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.

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 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, 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 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 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 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 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 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 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)

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 normative 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

"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 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

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 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 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 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 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.

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 consumer 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 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

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 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 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 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

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 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 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 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 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 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 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 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 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:*

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 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 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 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 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.

