discovery system developed by an atypically *avant garde* sociologist, John A. Sonquist, who not surprisingly has never had any paper appear in any academic sociology journal. Read on.

Sonquist on Simulating the Research Analyst with AID

John A. Sonquist (1931-   ) received a doctorate in sociology from the University of Chicago, and is at this writing a professor of sociology and the Director of the Sociology Computing Facility at the University of California at Santa Barbara, California. He was previously on the faculty at the University of Michigan at Ann Arbor, and was Research Associate and Head of the Computer Services Facility for the University's Institute for Social

Research. He is also a past chairman of the Association for Computing Machinery. For his Ph.D. dissertation written in 1963 at the University of Chicago he developed a computerized discovery system called the **AID** system. "AID" is an acronym for "Automated Interaction Detector" system. Today description of the **AID** system can be found in many marketing research textbooks in chapters discussing data analysis techniques for hypothesis development. The system is also used extensively by lending institutions for risk analysis. The **AID** system performs a type of statistical analysis often called "segmentation modeling", and in Sonquist's system, which is described in his *Multivariate Model Building* (1970) and elsewhere, the analysis uses a well known statistical segmentation method called "one-way analysis of variance." A variation on **AID** has been developed by Jay Magidson of Statistical Innovations, Inc., Belmont, MA, which is based on the equally well known segmentation method called "chi-squared analysis." The system is called **CHAID** (Chi-squared Automatic Interaction Detector), and is now commercially available in the **SPSS** computer statistical software package. And a version also exists in the SAS system called **SY-CHAID**.

In the "Preface" to his 1970 book Sonquist says that his interest in such a system started with a conversation with Professor James Morgan, in which the question was asked whether a computer could ever replace the research analyst himself, as well as replacing many of his statistical clerks. He writes that they discarded as irrelevant the issue of whether or not a computer can "think", and instead explored the question of whether or not the computer might simply be programmed to make some of the decisions ordinarily made by the scientist in the course of handling a typical analysis problem, as well as doing the computations. Developing such a computer program required firstly examining the research analyst's decision points, his alternative courses of action, and his logic for choosing one rather than the other course, and then secondly formalizing the decision-making procedure and programming it but with the capacity to handle many variables instead of only a few. An early statement of this idea was published in Sonquist's "Simulating the Research Analyst" in *Social Science Information* (1967). In this earlier work Sonquist distinguishes three kinds of computer applications in social science: data processing, simulation, and information retrieval. He observes that data processing systems and many information retrieval systems are nothing but an extension of the analyst's pencil and lack really complex logical capabilities. But he adds that there also exist information retrieval systems which are much more sophisticated, because simulating the human being retrieving information is one of the objectives of the system designer. These sophisticated retrieval applications combine both a

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considerable data processing capability and a logic for problem solving, such that the whole system is oriented toward the solution of a specific class of problems without human intervention in a long chain of decisions.

Sonquist then argues that such a combination of capabilities need not be limited to information retrieval, and that major benefits can be gained from the construction of a new type of simulation program, one in which the phenomenon simulated is the research analyst attempting to "make sense" out of his data. The phrase "make sense", which is a characteristic locution of the *verstehen* Romantics, is placed in quotation marks by Sonquist, and there is no evidence that he is advocating the *verstehen* philosophy of scientific criticism. In fact on the *verstehen* view a computer cannot "make sense" of social data, because it is not human and therefore cannot empathize with the human social actors. He says instead that an important function of the research analyst in the social sciences is the construction of models which fit the observed data at least reasonably well, and that this approach to the analysis of data can be likened to curve fitting rather than to the testing of clearly stated hypotheses deduced from precise mathematical formulations. He offers his own **AID** system as an example of a system that simulates the research analyst.

Sonquist and Morgan initially published their idea in their "Problems in the Analysis of Survey Data, and a Proposal" in *Journal of the American Statistical Association* (June, 1963). The authors examine a number of problems in survey research analysis of the joint effects of explanatory factors on a dependent variable, and they maintain that reasonably adequate techniques have been developed for handling most of them except the problem of interaction. Interaction is the existence of an intercorrelating influence among two or more variables that explain a dependent variable, such that the effects on the dependent variable are not independent and additive. This is contrary to the situation that is assumed by the use of other multivariate techniques, such as multiple classification analysis and multiple linear regression. In multiple regression each variable associated with an estimated coefficient is assumed to be independent, so that the effects of each variable on the dependent variable can be treated as additive. In "Finding Variables That Work" in *Public Opinion Quarterly* (Spring, 1969) Sonquist notes that it is possible to represent interaction among explanatory variables in a regression, if the interacting variables are combined multiplicatively prior to statistical estimation. But there still remains the prior problem of discovering the interacting variables. A triangular correlation matrix of a factor analysis can do this. Another is the **AID** discovery system, which may be used in conjunction with such techniques as

regression or multiple classification, in order to detect and identify interaction effects and to assist equation specification for regression. The **AID** system also resembles an earlier statistical technique called "cluster analysis", because it too combines and segments the observations into groups. But the **AID** system differs in that it is a segmentation analysis procedure that uses a dependent variable as a criterion for forming the segments, and therefore the segments are derived to predict a dependent variable. Furthermore clusters generally are not defined as explicit functions of the predictors, and so cannot easily be used to classify a new sample into clusters.

In *The Detection of Interaction Effects: A Report on a Computer Program for the Optimal Combinations of Explanatory Variables* (1964, 1970) and in *Searching for Structure: An Approach to Analysis of Substantial Bodies of MicroData and Documentation for a Computer Program* (1971, 1973) Sonquist and Morgan describe their algorithm, as it is implemented in the **AID** computer program used at the University of Michigan, Survey Research Center. The program answers the question: what dichotomous split on which single predictor variable will render the maximum improvement in the ability to predict values of the dependent variable. The program divides a sample of at least one thousand observations through a series of binary splits into a mutually exclusive series of subgroups. Each observation is a member of exactly one of these subgroups. The subgroups are chosen so that at each step in the procedure, the arithmetic means of each subgroup account for more of the total sum of squares (i.e. reduce the predictive error more) than the means of any other pair of subgroups. This is achieved by maximizing a statistic called the "between-group sum of squares." The procedure is iterative and terminates when further splitting into subgroups is unproductive.

The authors illustrate the algorithm with a tree diagram displaying the binary splits for an analysis of income using data categories representing age, race, education, occupation, and length in present job. When the total sample is examined, the minimum reduction in the unexplained sum of squares is obtained by splitting the sample into two new groups: persons under sixty-five years of age and persons aged sixty-five and over. Both of these groups may contain some nonwhites and varying degrees of education and occupation groups. The group that is sixty-five and over is not further divided, because control parameters in the system detect that the number of members in the group is too small. It is therefore a final grouping. The other group is further subdivided by race into white and nonwhite persons. The nonwhite group is not further subdivided, because it is too small, but the

system further subdivides the white group into persons with college education and persons without college education. Each of these latter is further subdivided. The college-educated group is split by age into those under forty-five years and those between forty-six and sixty-five. Neither of these subgroups is further subdivided. Those with no college are further subdivided into laborers and nonlaborers, and the latter are still further split by age into those under thirty five and those between thirty six and sixty five. The variable representing length of time in current job is not selected, because at each step there existed another variable which was more useful in explaining the variance remaining in that particular group. The predicted value of an individual's income is the mean value of the income of the final group of which the individual is a member. Such in overview is the **AID** discovery system.

Sonquist offers little by way of philosophical commentary; unlike sociologists such as Parsons and Lundberg, he does not develop a philosophy of science much less a philosophy of language. But there is little imperative that he philosophizes, since the application of his **AID** system is less often philosophically controversial. In his applications there is typically no conflict between the data inputted to his system and the mentalistic ontology required by Romantic sociologists, when his system is used to process data collected by survey research consisting of verbal responses revealing respondents' mental states. In such applications a conflict occurs only with those extreme Romanticists requiring the *verstehen* truth criterion for scientific criticism. In his 1963 paper, "Problems in the Analysis of Survey Data", Sonquist considers the problem that occurs when theoretical constructs are not the same as the factors that the sociologist is able to measure, even when the survey questions are attitudinal or expectational questions, and when the measurements that the sociologist actually uses, often called "proxy variables" or "indicators", are not related to the theoretical constructs on a simple one-to-one basis. This is a problem that occurs only in cases in which a theory pre-exists empirical analysis, and in this circumstance Sonquist advocates a role for the **AID** system, in which the system's empirical analyses are used for the resolution of problems involving interaction detection, problems which theory cannot resolve, or which must be solved either arbitrarily or by making untestable assumptions about the connection between theoretical construct and measurement factor. Later he considers the role for the discovery system for the development of theory, and the influence of Robert K. Merton is evident. In *Multivariate Model Building* he states in the first chapter that he is not attempting to deal with the basic scientific problems of conceptualizing causal links or with latent

and manifest functions, but only with the apparent relations between measured constructs and their congruence with an underlying causal structure. He defines a "theory" as sets of propositions which describe at the abstract level the functioning of a social system, and proposes that in the inductive phase, *ex post facto* explanations of the relationships found within the data may form a basis for assembling a set of interrelated propositions which he calls a "middle range theory", that describes the functioning of a specific aspect of a social system. The **AID** system facilitates the inductive phase by identifying interacting variables, so that mathematical functions relating sociological variables are correctly specified for statistical estimation.

Sonquist draws upon an introductory text, *An Introduction to Logic and Scientific Method*, written in 1934 by two academic philosophers of science, Morris R. Cohen and Ernest Nagel. Cohen (1880-1947) received his Ph.D. from Harvard in 1906, and Nagel (1901-1985) studied under Cohen at City College of New York and received his Ph.D. from Columbia University in 1931. The relevant chapter in the book is titled "The Method of Experimental Inquiry", which examines the experimental "methods" for discovering causal relationships advanced by Francis Bacon and later elaborated by John S. Mill. These Baconian experimental methods are anything but Romanticist: the two authors define the search for "causes" to mean the search for some invariable order among various sorts of elements or factors, and the book gives no suggestion that the social sciences should receive any distinctive treatment. Since all discovery systems search for invariant relations, the attractiveness of the Baconian treatment for scientists such as Sonquist is self evident. The propositions which Sonquist views as constituting middle-range sociological theory and which following Cohen and Nagel express a causal relationship, have the linguistic form: *X(1)...X(n)* implies *Y*. The researcher's task in Sonquist's view is to relate the causal proposition to a mathematical functional form, which is statistically estimated, and he concludes that a well specified, statistically estimated mathematical function with a small and random error term, expresses a causal relationship understood as the sufficient condition for an invariant relationship between the dependent or caused variable and the set of independent or causal variables.

In "Computers and the Social Sciences" and "'Retailing' Computer Resources to Social Scientists" in *American Behavioral Scientist* (1977) Sonquist and Francis M. Sim discuss the inadequate social organization in universities for the effective utilization of computer resources, especially by social scientists, whom they report are described derisively by other

academicians as "the marginal computer users." The authors present some arguments for changing the professional roles and social organization of computing in social science departments, and they propose some organizational forms. While the authors' sociological analysis of computing in social science is interesting, and while their reorganization proposals may offer benefits, the underutilization of computer resources and systems analysis by social scientists cannot be remedied by such measures as academic reorganization, so long as the prevailing philosophy of science is still Romanticist. Reorganizing roles could do no more for Romantic sociology than could re-arranging deck chairs for the sinking *H.M.S. Titanic*.

Examination of Sonquist's writings in their chronological order suggests that, as he had attempted to expand the discovery function of his system, he discovered that he had to move progressively further away from the Romanticism prevailing in contemporary academic sociology. He would have been better served by the contemporary Pragmatist philosophy of science, than he had been by invoking the 1930's Positivist views of Cohen and Nagel. Both Positivism and Romanticism give a semantically based definition of "theory" and ontologically based criteria for scientific criticism. On the Pragmatist view "theory" is defined by the pragmatics of language, its function is what Hanson called research science as opposed to almanac science. And according to the Pragmatist realism practiced by Galileo, Einstein and Heisenberg and formulated as "ontological relativity" by Quine, every causal claim is based exclusively on the empirical adequacy of a tested theory. Discovery systems therefore are not only able to discover causality; they can also make causal theories.

Hickey's correspondence with the editors of the academic sociological journals described above reveals that these editors and their selected referees are committed to the enforcement of archaic criteria for scientific criticism, criteria based on an institutionalized Romantic philosophy of science. In this context Sonquist's **AID** discovery system was not merely an anomaly in academic sociology. Developed in 1963 at the apex of Parsonian Romanticism, the **AID** system was a portent. It signaled the widening cultural lag in academic sociology between technological modernity represented by the innovative computerized discovery systems on the one hand and anachronistic philosophy represented by an atavistic paleo-Romanticism on the other hand. This conflict is not merely one between alternative theories; a multiplicity of empirically acceptable conflicting theories is consistent with Pragmatism. This conflict is furthermore institutional; and it is irreconcilable. The new technology is an implementation of ontological relativity, which subordinates ontology to

empirical criticism, while the old philosophies subordinate empirical criticism to their peculiar ontological dogmas. Only one or the other type of criterion can prevail, and the prevailing one will at least banalize if not completely exclude the other.

Comment and Conclusion

On Pragmatism vs. Romanticism

Contemporary Pragmatism differs fundamentally from Romanticism. Romanticism requires a mentalistic ontology as a criterion for scientific criticism, such that any proposed explanation describing a mentalistic ontology is rejected out of hand without regard to its demonstrated empirical adequacy. Pragmatism on the other hand accepts only empirical criteria for scientific criticism, and rejects all prior ontologies as criteria for scientific criticism. Thus Pragmatism permits but does not require mentalistic ontologies. This difference is due to the different concepts of the aim of science. Romanticism defines the aim of cultural science as the development of explanations having semantics that describe mentalistic ontologies, a semantics that Romantics call interpretative understanding. On the other hand Pragmatism does not define the aim of social science in terms of any ontology. Pragmatists will accept any theory as an explanation that has been empirically tested and not falsified regardless of its semantics and associated ontology.

For example consumer services and nondurable consumer goods, which together represent fifty-eight percent of the gross domestic product of the United States national economy. But econometric models for these sectors are seldom empirically satisfactory when based on microeconomic theory. The models' errors in reproducing a development sample are large and manifest conspicuous serial correlation. Furthermore the algebraic sign on the statistically estimated parameter for relative price is often positive instead of negative, and is seldom statistically significant. But the experienced business econometrician will add demographic variables that account for the fact that consumers enter the market for a product at a certain age in their life cycles, and may also exit a market at some later age. Trying various time lags in the number of aggregate births or birth rates easily accomplishes this. The result does not conform to the Romantic philosophy of science, because it is a rare consumer who makes a purchase decision, and knows the size of his demographic cohort, much less knows his place in his

society's demographic profile. Furthermore the demographic variables so dominate the model that their inclusion often make the relative price and income variables statistically nonsignificant thus requiring exclusion of these traditional microeconomic variables. Typically nonsignificance also occurs when variables representing advertising and nonprice promotions, which are paradigmatically Romantic variables representing sellers' attempts to influence consumers' product knowledge and attitudes, are included in category or industry models. Such variables representing nonprice competition are often important only for individual brands and not for whole industries or product categories. And even in the industry models these Romantic variables must be statistically significant and the model must otherwise be empirically acceptable, or the econometrician will reject the model.

The net effect is that the econometric model that works well empirically is not always the model that conforms either to received microeconomic theory, or to the Haavelmo agenda, or to the Romantic philosophy of science. Of course there are other sectors of the economy for which econometric models work well, when the models include variables representing either expectations data or economic conditions consciously considered by the actors, as Haavelmo had prescribed. But their inclusion must be subordinated to the Pragmatist concepts of the aim of science, criticism, and explanation, if interpretative understanding is have a role in scientific explanation instead of remaining an investigative humanities subject like history or metaphysics. A central insight of the contemporary Pragmatist philosophy of science is that only empirical criteria can reveal the connection between actions or events and their consequences in social and behavioral sciences. Historically in successful science empirical criteria have always had priority over all other considerations for theory evaluation. It took about three hundred years for natural scientists and philosophers to recognize this. Apparently it will take another hundred years or more for the retarded slow learners in the social sciences to institutionalize it also.

On Pragmatism vs. Psychologism

Is computational philosophy of science conceived as cognitive psychology a viable agenda for twenty-first century philosophy of science? Both Simon and Thagard recognized the lack of empirical evidence needed to warrant claims that their computational cognitive systems are anything more than very rough approximations to the structures and processes of the human mind. In fact Simon furthermore admitted that in some cases the

historical discoveries replicated with the discovery systems described in his *Scientific Discovery* were actually performed differently from the way in which the discovery systems performed the rediscoveries. Recognition of this variance amounts to the falsification of the cognitive psychology claims. Yet Simon did not explicitly reject his colleagues' discovery systems as empirically falsified psychology. Rather the psychology claims were tacitly ignored, while the systems continue to be developed without independent empirical research into psychology to guide new cognitive system development.

Others have also found themselves confronted with such a conflict of aims. In "A Split in Thinking among Keepers of Artificial Intelligence" the *New York Times* (18 Jul. 1993) reported that scientists attending the annual meeting of the American Association of Artificial Intelligence expressed disagreement about the goals of artificial intelligence. Some maintained the traditional view that artificial-intelligence systems should be designed to simulate intuitive human intelligence, while others maintained that the phrase "artificial intelligence" is merely a metaphor that has become an impediment, and that AI systems should be designed to exceed the limitations of intuitive human intelligence. The article notes that the division has fallen along occupational lines with the academic community preferring the psychology goal and the business community expressing the alternative goal, and also that large AI systems have been installed in various major American corporations. This alignment is not inherent, since the academic community need not view artificial intelligence exclusively as an agenda for psychology. But the alignment is understandable, since the business community financially justifies artificial-intelligence systems pragmatically as it does any other computer system, and it has no interest in faithful replicas of human limitations such as the computational constraint described in Simon's thesis of bounded rationality or the semantical impediment described by Hanson and called the "cognition constraint" by Hickey. This is the same pragmatic justification that applies generally in basic scientific research. Scientists will not use discovery systems to replicate the scientist's limitations, but to transcend these limitations to enhance the scientist's research capability, productivity and performance.

Artificial intelligence may have outgrown its original home in academic psychology. The functioning of discovery systems developed to facilitate basic science research is more adequately described as constructional language-processing systems with no psychological claims. The relation between the psychological and the linguistic perspectives can be illustrated by way of analogy with man's experience with flying. Since

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primitive man first saw a bird intended for dinner spread its wings and escape the hunter by flight, mankind has been envious of birds' ability to fly. This envy is illustrated in ancient Greek mythology by the character Icarus, who escaped from the labyrinth of Crete with wings he made of wax. The story describes him as having flown too near to the hot sun, so that he fell from the sky as the wax melted, and then was drowned in the Aegean Sea. Icarus' fatally flawed choice of materials notwithstanding, his basic design concept was a plausible one in imitation of the evidently successful flight capability of birds. Call Icarus' design concept the "flapping-wing" technology. A contemporary development of the flapping-wing technology with superior materials might serve well for an investigation of how birds fly, but it is not the technology used for modern flight by mankind. Successful human flight has evolved very different technologies, such as initially the hot-air balloon, then the Wright brothers' fixed-wing motor-powered propeller airplane, then later the rotary blades of the helicopter, and most recently the jet and the rocket. It is noteworthy that none of these successful technologies imitate the capabilities of the birds - a fact that is not surprising, when one recalls that the motivating aim was not to investigate birds, but rather to enable men to fly.

When proposed imitation of nature fails, pragmatic innovation prevails, in order to achieve the motivating aim. Therefore when asking how a computational philosophy of science should be conceived, it is necessary firstly to ask about the aim of philosophy of science and whether or not computational philosophy of science is adequately characterized as normative cognitive psychology. Contemporary Pragmatist philosophy of science views the aim of basic science as the production of a linguistic artifact having the status of an "explanation", which is a theory that has been proposed and not falsified when tested empirically. Then the aim of a computational philosophy of science in turn is derivative from the aim of science: to enhance scientists' research practices by developing and employing mechanized procedures capable of achieving the aim of basic science. To accomplish this, the computational philosopher of science should be at liberty to employ any technology - any computer hardware or software design irrespective of psychology or neurology - that facilitates production of testable theory proposals and therefore of scientific explanations. And it might be added that the empirical test is prediction, which in the event is a contribution to basic science. Since a computer-generated explanation is a linguistic artifact, the computer system may be viewed as a constructional language-processing system. Psychology or neurology may or may not suggest some tentative hypotheses to this end.

But the aim of basic science does not require reducing a computational philosophy of science to the status of a specialty in either psychology or neurology, any more than the aim of aerospace science need be reduced to a specialty in ornithology. Thus to construe computational philosophy of science as normative cognitive psychology, as Thagard would have it, is to have lost sight of the aim of basic science. And to date attempts at a cognitive psychology of science appear to have offered basic science no better prospects for improvement of research practices, than did the Icarus wing-flapping technology for manned flight. In retrospect the thesis that it could, might be labeled the "Icarus fallacy." Accordingly in computational philosophy of science the phrases "cognitive psychology" and "artificial intelligence" are rendered as irrelevant as "engineering ornithology."

The developers of the practical and successful discovery systems have been practicing researchers in the sciences for which they have developed their discovery systems. They have created systems that have produced serious and responsible proposals for advancing the contemporary state of the empirical sciences in which they work. To date none have been cognitive psychologists. Those fruitful discovery systems are Sonquist's **AID** system, Litterman **BVAR** system, and Hickey's **METAMODEL** system. If they have not been cognitive psychologists, neither have they been academic philosophers. In some cases their understanding would have benefited from the contemporary Pragmatist philosophy of science perspective. Sonquist, who developed the **AID** system, is a practicing research sociologist. His inadequacy in contemporary philosophy of science led him to turn to Positivism, in order to evade the obstructionist Romanticism still prevailing in academic sociology. Pragmatism would have served him better. Now known as the **CHAID** system, Sonquist's discovery system is probably the most widely used of all the discovery systems created to date. It is also the earliest. For Litterman, evasion of the Romantic philosophy was easier. He is an economist who developed his **BVAR** system under teachers at the University of Minnesota who were rational expectations advocates. Ironically their economic "theory" notwithstanding, they were economists who had rejected Haavelmo's structural-equation agenda, thereby rendering Romanticism inoperative for determining the equation specifications for econometric model construction. But Litterman would have had a better understanding of the significance and value of his work for economics, had he understood the contemporary Pragmatist philosophy of science. His system is still used by the Federal Reserve Bank of Minneapolis. Hickey was more fortunate, since he is both an Institutionalist economist and – notwithstanding the reformist

obstructionism of the University of Notre Dame philosophy faculty – a contemporary Pragmatist philosopher of science. In the thirty years since he created his system, he has applied his **METAMODEL** discovery system for market analysis of both consumer and industrial products, for consumer credit risk analysis, for macroeconomics, for regional economics, and for macrosociology.

The practical discovery systems developed by Sonquist, Litterman, and Hickey also reveal a distinctive strategy. Their designs, procedures, and computer languages remained close to the analytic practices actually used by researchers in their respective sciences. The difference between these systems and those developed by Simon, Thagard, and other cognitive psychologists, echoes the old philosophical issue between the ideal-language and the ordinary-language philosophers in the twentieth century. What may be called the ordinary-language computational philosophy-of-science approach used by Sonquist, Litterman, and Hickey is based on the analytical techniques that are ordinary in their sciences. The ideal-language cognitive-psychology philosophers of science use computer languages that are not used in the sciences in which they implemented their systems and that furthermore often have structures resembling the Russellian symbolic logic. The Logical Positivists' used Russellian logic for philosophy of science, and their approach is now of interest only to the antiquarian or historian. Today the ideal-language discovery systems are the exclusive property of the academic cognitive philosophers of science. The world of science still awaits their contribution to the advancement of the contemporary state of science.

Computational philosophy of science is the wave of the future that has arrived. But some philosophers will have to make fundamental adjustments in their psychologistic views in philosophy of language. The psychologistic turn has in no small part been due to their doctrinaire nominalism built into their Orwellian newspeak, the Russellian symbolic logic. Yet nothing precludes a linguistic computational philosophy of science that views the discovery systems as language-processing systems and recognizes a three-level semantics enabling philosophers to speak unabashedly about mental concepts without having to make pretentious psychologistic claims. Cognitive psychology of science is still a promissory note issued by a profession having no payment or credit history, and science awaits evidence of its redeeming cash value.

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