HERBERT SIMON, PAUL THAGARD, PAT LANGLEY AND OTHERS ON DISCOVERY SYSTEMS

This book examines the late twentieth-century specialty called "computational philosophy of science", which consists of computerized strategies encoded in the software designs of the automated discovery systems developed by Herbert Simon, Paul Thagard, Pat Langley, Thomas Hickey, John Sonquist, Robert Litterman, Jan Zytkow, Gary Bradshaw and others.

Nobel laureate Herbert Simon is the principal figure considered in this BOOK. Much of this material is presented in reverse chronological order, and the exposition therefore starts with the work of the philosopher of science Paul Thagard, who follows Simon's cognitive-psychology agenda for his computational philosophy of science investigations. Thagard's philosophy of science is rich enough for exposition in terms of the four functional topics in philosophy of science. But before considering Thagard's treatment of the four functional topics, consider firstly his psychologistic views on the nature of computational philosophy of science and on the semantics of conceptual change in scientific revolutions.

Thagard's Psychologistic Computational Philosophy of Science

Thagard has been a Professor of Philosophy at the University of Waterloo since 1992, and is also Adjunct Professor of Psychology, Adjunct Professor of Computer Science, Director of his Computational Epistemology Laboratory, and Director of the Cognitive Science Program. He had previously been an associate professor of philosophy at University of Michigan, Detroit, where he was associated with their Cognitive Sciences Program, and also 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).

Simon, Thagard and Langley

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 exceptional authors who have actually designed, written and exhibited such computer systems for philosophy of science are considered in this book. Paul Thagard, Pat Langley and Herbert Simon are among the few philosophers of science who have the requisite technical skills to make such contributions, and have demonstrated such skills by actually writing systems. Thagard's work is also selected because by the closing decades of the twentieth century he has been one of the movement's most prolific authors and most inventive academic philosophers of science. Thagard follows the artificial intelligence (AI) approach and the psychological interpretation of AI systems initially proposed by 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 cognitive psychology. This contrasts with the established linguistic-analysis tradition that achieved ascendancy in twentieth-century academic philosophy of science, and that Hickey prefers for computational philosophy of science.

The analysis of language has often been characterized by a nominalist view, also called "extensionalism" or "referential theory of meaning." The nominalist view proposes a two-level semantics, which recognizes only the linguistic symbol, such as the word or sentence, and the objects or entities that the symbols reference. Two-level semantics 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 referenced objects.

Two-level semantics is the view typically held by the positivist philosophers, who rejected all mentalism in psychology and who preferred behaviorism. Thagard explicitly rejects the behavioristic approach in psychology and prefers cognitive psychology, which recognizes mediating mental realities. Two-level semantics is the view that is also characteristic of philosophers who accept the Russellian predicate calculus in symbolic logic, which has the notational convention that expresses existence claims by the logical quantifiers. It is therefore in effect a nominalist Orwellian newspeak, in which predicate terms are semantically vacuous unless they are

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placed in the range of quantifiers, such that they reference entities. If the predicates are quantified, the referenced entities are then called either "mental entities" or "abstract entities." Due to this consequent hypostatizing of concepts the positivist philosopher Nelson Goodman divides philosophers into nominalists and "platonists", and identifies himself as a nominalist. Logical positivists adopted the Russellian symbolic logic, although some like Rudolf Carnap and Alonzo Church recognize a three-level semantics with meanings associated with predicates without hypostatizing by quantifying predicates.

Thagard explicitly rejects the behavioristic approach in psychology and prefers cognitive psychology, which recognizes mediating mental realities. But he does not reject the Russellian symbolic logic and he refers to concepts as "mental entities". Conceivably his turn away from linguistic analysis and toward psychologism has been motivated by his recognition of the mentalistic semantical level. Like Simon, Thagard seeks to investigate concepts by developing computer systems that he construes as analogues for the mental states, and then to 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"**, which he defines as **the attempt to understand the structure and growth of scientific knowledge in terms of computational and psychological structures**. He thus aims to offer new accounts both of the nature of theories and explanations and of the processes involved in their development. And he distinguishes computational philosophy of science from general 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 the data structures can model the mind's 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 this book he examines the historical development of the bacteriological explanation for peptic ulcers. He explores how 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. Thus interestingly unlike, *e.g.*, neoclassical economists he states that principles of rationality are not derived a priori, but should be developed in interaction with increasing understanding of both social and human cognitive processes.

Thagard's computational philosophy of science addresses the functional topics: the aim of science, discovery, criticism and explanation. He has created several computer systems for computational philosophy of science. As of this writing none of them have produced mathematically expressed theories, and all of them have been applied to the reconstruction of past episodes in the history of science. None have been applied to the contemporary state of any science, either to propose any new scientific theory or to resolve 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 Pat Langley, Herbert A. Simon, Gary L. Bradshaw, and Jan M. Zytkow in 1987 has neglected conceptual change. (This important book is discussed below in the sections reporting on the views and systems developed by Langley, Simon, Zytkow, Bradshaw and colleagues.) Pat Langley is presently Professor of Computer Science at the University of Auckland, New Zealand, Director for the Institute for the Study of Learning and Expertise as Professor of Computing and Informatics, and Head of the Computing Learning Laboratory at Arizona State University. Bradshaw at the time of this writing is a member of the psychology department at Mississippi State University. Zytkow (1944-2001) received a Ph.D. in philosophy of science from University of Warsaw in 1979, and since 1996 had been chairman of the computer science department at Wichita University, where he founded the Machine Discovery Laboratory. In his later years Zytkow focused on mechanized knowledge discovery by data mining with very large data sets. Thagard proposes both a general semantical thesis about conceptual change in science and also a thesis

specifically about theoretical terms. He maintains that (1) kind-hierarchies and part-hierarchies give structure to conceptual systems, (2) relations of explanatory coherence give structure to propositional systems, and (3) scientific revolutions involve structural transformations in conceptual and propositional systems. His philosophy of scientific criticism is his thesis of explanatory coherence, which is described separately below. Consider firstly his general semantical thesis.

Thagard opposes his psychologistic 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 pragmatist analytic philosophy since Willard van Quine. Thagard says concepts are mental representations that are learned, and that they 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 in hierarchies shows how a theory of conceptual change can involve much more than belief revision. He notes in passing that 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. The idea of kind-hierarchies is not new; the third-century logician Porphyry proposed the tree-hierarchical arrangement since called the Porphyrian tree. In his *Semiotics and Philosophy of Language* (1968) the philosopher Umberto Eco calls the Porphyrian tree a "disguised encyclopedia". Linguists also recognize taxonomic hierarchies.

Thagard maintains that a conceptual system can be analyzed as a computational network of nodes with each node corresponding to a concept, and each connecting 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 old ones in the network. Thagard calls the two most radical 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 a more radical change, and

consists of reorganization by changing the organizing principle of a hierarchical tree. It is exemplified by Darwin's reclassification of human as animal while changing the meaning of biological 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" meant unanalyzable, but clearly Thagard is not opposed to analysis; perhaps "systematic" might be a better term than "holistic".

It is difficult to imagine either "branch jumping" or "tree switching" without belief revision. In his *Computational Philosophy of Science* Thagard references Quine's metaphorical statements in his article "Two Dogmas of Empiricism" in *Logical Point of View* that science is a web of belief, a connected fabric of sentences that faces the tribunal of sense experience collectively, all susceptible to revision. 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 Quine's "web of belief" does not consist of beliefs, but rather consists of rules, concepts, and problem solutions, and the procedures for using them.

In *Conceptual Revolutions* Thagard maintains that semantical 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-view changes. He says that old and new theories have links to concepts not contained in the affected theories. He cites by way of example that while Priestly and Lavoisier had very different conceptual systems describing combustion, there was an enormous amount of information 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. There he said 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. And he said that there are few cases of disagreement about scientific observations, because all humans operate with the same sort of stimulus-driven inference mechanisms.

Consider next Thagard's thesis specific to theoretical terms. Both Thagard and Simon accept the distinction between theoretical and observation terms, and both use it in some of their computer systems. In these systems typically the theoretical terms are those developed endogenously by an AI system and the observation terms are inputted exogenously into the system. But in both their literatures the distinction between theoretical and observation terms has a philosophical significance apart from the 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, in which he includes analogy, is a thesis in 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' foundational agenda and their naturalistic philosophy of the semantics of language. Unlike the positivists he rejects any strict or absolute distinction between theoretical and observable entities, and says that what counts as observable can change with technological advances. And since Thagard is not a nominalist, he does not have the positivists' problem with the meaningfulness of theoretical terms.

But he retains the distinction thus modified, because he believes that science has concepts intended to refer to a host of postulated entities and that it has propositions containing these theoretical concepts making such references. Theoretical 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. Yet he subscribes to the semantical thesis that all descriptive terms – observational terms as well as theoretical terms – acquire their meanings from their functional rôle in thinking. Thus instead of a naturalistic semantics, he apparently admits to a kind of 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. 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. Functionally Thagard could retire these linguistic atavisms – "theoretical" and "observational" – if instead he used the terms "endogenous" and "exogenous" respectively to distinguish the descriptive terms developed by a system from those inputted into it.

Collaboratively with Keith J. Holyoak, Thagard developed an artificial-intelligence system called **PI** (an acronym for "Process of Induction") that among other capabilities creates theoretical terms by conceptual combination. Hickey says that in the expository language of science all descriptive terms – not just Thagard's theoretical terms – have associated with them concepts that are combinations of other concepts ultimately consisting of semantic values that are structured by the set of beliefs in which the concepts occur.

Thagard's system **PI** 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 deemed unobservable, while in fact the instances of the perceived effects of water waves and sound waves are observable. In fact contrary to Thagard a simple standing sound wave can be observed in an enclosed smoke chamber. In **PI** the combination is triggered when two active concepts have instances in common. **PI** only retains such 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 the sound-wave combined concept 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 from a source is "stronger" than the rule that waves spread in a single plane, where the "strength" of a rule is a

parameter developed by the system. Thus the combination of the three-dimensional wave is formed. The meaningfulness of this theoretical term is unproblematic for Thagard, a post-positivist philosopher.

Thagard on Discovery by Analogy and Systems ACME and ARCS

In *Conceptual Revolutions* Thagard distinguishes three 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 design that creates new theories by the coherence-driven method, but he has implemented the other two methods in his systems.

Consider firstly data-driven generalization. The central activity of artificial-intelligence system **PI** is problem solving with the goal of creating explanations. The system represents knowledge consisting of concepts represented by nodes in a network and of propositions represented by rules linking the nodes. Generalization is the formation of general statements, such as may have the simple form "Every X is 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 of kinds of instances referenced.

Consider secondly explanation-driven discovery by 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, i.e., data types in **LISP** property lists, which are called "messages", "concepts", and "rules." The message type represents particular results of observations and inferences. The concept type locates a concept in a hierarchical network of kinds and subkinds. The concepts manage storage for abductive problem solving. The rules type represents laws in the conditional "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, i.e., empirical generalizations. (2) Existential abduction, which postulates the existence of formerly unknown objects. This type results in theoretical terms referencing theoretical entities, which was discussed in the previous section above. (3) Rule-forming abduction, which produces rules that explain other rules. These rules are the 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 schema 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 laws.

In *Conceptual Revolutions* Thagard also mentions a fourth type of abduction, (4) analogical abduction, which uses past cases of hypothesis formation to generate hypotheses similar to existing ones. But he treats analogy at greater length in his *Mental Leaps: Analogy in Creative Thought* (1995) co-authored with Keith Holyoak. In *Conceptual Revolutions* 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 in the way the investigator uses analogy to try to understand the targeted domain by seeing it in terms of the source domain. Analogic 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 a "multiconstraint theory", because he identifies three regulating constraints, which are (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 rôles 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 consists of 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 the practice of several historic scientists.

Each of the three regulating constraints – similarity, structure, and purpose – is operative in four steps that Thagard distinguishes 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). In 1987 Thagard and Holyoak developed a procedure whereby a network could be used to perform analogical mapping by simultaneously satisfying the four constraints. The result was the **ACME** system, which mechanizes the mapping function. It creates a network when given the source and target analogues, and then 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 procedures and criteria whereby scientists choose to abandon an old theory and its conceptual system, and accept a new one. He sets forth principles for his system called **ECHO** 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 two propositions do not cohere; i.e., they resist holding together. The terms "explanatory" and "analogous" are primitive terms in the system, 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 the propositions in the system. Furthermore the acceptability of a proposition that explains a set of evidence propositions is greater than the acceptability of a proposition that explains only a subset or less than the number in the set including a subset.

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In "Explanatory Coherence" in *Behavioral and Brain Sciences* (1989) and in several later papers Thagard's theory of explanatory coherence is implemented in a system written in the **LISP** computer language that applies connectionist algorithms to a network of units. The system name "**ECHO**" is an acronym for "Explanatory Coherence by Harmony Optimization". Although elsewhere Thagard mentioned a coherence-driven discovery method, his **ECHO** system is a system of theory choice. 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 the eight principles of explanatory coherence in the above list says that a proposition coheres with another, an excitatory link is established between the two propositions in the computer network. And when one of the eight principles says that two propositions incohere, then 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, winning 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 *firstly* those that explain a greater breadth of the evidence than their competitors. Then *secondly* it prefers those that 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 naïve 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.

Thirdly in addition to breadth and simplicity the system prefers those exhibiting analogy 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 and 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: *firstly* explain more evidence, *secondly* provide simpler explanations, or *thirdly* are analogous to other explanatory hypotheses. This is Thagard's philosophy of scientific criticism.

These three criteria are also operative in his earlier **PI** system, where breadth is called "consilience." During execution this 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 effect on every other unit linked to it by a path, however lengthy. He reports that 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 applied system **ECHO** to several revolutionary episodes in the history of science. He lists (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 two criteria of simplicity and analogy. **ECHO** seems best suited to evaluate nonmathematically expressed alternative theories, but can also evaluate mathematical theories.

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 "cognitive architecture", by which is meant a general specification of the fundamental operations of thinking, and he references Herbert Simon's "General Problem Solver" agenda.

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: **The aim of science is 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 can 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 equates to 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 and practice of ontological relativity. Thagard's acceptance of the distinction between observation and theoretical terms suggests that he does not admit the thesis of ontological relativity.

Herbert Simon and Logic Theorist

1978 Nobel-laureate Herbert Simon (1916-2001), a polymath of promethean creative intelligence, was born in Milwaukee, Wisconsin, and 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. He was also a member of the University's board of trustees. His excellent intellectual 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 operations that people use in their thinking processes. He and his long-time collaborator, Alan Newell, had concluded that computers could be applied generally to imitating human 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 symbolic 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 implementation of his heuristic-search methods for symbol manipulation.

Simon defines artificial intelligence as symbolic processing, and he defines cognitive psychology as understanding human thinking by modeling problem solving with artificial-intelligence systems. Newell and Simon have developed many artificial-intelligence systems, several of which are described in their lengthy 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 his published articles relating information processing concepts to scientific discovery, and most notably his and Pat Langley's *Scientific Discovery; Computational Explorations of the Creative Process* (1987), which describes several discovery systems that simulated discoveries of 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 in quantum theory, so too examination of the development of the artificial-intelligence discovery systems requires consideration of issues in the social sciences including notably economics. 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 to review the various alternative rationality theses including Max Weber's semantical thesis of "ideal-types." So, let us start with the prevailing neoclassical economists' concept of rationality.

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 **first** and principal alternative to Simon's view is the prevailing neoclassical rationality postulates, which say 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 United States 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 postulate of rationality.

The idea of rationality in economic behavior emerged in the Enlightenment era of Western thought. The neoclassical postulate of rationality originated 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 he aspired to describe economic life as a harmonious mechanism, as Newton had done for the heavens. Ever since then academic economists have been enthralled by this agenda, which has much less empiricism than rationalism. 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 famous doctrine of the "invisible hand."

However Smith's perspective is not closed or self-contained. It is 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 Smith's moral constraints on the pursuit of self-interest from theoretical economics. 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 a school of economists called the American Institutionalists.

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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 that many academics find compellingly attractive, in which the rationality postulate became a matter of calculating the global maximization of consumer utility and producer profits by setting the first derivative of the relevant mathematical function to zero and then calculating the positive maximum critical point in the function's second derivative. The theory of relative price determination describes the allocation of all goods and services in an optimally efficient manner later called "Pareto optimality" after the economist Vilfredo Pareto.

A half century later there was another revolution called the "Keynesian revolution" named after the English economist, John Maynard Keynes. Pre-Keynesian economic theory had assumed that the Pareto optimum allocation resulting from rational maximizing behavior by each consumer and producer would also maximize income, output and employment for the whole economy, as both 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 or stagnation, which he said had occurred during the Great Depression of the 1930's. This new thesis resulted in economists' dividing economics into the "microeconomic" theory of relative prices, which is about the determinants of maximally efficient allocation of resources in response to consumer preferences, and the "macroeconomic" theory of maximal aggregate income, output and employment determination. But while Keynes produced a revolution in economic theory, he did not explicitly attack the classical economists' rationality postulate of individual human behavior, even though his consumption and liquidity preference relations did not conform to the classical rational psychology of maximizing postulates. Nonetheless his underemployment equilibrium thesis cogently attacked the classical economists' optimistic thesis of maximized national income, output and employment.

Soon afterwards economists began applying statistical inference techniques to estimate equations with the macroeconomic data developed by 1971 Nobel-laureate economist Simon Kuznets of Wesley Mitchell's 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 Trygve Haavelmo's 1944 paper, however, nearly all the econometricians have been neoclassical economists requiring that the selection of explanatory 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 testing techniques to econometric models structured in accordance with 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 romanticism of the earlier times.

The above overview of the neoclassical rationality theses of human behavior reveals that rationality 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 theses 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 simplistic unrealism of the neoclassical theory with its heroically imputed rationality theses. Neoclassical theorists have not been completely unaware of these problems caused by their fidelity to the maximizing rationality theses. 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 proposes the same ideas that Simon explicitly and specifically 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.

Weber defined the ideal-type as a mental construct that has two basic features: The ***first*** feature is 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 German 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 the central aspect of reality that Weber includes in the ideal-type in so-called "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. 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 seldom 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 ideal-types. As Weber explicitly 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 assumes the maximizing rationality, which almost never corresponds completely with reality but only approximates it.

Thus 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. What is conventionally called "pure economic theory" with its ideal-types is true where it is applicable, and it is deemed inapplicable 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 evasive denial of falsification by contrary evidence is so unabashedly 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 continuous Newtonian track of the free electron in the Wilson cloud chamber, his response too was to revise the Newtonian language for describing the observed cloud-chamber tracks. 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 just dismissively 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 for a more empirically adequate post-neoclassical economic theory, much less as a phenomenon to be included in a reinterpreted or revised test-design language.

Many contemporary economic theorists are only less explicit in their dogmatic adherence to neoclassicism with its definitive maximizing rationality. They are reluctant to dispense with the elegantly uniquely determinate mathematical solutions enabled by merely setting the first derivative of the demand equations to zero and then checking the second derivative for a maximum inflection point, even though the commercially viable econometric models used in business and government almost never have their equation specifications deductively derived from maximizing assumptions. Yet the accepted college economics textbooks are replete with graphs that do not represent actual measurement data, but are just imaginary

descriptions of what the received theory says the measurement data **should** look like.

The siren of mathematical elegance has compellingly seduced these blackboard economists. They are scandalized by the observed absence of optimizing behavior and the rejection of their maximizing theses, because it implies that paradigmatic problems thought to have been so elegantly solved after two-hundred-fifty years of theoretical development in the neoclassical tradition have not actually been solved at all. Ensconced academic economists have dutifully labored for years to earn their doctorate credentials and then have obsequiously groveled before the journal editors and referees to get their papers published and before their colleagues to get tenure. They do not welcome being advised that their ostensibly 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 that it is hardly more ontologically realistic than the Ayn Rand romantic-utopian novels used to propagandize it for the general public – to say nothing of reactionary American politicians. But the Great Recession that started in 2007 has produced a crisis for economic rationalism.

Interestingly Lloyd S. Shapley, a recipient of the 2012 Nobel Prize for Economic Sciences, told the *Globe and Mail* (15 October 2012) that he has never in his life taken a course in economics. As 2001 Nobel-laureate economist Joseph Stiglitz explains at length in his book *Freefall: America, Free Markets, and the Sinking of the World Economy* (2010) that the failure of economic rationalism has been egregious, since these rationalistic dogmas underlie the practices that caused the Great Recession. And it might be added – just they had caused the Great Depression of 1929-1933. In the chapter titled "Reforming Economics" he states that economics has moved from being a scientific discipline into becoming free-market capitalism's biggest cheerleader. He critiques its Walrasian general equilibrium approach and its fallacious belief that the so-called efficient or perfect market unrestrained by any government regulation is self-correcting, and he references recent studies that show there is no scientific basis for this belief. He reports that as a graduate student he soon concluded that rationality is nonsense, that his colleagues had an irrational faith in the assumption of rationality, and that shaking their irrational faith would not be easy. He describes various cases in which people systematically act irrationally including their behaviors that produce speculative bubbles. Doctrinaire

neoclassical economics might be a paradigm for Kuhn's dogmatic "normal science", save for the crucial fact that for its true believers its elegant rationalism admits to no anomalies that might occasion correction, much less a new scientific revolution. Yet today there are still true believers blithely ignoring falsifying data and experience, and teaching their elegantly deductive Scholastic-like dogma to unwary students. They are latter-day Weberians.

Yet in truth not all economists are purists devoted to their orthodoxy of "pure economic theory". The ascendancy of econometric modeling 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 rather make statistically acceptable models, than to incur large error residuals in his statistical equations, and try to dismiss them as merely "irrational" behavior that can be ignored notwithstanding egregiously bad forecasts. 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 practicing pragmatists. They have turned increasingly to data analysis for their equation specifications, and include in their equations even such heretical noneconomic factors as demographic, sociological or political variables, which are never found in sanctioned textbooks' pontificating neoclassical theory. And Simon is so scandalously heretical as to replace the sacrosanct maximizing rationality postulates. Read on (carefully).

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 distinctive functions, and he generalized from this experience that people bring decisions within reasonable bounds by identifying with partial

goals for which their own organizational units are responsible. The Institutionalist economist John Commons calls this a "rationing decision".

This insight was taken up in Simon's Ph.D. dissertation (1942), which he later published as *Administrative Behavior* (1947), the book referenced 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 writes that his entire scientific output may be described as a gloss on two basic ideas contained in his *Administrative Behavior*. They are that (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 and artificial intelligence.

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 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 singular 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. ***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 dismissively 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 more realistic thesis 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. He says that the study of cognitive processes or procedural rationality is interesting only when the substantively rational response is not trivial or obvious. It is usually studied in situations in which the decision-maker must gather information of various kinds, and must process it in various 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 participants 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 1974 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. Simon 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?

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, that describes the markets of the real world, and that the substantive rationality of neoclassical theory is worthless, since executable maximizing algorithms do not back it up. 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 as an institution that reduces the amount of nonlocal information which economic participants 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 in the history of economics 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 the classical economists to construct a deductive theoretical economics based on their maximizing 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, a revolt that rejected the natural-law and utilitarian traditions in European academic philosophy.

The founding father of American Institutionalism is an iconoclastic economist and eccentric individual named 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, and most notably he proposed that the institutions evolve. Thus he introduces what today would be called a sociological perspective. 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) of the University of Wisconsin. In his *Institutional Economics* (1934) Commons states explicitly that he is following the pragmatist philosophy of Charles S. Peirce, the founder of pragmatism at Harvard University. 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 become more empirical. Weber had explicitly denied that the ideal-type is a hypothesis. Commons says that use of changeable hypotheses makes less utopian the utopias that our minds create. Commons anticipates Simon in important respects, but unlike Simon, Commons does not explicitly propose revising the maximizing assumption in the neoclassical rationality postulate. But he rejects its centrality to economics. 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), Commons proposes 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. Simon's experiences with rationing in Milwaukee come to mind.

Commons 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 statement of the central thesis 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 prior to the Roosevelt Administration, the courts viewed collective bargaining by labor unions as an illegal conspiracy in restraint of trade. The second and third types of transactions, however, are also relevant to Simon's interests.

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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 *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 **functionalist explanations are about movements of a system toward stable self-maintaining equilibria.** Most notably Simon states that there is no reason to suppose that the attained equilibria are global maxima. Thus functionalist explanation describes satisficing behavior.

In this paper he furthermore notes that functionalist 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. This qualitative functionalism is represented in the sociological literature. Like economic sociologists, who recognize the underlying rôle of economic institutions, Simon argues that economists have in fact not actually limited themselves to maximization analyses, but have utilized qualitative functionalist 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. Functionalist 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.

There have been more recent dissenters than the Institutionalists to conventional academic economics, and the reason for dissent as always has been the empirical inadequacy and simplistic unrealism of the neoclassical theory with its heroically imputed rationality postulates. More recently a new empirical psychological style of economic research has emerged that is called "behavioral economics". One pioneer in this new style at the University of Chicago is 2017 Nobel laureate Richard H. Thaler, who published a book titled *Misbehaving: The Making of Behavioral Economics* (2015). Thaler wrote an article in *New York Times* (10 May 2015) Business Section front page titled "The Importance of Irrelevance". Thaler notes in the article that an important problem for neoclassical economic theory is economists' discounting as irrelevant any factor that does not influence the maximizing rational thinking of a person. Like Veblen, Thaler rejects economists' insistence on studying mythical maximizing creatures often known as *Homo Economicus*, creatures that Thaler ridicules with the name "Econs". Thaler says that for his theory many supposedly irrelevant factors such as emotions do matter. In a *The Wall Street Journal* (16-17 May 2015) review of Thaler's book, a review titled "How Homo Economicus Went Extinct" and subtitled "Making Economics Irrational", Carol Tavris expresses incredulity at how delusional academic economists are with rational economic "theory". Upon winning his Nobel Prize Thaler said in a *Chicago Tribune* newspaper (10 October 2017) interview that his inspiration for behavioral economics came forty years earlier, while reading the works of 2002 Nobel laureate economist Daniel Kahneman and Amos Tversky. In 2013 Kahneman published his recent *Thinking Fast and Slow*, in which he reported how nonrational behavior be economic actors are predictable, and that therefore economic models can be made to account better for human behavior and decision making.

But while behavioral economics relies on survey research instruments often used by social psychologists and modern market researchers for businesses, Simon proposes "cognitive psychology" based on his imputed bounded rationality postulate of satisficing behavior.

Human Problem Solving, Cognitive Psychology and Heuristics

Simon's theory of human problem solving is his theory of procedural rationality, 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 that has set the course for their book is the development of a science of information processing. 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, a concept that 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 both the mathematical and computational 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 computer program to predict the resulting time path, and then by comparing this predicted path with the actual path of the system. The advantage of an information-processing language over the mathematical languages for a theory of thinking is that an information processing language takes symbolic structures rather than numbers for the values of the variables.

The information theory about human thinking and problem solving is a theory in cognitive psychology. Newell and Simon note that their cognitive 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 that 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 furthermore 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 problem-solving thinking as *a system of elementary information processes, organized hierarchically and executed serially, and consisting of procedures that exhibit large amounts of highly selective trial-and-error search based on rules of thumb or "heuristics".* 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 in his autobiography 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 **LISP**, 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. ***Firstly*** there is the "task environment", by which he means the problem-solving processor's outer environment. ***Secondly*** the task environment as viewed by the problem solver produces 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 there is a 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*** type 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.

The *third* type of method is Simon's theory of heuristics, 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 made toward it. This dependence functions as a feedback 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 Simon believes that it explains how complex problems are solved with both human and mechanical bounded rationality.

These three alternative methods represent different artificial-intelligence research programmes, software development v hardware development, which may also be characterized as knowledge v speed. The generate-and-test method is dependent on fast hardware; the heuristic-search method is dependent on efficient software design. Developments in hardware technology, as well as the magnitude of the problems they select affect researcher preferences for one or another of the methods. 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. It has often been implemented in supercomputers.

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. Developments such as the quantum-computing technology promise to enable the combinatorial generate-and-test method with effectively minimal hardware constraint.

On Scientific Discovery and Philosophy of Science

Before Simon and his colleagues at Carnegie-Mellon University had developed functioning computerized discovery systems simulating historic scientific 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 insightfully comments in his "Introduction" that dense mists of romanticism and downright knownothingness have surrounded the subject of scientific discovery and of creativity. And 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 he used to explain the processes of problem solving. Problem-solving thinking uses a collection of elementary information processes organized hierarchically and executed serially, and consists 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.

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Superior problem-solving scientists have more powerful heuristics, and therefore produce either 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 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. Curiously Simon also says that a theory explaining scientific discovery must predict innumerable failures for every success.

In this same 1966 article 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 to conform to certain conceptions of logic than with how scientists do in fact proceed. And he adds that their notions of how scientists ought to proceed have focused 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: Norwood Russell Hanson and Thomas S. 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 philosophers' 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 ironically titled *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 is also opposed by Peirce. Peirce used the term "abduction", 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.

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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. ***Simon 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.*** Simon emphasizes that the heuristic strategy does not guarantee success. He 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. 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. 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* he explicitly rejects the search for certainty associated with attempts to justify inductivism. He subscribes to Popper's falsificationist thesis of criticism.

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 to 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 defining 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 for scientific revolutions. 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, namely 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 intended to explain human problem solving and scientific discovery need not incorporate a highly powerful mechanism for inventing completely novel representations. Even in revolutionary science the representations are rooted in the past and not from whole cloth.

Later in his "Ramsey Eliminibility and the Testability of Scientific Theories" (1973) reprinted in his *Models of Discovery* 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.

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 he adds 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 from the givens of the problem and then 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 (1987)* 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 himself had identified a particular episode as revolutionary. 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 pagan religion 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 used by the logical positivists.

Nonetheless Simon's psychological approach shares difficulties with Kuhn's sociological approach. Philosophers' reaction against Kuhn's sociological approach was often due to the recognition that conformity to and deviance from a consensus paradigm may explain the behavior of scientists without explaining the success of science. Turn next to 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 for science is his *Scientific Discovery: Computational Explorations of the Creative Processes* (1987) co-authored with several colleagues including notably Pat Langley. Simon is the editor of the book. In the introductory section he says that the central hypothesis of the theory of scientific discovery 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.

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The theory of scientific discovery is therefore a theory in cognitive psychology. Simon 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. The book exhibits a series of computer systems capable of making nontrivial scientific discoveries, which are actually replicated discoveries of historic scientific laws and theories including but not limited to empirical generalizations. 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.

Simon 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 same problem. Then the system can be tested as a psychological theory of cognitive behavior by comparing the trace with the protocol. But Simon also admits that his book supplies no detailed comparisons with human performance. And in discussions of particular applications involving particular discoveries, **he notes that in some cases the historical discoveries were actually performed differently than his systems performed the rediscoveries.** The interest in this book therefore is actually system design and performance rather than psychological testing and reporting.

Simon states that he wishes to provide some foundations for a normative theory of discovery, which 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 theories. Accordingly Simon sets forth **a satisficing 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 some 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 the computer's memory

capacity and computational speed, but also due to the limit imposed by the inventory of available heuristics.

Langley's BACON and Other Discovery Systems

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In his *Novum Organon* (Book I, Ch. LXI) Francis Bacon had expressed the view that with a few easily learned rules or 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 instead 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 skill, so exactly is it with his method. Computer discovery systems do not quite warrant Bacon's optimism, but they are a huge improvement over inexplicable and mysterious intuition so dear to romantics. Today Bacon's agenda is called proceduralization for mechanization, and it is appropriate therefore that Pat Langley's early and successful discovery system should be named **BACON**.

The **BACON** discovery system is a set of successive and increasingly sophisticated discovery systems that make quantitative theories from data. Given sets of observation measurements for several variables, **BACON** searches for functional relations among the variables. The search heuristics in earlier versions of each **BACON** computer program are carried forward into later ones, and the later versions contain new 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 Langley in 1979 for his Ph.D. dissertation written in the

Carnegie-Mellon department of psychology under the direction of Simon. The dissertation is titled *Descriptive Discovery Processes: Experiments in Baconian Science*. Langley 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 in Simon's *Scientific Discovery* (1987).

The **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 internally 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 repeats 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 replicated the discoveries of 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 later version named **BACON.3** has rediscovered Coulomb's law of electrical current. For making these discovery replications Simon and his associates used measurement data actually used by the original discoverers. His book references W.F. Magie's *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 *Scientific Discovery*. 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", firstly 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 discoveries of laws replicated by **BACON.4** include: (1) Ohm's law of electrical circuits, where the intrinsic properties 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 replicated discovery of Gay-Lussac's principle that the relative densities of elements in their gaseous form are proportionate to their corresponding molecular weights. Replicating discovery of 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 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 measurement 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, where the system is given very general theoretical postulates. Then it reasons analogically by postulating symmetries between the unknown law and a theoretical postulate given to the system as input. The general theoretical postulate given 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, presently CEO of Cycorp, Inc. of Austin Texas, published his Ph.D. dissertation titled *AM: An Artificial Intelligence Approach to Discovery Mathematics as Heuristic Search* written at Stanford University. 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 describes Simon's theory of heuristic problem solving in science and the specific heuristics in his **AM** discovery system. While Lenat's article includes discussion of artificial intelligence in empirical science, his **AM** computer system is not for empirical science, but develops new mathematical concepts and conjectures with the heuristic strategy. He also published "Automated Theory Formation in Mathematics" in the 1977 *IJCAI Proceedings*. This paper offers a more detailed description of the system's two-hundred fifty heuristics, and 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 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 this paper 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 (AI) 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. Interestingly he notes that in a scientific profession the community members' work is in parallel, whereas 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.   Three such systems named **GLAUBER**, **STAHL** and **DALTON** are described in *Scientific Discovery* (1987). The **GLAUBER** discovery system developed by Langley in 1983 is named after the eighteenth century chemist, Johann Rudolph Glauber, who contributed to the development of the acid-base theory. For its historical reconstruction of the acid-base theory **GLAUBER** was given facts 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, but **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 rôles 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 describes the reactions in a general law that states that acids and alkalis react to produce salts.

The second discovery system is **STAHL** developed by Jan Zytkow. From 1982 to 1984 he was a visiting professor at Carnegie-Mellon and worked with Simon and Langley. **STAHL** 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** replicates 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 later been rejected in favor of newer theories 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 of 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 says that the same principles of inference were widely used by chemists in their search for componential explanations of chemical substances and their reactions. Thus he claims that the 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 earlier by Amadeo 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 reaction.

Since the publication of *Scientific Discovery* Simon and his associates have continued their work on discovery systems and have pursued their work in 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 **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 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 on the basis of the current state of the rule space, and the outcome 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 the 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 shifts in the focus of attention and by 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 therefore not *ad hoc*.

More recently in "Two Kinds of Knowledge in Scientific Discovery" (2010) Langley and Bridewell at the Institute for the Study of Learning and Expertise in Palo Alto, CA, describe a computational approach that carries out search through a problem space for a "reasonable" explanation, i.e. one that is "interpretable", because it is familiar to scientists. In general their approach models *processes* with *constraints* – the processes provide the content from which scientists construct models, while the constraints correspond to theoretical principles about how to combine processes. Their discovery system is called "inductive process modeling", **IPM**, which they define as: given (1) observations for a set of continuous variables as they change over time, (2) generic entities that have properties relevant to the observed dynamics, (3) generic processes that specify causal relations among entities using generalized functional forms, and (4) a set of entities present in the modeled system – then find a specific process model that, when given initial values for the modeled variables and values for any

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exogenous variables, explains the observation data and predicts unseen data accurately. A system that carries out these steps would produce a model that links domain knowledge to scientific data, and importantly the model would explain the measured phenomena in a formalism much like a scientist's own.

A technical justification for Simon's heuristic-search approach followed by Langley is the view that the alternative combinatorial generate-and-test approach would require excessive computer resources. The search employs generic processes, which are a form of background knowledge that defines the space of candidate models, and modeling constraints that are another type of scientific knowledge. But the authors also invoke a philosophical justification for the **IPM** system design: they say that scientists call upon theory-level constraints, in order to exclude "implausible models". Using theory-level constraints the system searches through a problem space for a "reasonable" explanation that is acceptable to scientists, **because it is based on a relevant theory.** This discovery strategy implements what Hickey calls "theory extension". The authors also state that the causal explanatory content of the model stems from its relationship to scientific concepts and not from the equations themselves, and that equations without a theoretical interpretation provide a description of system dynamics, but are not explanations. Thus two kinds of scientific knowledge are distinguished: theory-based "explanations" and databased "descriptions". This philosophy is at variance with the contemporary pragmatism and its theses of relativized semantics and ontological relativity.

The authors illustrate the system to develop the Lotka–Volterra equations for population dynamics in protists. Stereotypically foxes prey upon rabbits, which in the absence of the predators would overgraze thus starving the rabbits and then the foxes. This is a "quantitative process model", which in "Discovering Ecosystem Models from Time-Series Data" (2003) by Langley *et. al.* is defined as a set of processes, each specifying one or more algebraic or differential equations that denote causal relations among variables along with optimal activation conditions. The **IPM** system uses a nonlinear optimization routine called "beam search" to estimate parameter values in the equations. In computer science beam search is a search algorithm that explores by expanding the most promising node or state in a tree diagram. At each level of the tree it generates all successors of the states at the current level, sorts them, and then stores only a predetermined number of best states at each level, called the "beam width".

But it is not an optimizing algorithm, because at the end of the tree the search may or may not have found the optimum state.

In their Lotka-Volterra demonstration implementation the entities with types **predator** and **prey**, each type has a variable that stores its respective population size. The system includes processes and entities related to population dynamics using predator–prey experiments between microscopic species using time-series data collected by Jost and Adiriti reported in "Identifying predator-prey processes from time series" in *Theoretical Population Biology*, 57 (2000) 325-337 and by Veilleux reported in "An analysis of predatory interaction between Paramecium and Didinium" in *The Journal of Animal Ecology*, 48, (1979) 787–803. The model generated by **IPM** successfully predicted that, when the predator population is high, the prey population decreases exponentially with predation controlled by multiplicative equations that add predators for each prey that is consumed.

In their "Integrated Systems for Inducing Spatio-Temporal Process Models" (2010) Pat Langley, Chunki Park, and Will Bridewell describe a more sophisticated system they call **SCISM**, an "integrated intelligent system". **SCISM** solves the task of **IPM** systems that account for spatial and temporal variation, and is furthermore integrated with a constraint learning method to reduce computation during induction. Once provided with background knowledge consisting of spatio-temporal data and the knowledge encoded in a library of generic processes and entities, **SCISM** has a learning component that searches through the space of possible models. This part of the system integrates an algorithm for exploring the space of model structures with one for estimating the parameters of a particular structure. The combined procedure for model generation has three steps: 1. Generate all possible instantiations of generic processes with specific entities but without parameter values. 2. Combine instantiated processes to form a generic model that satisfies all the structural constraints. 3. Estimate the parameter values and scores each model's fit to the data. After this search the system returns the quantitative process model that best accounts for the data.

Two decades earlier Langley and Shrager had described their philosophy of science more elaborately in *Computational Models of Scientific Discovery and Theory Formation* (1990). The book reports on a symposium with twenty-four contributors including Simon, Thagard and

Langley. In the introductory chapter titled "Computational Approaches to Discovery" the editors affirm the cognitive-psychology conceptualization of the computational approach, and divide scientific behavior into "knowledge structures" and "knowledge processes".

The *knowledge structures* include: (1) "observations", which represent recordings of the environment made by sensors or measuring instruments, (2) "taxonomies", which define or describe concepts for a domain along with specialization relations among them, (3) "laws", which are statements that summarize relations among observed variables, objects or events, (4) "theories", which are hypotheses about the structures or processes in the environment, and which describe unobservable objects or mechanisms, (5) "background knowledge", which is a set of beliefs or knowledge about the environment aside from those that are specifically under study, (6) "models", which are descriptions of the environmental conditions for an experimental or observational setting, (7) "explanations", which are narratives that connect a theory to a law by a chain of inferences appropriate to the field.

Langley proposes that the *knowledge processes* that use these structures should include the following: (1) "the "observation process" inspects the environmental setting by training an instrument, sometimes only the agent's senses, on that setting to produce a concrete description, (2) "taxonomy formation and revision" involves the generation of empirical laws that cover observed data, (3) "theory formation and revision" generates a theory from which one can derive the laws for a given model by explanation, thereby interconnecting a set of laws into a unified account, (4) "deductive law formation" produces laws from a theory by using an explanatory framework to deduce both a law and an explanation of how that law derives from the theory, (5) "the "explanation" process connects a theory to a law by a narrative whose general form is given by the field's explanatory framework, (6) "experimental design" generates models of settings in which observations are made.

The authors call the above conceptualization the "classical view of science". To the extent that there is any systematic philosophy of language, the philosophy of science is unquestionably positivist. It has positivism's identifying dichotomy between theories and laws on the basis of unobservables that echoes Mach and other earlier positivists, and its characteristic organization of levels consisting of theories explaining laws of

laws explaining observations and data that echoes Duhem and the positivists of the Vienna Circle. On Langley's version the models function as the empirical laws in Duhem's schema. Like all positivist views, Langley's "classical view" is a philosophy of science that is based on the old paradigm of Newtonian physics, not on the newer pragmatic paradigm of contemporary quantum theory. The stereotypic paradigm they seek to imitate is Kepler's planetary laws of the orbit of Mars explained by Newton's gravitational *theory*, while Kepler's *laws* are viewed as merely descriptive summaries of the celestial observations of Mars. In other words theories "explain", while laws or models merely "describe". They believe that causal relations cannot be extracted from the models. This view is contrary to the contemporary pragmatist thesis, as for example the philosopher of science Russell Hanson set forth in his *Patterns of Discovery*.

Langley's systems are not without interest, because there are problems in basic research that can be addressed effectively with the design of the **IPM** and **SCISM** systems. But these systems are not for big-game hunting, as it were, for new contributions to science; they are for hunting hares rather than hippos, because if the user inputs familiar theories, he will get only those familiar theories for output, and will get nothing newer much less fundamentally superior. In general ***the more old knowledge that is built into a system, the less new knowledge that can come out of it.*** Familiarity in the output may gain acceptance among the conventionally minded, but familiarity is a high price to pay at the expense of discovery; it is a Faustian bargain. The scientists whose practices are modeled by such theory-driven systems suggest what Thomas Kuhn called "normal science" – the detailing and extension of accepted paradigms, such as what Langley calls "generic processes". The aim of Kuhn's "normal" science is the further articulation of the familiar paradigm by a "puzzle-solving" type of research uncritical of the paradigm.

Langley's **IPM** strategy applied in economics would amount to automating the Haavelmo agenda: The generic processes that are theory-inspired and deemed "causal" are the concepts of supply and demand, the generic entities are the quantities demanded and supplied, the relative price and the aggregate income constraint, and the observations are the time-series measurements for a specific industry. But historically it was fidelity to these familiar classical ideas that proved to be the biggest obstruction to recognition of a distinctive macroeconomic perspective at the time of Keynes' *General Theory*. Today Langley's positivist ideas also echo the

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views of the handful of sociologists who attempt sociometric modeling while demanding sociopsychological-causal "explanations".

It is furthermore ironic that in this book these authors should reference philosophers such as Kuhn and Feyerabend, who truculently rebelled against this "classical" positivist view. As it happens, neither Langley's "representational structures" nor his "mechanized algorithmic processes" in discovery systems designs need be cast in the positivist context, nor need they be conceptualized in the psychologistic terms of cognitive psychology. The system designs can be conceptualized in pragmatic terms and described as language-processing systems. Furthermore scientists became pragmatic, because their historic and greatest discoveries including notably quantum theory were not what they had expected or found "reasonable". They accepted the unexpected and "unreasonable", because the new finding was empirically more adequate.

Nor need the "representational structures" and "mechanized algorithmic processes" in discovery systems be conceptualized in psychologistic terms. As it happens, in "Processes and Constraints in Explanatory Scientific Discovery" (2008) Langley and Bridewell appear to depart from the cognitive psychology interpretation of their **IPM** discovery systems. They state that they have **not** aimed to "mimic" the detailed behavior of human researchers, but that their systems address the same tasks as ecologists, biologists, and other theory-guided scientists, and that their systems carry out search through similar problem spaces. They have thus taken a step toward pragmatism and away from psychologism.

Simon's Philosophy of Science

Simon's literary corpus is rich enough to contain a philosophy of science that addresses all four of the functional topics.

Aim of Science

What philosophers of science call the aim of science may be taken as a rationality postulate for basic scientific research. In his autobiography in an "Afterword" titled "The Scientist as Problem Solver" Simon explicitly applies his thesis of bounded rationality developed for economics to scientific research. This explicit statement would not have been necessary for the attentive reader of his literary corpus. He describes his concept of

scientific discovery as a special case of his concept of human problem solving, because both concepts are based on his strategy of heuristic search. And he views his strategy of heuristic search in turn as a special case of his postulate of bounded rationality.

To this metascientific concept one need only add that Simon's application of his thesis 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 satisficing 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", an incidental circumstance that bounds the scientist's rationality and constrains the scientist from global optimization of solution search. The research scientist is therefore a satisficer, and the aim of the scientist is satisficing within both the institutional empirical and the incidental computational constraints.

Scientific Explanation

Simon's views on 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. When he makes the distinction, variables that have their associated numeric values assigned before the system is run are considered to be observational variables, while those that receive their values by the operation of the discovery system are considered to be theoretical variables. 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 to be theoretical terms. Simon also 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. This definition is compatible with the contemporary pragmatist sense, in which observation language is merely test-design language given

particular logical quantification.

Unfortunately Simon does not follow through with this pragmatic relativizing of semantics to problem-solving discovery systems, but reverts to the positivist concept of explanation. In his exposition of **DALTON**, which creates 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 these examples involve a structural model in which macroscopic phenomena are described in terms of their inferred component atoms.

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 measurement-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 three-level hypothetical-deductive concept of explanation in which theories are said to "explain" deductively empirical laws, and the empirical laws in turn deductively explain observation reports of particular events. In this view theories and empirical generalizations are distinguished by their semantics.

On the pragmatist view theory and empirical description are not distinguished semantically, but are distinguished pragmatically by their use in basic-science research. Theory is what is proposed for empirical testing, and description in test design is what is presumed for testing. Explanation employs language that was theory but then made into law after it has been empirically tested and not falsified. One who speaks of "theoretical explanation" is thus merely speaking of a proposed explanation, which is an antilogous concept. The pragmatist concept is a functional or research-science view of the language of science instead of the positivist catalogue-science view. Thus given that the discovery systems are problem-solving systems, defining "theory" and "explanation" relative to the discovery

system is to define them consistently with the pragmatist philosophy.

Scientific Discovery

In addition to the physical theories that the discovery systems rediscovered, 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 no economic theory embodying his thesis of bounded rationality lends itself to creation by any discovery system like those that he or his colleagues have yet designed. So, there is irony here. Simon's discovery systems are purportedly explorations in cognitive psychology with his heuristic-search system design exhibiting his thesis of bounded rationality. But the subjects to which his heuristic-search system design should be applicable cannot be the romantic's subjective mental states such as motives and values or the bounded-rational deliberative processes of human subjects.

Scientific 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 recognize the interdependence between these two functions in scientific research. Simon also notes this interaction between discovery and justification in *Scientific Discovery*, because it is integral to the heuristic procedure in 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 by the heuristics is a major source of the efficiency of the discovery methods. The product of each step of a search is evaluated in terms of the evidence it has produced, and the search process is modified on the basis of the outcome of these evaluations. 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 theses: the neoclassical thesis of global rationality still prevailing in academic economics today, his own thesis of bounded rationality, and the rational-expectations hypothesis. The reader of Simon's autobiography, however, would never likely guess that about two decades after its first appearance, the rational-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 call **BVAR** models "atheoretical." But if the term "theory" is understood in the pragmatist sense, the equations created by the **BVAR** system are economic theories, because they are universally quantified and proposed for empirical testing. Before taking up the **BVAR** system, 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 inevitably find reactionaries 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 agenda. 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 economists in the Graduate School of Industrial Administration.

Muth's rational-expectations hypothesis, which Simon curiously says deserves a Nobel Prize even though he maintains that the hypothesis 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 just a pronouncement of what business firms ought to do. The idea of rational expectations is not a pet without pedigree. It is a variation on an approach in economics known as the Stockholm School, in which expectations play a central rôle, and which Muth references in his article. Therefore 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 1977 Nobel-laureate economist Bertil Ohlin, 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 theory of underemployment the "Swedish theory of unused resources." In 1949 he published his *Problem of Employment Stabilization*, his macroeconomic theory, which concludes with a critique of Keynes' *General Theory* from the Stockholm School viewpoint. He had earlier published a summary of his "Stockholm Theory of Processes of Contraction and Expansion" as "The Stockholm Theory of Savings and Investment" in the *Economic Journal* (1937).

In his critique Ohlin draws upon a distinction between *ex ante* or forward-looking anticipations perspective and *ex post* or backward-looking historical perspective. The distinction refers not to the viewpoint of economists but to the viewpoint of the economic participants in the economy. This distinction was firstly proposed by 1974 Nobel-laureate economist Gunnar Myrdal (1898-1987), Ohlin's colleague of Stockholm School persuasion and fellow critic of Keynes. Later in life Myrdal evolved his theory of *ex ante* perspective 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 price inflation. In Keynesian economics price inflation is thought to be due to excessive aggregate demand, the opposite of stagnation. Myrdal does not address the effect of institutional change on the structural parameters in econometric models, and he dislikes econometrics.

In the first chapter, "Development of Economics: Crises, Cycles", Myrdal says that when he was still in his "theoretical stage" of thinking, *i.e.*, pre-Institutionalist stage, he was involved in 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 but is a neoclassical economist, also maintains that the *ex post* perspective alone cannot provide an explanation in economics, because any explanation must reference factors that govern actions, and actions refer to the future. Any economic explanation must therefore contain the *ex ante* perspective, which consists of the expectations or plans of the participants in their economic roles. Ohlin notes that Keynes theory may be said to contain an *ex ante* perspective of investment, because it includes the "marginal efficiency of capital", which is similar to Wicksell's "natural rate of interest" – the expected rate of 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 viewpoint there must be an *ex ante* analysis of saving, because saving and investment are performed by different persons. Ohlin maintains 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", and says that the saving-investment equilibrium must be expressed in terms of an equality of *ex ante* aggregate variables. Then contrary to Keynes' law of consumption Ohlin makes *ex post* consumption residual to *ex ante* savings and income. Oddly he does not also require an *ex ante* variable for aggregate consumption, which must also partake in macroeconomic equilibrium. Ohlin's Stockholm School approach is significant in the present context not only because Ohlin offers an explanation of how expectations are formed, but also because unlike the rational-expectations advocates he accounts for expectations by explicit variables, namely the *ex ante* variables, so that their effects need not be incorporated implicitly in the statistically estimated parameters of the econometric models.

Ohlin's elaborate 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 needed explanation. Muth notes two conclusions from studies of expectations measurements, which he says his rational-expectations hypothesis explains. The *first* conclusion is that, while admitting considerable cross-sectional differences of opinion, the averages of expectations made by economic participants in an industry are more accurate than the forecasts made with naïve models, and are as accurate as elaborate equation systems. 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 notably that the way expectations are formed depends on the structure of the entire relevant system describing the economy. His more convoluted statement of his hypothesis is as follows: he says that the 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 from commodity speculation, from running a business firm, or from selling information. In fact contrary to Muth, economists from Ricardo to Keynes have made large profits in speculation, as do such famous investors as Warren Buffet and George Sorros. In his discussion of price expectations Muth offers an equation for determining expected price in a market, and references a paper to be published by him. The published equation says that expected price is a geometrically weighted moving average of past prices. It is actually an autoregressive model. 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* of his two conclusions 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 or on average 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 inadequate for explaining observed phenomena, especially as the phenomena change over time. Muth's view is the opposite of Simon's: Muth maintains that 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 toward stable equilibrium, when the **feed forward** in the control system has destabilizing consequences, as when each participant is trying to anticipate the actions of others and their expectations. Simon writes that the paradigmatic example in economics is the speculative price bubble. Feed forward is also known as positive feedback loop.

In the next section of *Sciences of the Artificial* 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 participants form their expectations "rationally", by which is meant that the participants 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 argues that the rational-expectations hypothesis erroneously ignores 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 required to carry out the expectations strategy. He concludes that since economists have little empirical knowledge about how people form expectations about the future, it is difficult to choose among the models that are currently proposed by competing economic theories to account for cyclical behavior of the economy.

The recent Great Recession crash of 2007 has fully vindicated Simon's 1959 critique. In his *After the Music Stopped* (2013) Princeton University economist and former Vice-Chairman of the Federal Reserve

System, Alan S. Blinder wrote that beginning in the 1990's Americans built a "fragile house of cards" based on asset-price bubbles exaggerated by irresponsible leverage, encouraged by crazy compensation schemes and excessive complexity aided and abetted by embarrassingly bad underwriting standards, by dismal performances by the statistical rating agencies, and by lax financial regulation. Together these elements of the house of cards supported each other to create a **positive feedback loop.**

Similarly in his Nobel Prize Lecture "Speculative Asset Prices" reprinted as the appendix to his book *Irrational Exuberance* (2015), 2013 Nobel laureate Yale University economist Robert J. Shiller established that stock markets' excessive volatility violates the efficient-markets hypothesis. The rational-expectations hypothesis is also known as the "efficient-market" hypothesis and also as the "random-walk" hypothesis. Since 1991 Shiller has also been a Director of the National Bureau of Economic Research (NBER) program in Behavioral Economics, which like other Institutionalists recognizes the importance of psychological, sociological and epidemiological behaviors in price determination, while depreciating the traditional rationality postulates. Federal Reserve Board Chairman Alan Greenspan coined the phrase "irrational exuberance" in 1996. The thesis of Shiller's book *Irrational Exuberance* based on his questionnaire surveys made at the Yale International Center for Finance, is that a *positive feedback* between investor psychology (irrational exuberance) and rising prices for assets such as equity shares, bonds and real estate, produce speculative price bubbles. Shiller measures the psychology component with the Yale International Center for Finance's "Valuation Confidence Index", a time series spanning1989-2014, and he likens the deceptive speculative bubbles to "natural" Ponzi scams and pyramid schemes.

In *Irrational Exuberance* Shiller lists many precipitating factors initiating irrational exuberance in three recent booms: the stock market, the bond market and the real estate market. Most of the factors are historically unique. Their irrational effects in turn are amplified by a **positive feedback loop**, a speculative bubble; as prices continue to rise, the level of exuberance is enhanced by the price rise itself. The psychological feedback involves changes in thought pattern that infect the entire culture as well as changes in prices, such that investors optimistically believe that a "new era" of opportunity has arrived. This irrational exuberance drives asset prices to unjustifiable heights. Shiller demonstrated with data graphs the relation between the volatile real (inflation adjusted) S&P Composite Stock Price

Index for the period 1871 to 2013, and the much steadier trend in the calculated present values for the same period of subsequent real dividends that firms paid out. The excessive volatility shown by the stock prices violates the efficient-markets rational-expectations hypothesis.

Muth had proposed his rational-expectations hypothesis as an explanation of two conclusions about expectations measurements. Therefore 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 therefore is an attempt to construct conventional structural-equation econometric models using *ex ante* expectations data, in order to demonstrate and test their explanatory hypothesis. But neither Muth nor the rational-expectations advocates took this approach. Historical macroeconomic *ex ante* time-series data are rare. But on the basis of his hypothesis Muth shifted from an explanation of empirical measurements of reported *ex ante* expectations to consideration of a forecasting technique using only *ex post* data.

This semantical shift has had three noteworthy effects on subsequent empirical work by the rational-expectations school: **Firstly** there was a disregard of available empirical expectations measurements that could serve as values for *ex ante* variables however few there are. **Secondly** there was an attack upon the conventional structural-equation type of econometric model and the development of an alternative type of empirical model as an implementation of the rational-expectations hypothesis but with no independently collected expectations measurements. **Thirdly** there evolved the design and implementation of a computerized procedure for constructing this alternative 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 three-quarters of a century, and it is still the prevailing practice in the economics profession where neoclassical economics prevails. To the dismay of conventional econometricians the rational-expectations advocates' attack upon the conventional neoclassical structural-equation econometric model is, therefore, barely less subversive to the status quo in the science, than Simon's attack on the neoclassical rationality postulate. And this outcome

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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 the econometric model, the structural-equation model. And for this we turn to Trygve Haavelmo.

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", which was initially a Ph.D. dissertation written in 1941 by 1989 Nobel-laureate econometrician, Trygve Haavelmo (1911-1999), and then later published as a supplement to *Econometrica* (July 1944). *Econometrica* is the journal of the Econometric Society, which was founded in 1930, and which describes 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 approaches in economics". The July supplement by Haavelmo advanced certain fundamental ideas for the testing of mathematical hypotheses expressing economic theory by application of the Neyman-Pearson theory of statistical inference. At the time that the supplement was published the society's offices were located at the University of Chicago, where econometricians found themselves isolated and unwelcome. In those days 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, but mostly because like sociologists today few economists were competent in the requisite techniques, which are now routinely taught to undergraduate students in economics departments.

Haavelmo argued quite unconventionally that time series data points are not a set of successive observations, but are one single observation with as many dimensions as there are independent variables in the model. This bizarre rationalization is not mentioned in textbooks today. The more enduring aspect of Haavelmo's structural-equation agenda consisted of construing the econometric model as a probabilistic statement of the economic theory, so that theory is neither held harmless by data that falsifies it nor immediately and invariably falsified as soon as it is confronted with

measurement data. He says that the model is an *a priori* hypothesis about real phenomena, which states that every set of numeric values that the economist might observe of the "true" variables, will be one that belongs to the set of numeric values which is admissible as the solution for the model's equations. 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 econometric models. 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 semantics that a variable symbol has due to its context consisting of 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 has its semantics defined by an ideal test design that the economist could at least imagine, 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. And he claims that there are also many indications that the economist nearly always has some such ideal test design and true variables "in the back of his mind", when the economist builds his theoretical models. In other words in the verbal description of his model in economic terms the economist suggests either explicitly or implicitly some type of test design to obtain the measurements for which he thinks his model would be empirically adequate. The measurements for the true variables are not only collected in accordance with an ideal test design, but are also error free. Thus before estimation and testing of the model the theoretical and true variables are distinguished but are not separated in the fully interpreted theory.

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 are collected for some other purpose. For example the

National Income Product Accounts (N.I.P.A.) data used for macroeconometric modeling are collected from tax records. The true variables are those such that if their behavior should contradict a theory, the theory would be conclusively rejected as false. On the other hand were the behavior of the observational variables to contradict the theory, the contradiction would be due to the fact that the economist is using observational variables 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. Thus 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 econometrician, 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 windowless ivory-tower theoreticians. Today there is much more adequate data available to economists from government agencies and private data-collection syndicates. 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 actually measure. For example in their *Industrial Production* monthly releases the Board of Governors of the Federal Reserve System says that when its monthly production index series cannot be based on physical measures of **output**, such as tons of steel or assemblies of automobiles and trucks, then it reports that monthly **input** measures, such as hours worked or kilowatt hours of electricity consumed in production are used to develop a monthly output quantity series. Nonetheless, the Federal Reserve Board calls these proxy data "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 test design setting forth ideal measurement procedures. Most often the descriptive words associated with theoretical variable symbols contextually defined in a mathematical model

are vague with respect to test design and not given further semantical resolution until measurements are actually collected and associated with the model. Then the description of the actual measurement procedures supplies additional information to resolve this vagueness. In the case of macroeconometric models for example descriptions of the procedures and sources used by the U.S. Commerce Department's Bureau of Economic Analysis (B.E.A) for collecting the N.I.P.A. data, supply the additional semantics that resolves the vagueness in the concepts symbolized by descriptive variables in the macroeconomic theory. It is only when economists like those with the Federal Reserve Board decide to use proxies for what they wish to measure, that there is more deviation involved in the data than just errors of measurement. Then such proxies introduce an equivocation like Haavelmo's "true" and "observational" semantics instead of supplying a resolution to the vagueness in the univocal meanings of the terms in the theory.

The second chapter titled "The Degree of Permanence of Economic Laws" sets forth Haavelmo's concept 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 of model that Haavelmo advocates in this monograph. The rational-expectations hypothesis is proposed as an explanation for structural breakdown, and it is the rationale for the vector-autoregression type of model that is an alternative to the structural-equation model. The **BVAR** discovery system constructs a refined version of the vector-autoregression 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. This is an unwitting statement of ontological relativity. 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 is equivalent to Popper's thesis that there is no observation without theory, and to Hanson's characterization of observation as theory laden; it is a statement of semantical relativity. But the term "theory" in Haavelmo's monograph means specifically the neoclassical economic theory with its rationality

postulates, 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 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 domestic product (GDP), which is merely the summation of its component sectors on either the income side or expenditure side. The *second* type is the technical relation. The paradigmatic case of the technical relation is the production function, which relates physical output to physical inputs such as capital and labor inputs. Technical engineering equations are more properly the tasks of applicable natural sciences, but the practice among econometricians has been to estimate aggregate production functions with the same statistical techniques that they use for all econometric equations. And to measure physical quantities in production functions, econometricians routinely use constant dollars, i.e., deflated current-dollar aggregates.

The **third** type is the relation describing the economic decisions of the participants. Neoclassical economists call equations of this type "behavioral equations" or "decision functions". The behavioral equations in conventional romantic econometric models are based on economic theory, and are not like the laws and theories developed in the natural sciences such as physics. Romantic neoclassical economic theory purports to describe a mental decision-making process made by economic participants, 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 consciously considered by the economic participants in relation to their motivating preference schedules or priorities as they make their best or optimized decisions, and the outcomes of these optimizing decisions are represented by the value of 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 "jump over the middle link" of preference schedules in the scheme, and claim that the decisions of the individuals or firms are determined by the set of independent variables representing factors that the participants mentally consider.

In this romantic neoclassical scheme the econometric model is based on the assumption that participating consumers' decisions to consume and businesses' decisions to produce can be described by certain fundamental behavioral relations, and that there are also certain behavioral and institutional restrictions upon the participant'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 structures in this sense; the failure in particular cases to solve this problem is usually manifested by an erroneous forecast with the model, which is called a "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 phenomenon represented by the model's dependent variable, and the solution therefore consists of recognizing the missing factor and then introducing an explanatory variable for it into the model.

Firstly in the case of a model of supply and demand in a market, one of the reasons for structural breakdown is a structural change due to the irreversibility of economic relations. This change is a shift in a demand curve, such that price-quantity pairs no longer represent movements along the demand curve, because the economic participants 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 additional factors responsible for the revision of preference schedules and consequent shifts in the demand curve. Econometricians routinely do this today.

A *second* explanation for structural breakdown is the simplicity of the model. Economists like simple models, even though the real world is complex. From a purely statistical point of view the simpler the model, the less the likelihood of distorting collinearity. 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 exclude factors mentioned in a theory, which would be sufficient to explain apparent structural breakdown that may occur later in reality, because the excluded

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factors do not presently exhibit a statistically detectable factual influence in the sample history used to estimate the equation.

One recent example of this reason for structural breakdown is the American domestic cigarette industry. Statistics collected by the U.S. Federal Trade Commission (FTC), and the U.S. Center for Health Statistics (NCHS) show that for most of the post-World War II era until the late 1990's, the quantity of domestic cigarette consumption in the United States was determined almost wholly by changes in the national demographic profile, advertising bans notwithstanding. And statistics collected by the U.S. Bureau of Labor Statistics (BLS) during this time show that relative prices rose little and only very gradually, making the relative price variable statistically nonsignificant in a model estimated with data prior to 1997. But with the "Global Settlement Agreement" with several State governments in 1997 the industry agreed to $370 billion settlement in response to litigation, and then with the "Master Settlement Agreement" with the remaining State governments in 1998 the industry agreed to an additional $246 billion settlement. The industry then greatly raised prices of cigarettes to acquire the needed funds for making the large settlement payments over an agreed twenty-five years. The result was effectively a high excise tax passed on to consumers, with the result that consumption declined dramatically, in spite of significant positive changes in the national demographic profile. Thus the new and formerly missing factor that produced structural breakdowns of cigarette industry econometric models estimated with pre-1997 data was the sharply increased relative price of cigarettes, making the relative price variable statistically significant in a model with the longer time series.

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 syntactical 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 rôle 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 affect the other. An autonomous equation is one that has successfully identified a fundamental behavioral relation described by neoclassical theory.

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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., *ad hoc*, or whether it is an effective test of a valid 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 prior to applying the statistical testing procedure, so that it is possible to calculate the power of the test and to determine the risk of error involved in 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 false hypothesis or the error of rejecting a true hypothesis.

Haavelmo's prohibition of use of the Neyman-Pearson statistical inference theory for discovery is ignored by the rational-expectations advocates. And it is also ignored by social scientists who have taken up the practice generically referred to as "data mining", which today is enabled by the enhanced processing power of the electronic computer. Developers of discovery systems like Hickey, who use regression modeling for computational philosophy of science, also ignore Haavelmo's prohibition.

Mary S. Morgan states in her *History of Econometric Ideas* 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 rôle of applied econometrics was downgraded relative to the theory-testing rôle. She notes that Haavelmo's paper was very influential both within the Cowles Commission and with others including Herbert Simon, which may explain why Simon never designed a discovery system for use in social science.

Mitchell's Institutionalist Critique

Haavelmo's agenda had its Institutionalist critics long before the rational-expectations advocates and data-mining practitioners. Morgan also

notes in her *History of Econometric Ideas* that some economists including the Institutionalist economist Wesley Clair Mitchell (1874-1948) opposed Haavelmo's approach. Mitchell had an initiating rôle in founding the prestigious National Bureau of Economic Research (N.B.E.R.), where he was the Research Director for twenty-five years. In 1952 the National Bureau published a biographical memorial volume titled *Wesley Clair Mitchell: The Economic Scientist* edited by Arthur Burns, a long-time colleague and later a Federal Reserve Board Chairman.

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 a paper by Mitchell titled "Quantitative Analysis in Economic Theory" in *American Economic Review* (1925). Mitchell 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 classical economist 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. Thus while Simon's thesis of bounded rationality is a radical departure from the neoclassical optimizing concept of rationality, Mitchell's is much more radical, because he dispensed altogether with such imputed motives.

In his "Prospects of Economics" in Tugwell's *Trend of Economics* (1924) Mitchell also said that economists would have a special predilection for the study of institutions, because institutions standardize behavior thus enabling generalizations and facilitating statistical inferences. He prognosticated in 1924 that as data becomes more available, economics would become a quantitative science less concerned with puzzles about economic motives and more concerned about the objective validity of its account 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 the monumental two-volume work *Types of Economic Theory* (ed. Dorfman, 1967).

Mitchell's principal work setting forth the findings from his empirical investigations is his *Measuring Business Cycles* 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 then 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. Their stated agenda is to concentrate on a systematic examination of the cyclical movements in different economic activities as measured by historical time series, and to classify these data with respect to their phasing and amplitude. They hoped to trace causal relations exhibited in the sequence that different economic activities represented by the time series reveal in the cycle's critical inflection 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 perturbations due to 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. In *Readings in Business Cycles* (1965) the American Economic Association has reprinted selections from this contentious literature. Defense of Haavelmo's structural-equation approach was given by 1975 Nobel-laureate economist 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 Haavelmo's 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 their actions 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 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 characteristics may not be deducible from the behavior characteristics of the component units.

Koopmans wrote "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 and 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 claims 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 rejected the dichotomy. 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 those of 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. Some philosophers have called Koopmans' thesis "methodological individualism". 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 or exhaustively in terms of the physical properties of constituent water molecules, so too for the economic waves of the business cycles cannot be describe exclusively or exhaustively in terms of the behavior of participant individuals. Both types of waves may be described as "real", even if the reality is not easily described as an "entity".

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 (B.E.A.) published the National Bureau's business cycle leading-indicators with selections of its many cyclical time series and charts in their monthly *Survey of Current Business*, which is the Federal agency's principal monthly periodical. In 1996 the function was also taken over by the Conference Board, which calculates and releases the monthly Index of Leading Indicators based on Mitchell's approach. The index has been occasionally reported in national media such as *The Wall Street Journal*. On the other hand the Cowles Commission's structural-equation agenda has effectively conquered the curricula of academic economics; today 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 be vindicated. 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 thesis of maximizing behavior, which enabled economists to use differential calculus to express and develop 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 debunked this optimism, 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 as revealed by relative prices.

This new macro perspective also brought certain other less obvious novelties. Ostensibly the achievement of Keynes' theory was to explain the less-than-full-employment equilibrium by the classical economic psychology that explains economic behavior in terms of the heroically imputed maximizing rationality theses. The economic historian Mark Blaug of the University of London writes in his *Economic History and the History of Economics* that Keynes' consumption function is not derived from individual maximizing behavior, but is instead a bold inference based on the known relationship between aggregate consumer expenditures and aggregate national income. Supporters as well as critics of Keynes knew there is a problem in deriving a theory in terms of communities of individuals and groups of commodities from the classical theory set forth in terms of individuals and single commodities.

For example in Keynes' macroeconomic theory saving and investment behaviors have a different outcome than in microeconomic theory, a difference known as "the paradox of saving". When the individual increases his saving he assumes his income will be unaffected by his action. But when the aggregate population seeks to increase its savings, consumption is thereby reduced and consequently the incomes of others and perhaps themselves will be affected, such that in the aggregate savings are reduced. Thus a motivated attempt to increase saving by individuals causes a reduction of their savings. In his *Keynesian Revolution* the 1980 Nobel-laureate econometrician Lawrence Klein called attempts to derive aggregate macroeconomic relations from individual microeconomic decisions "the problem of aggregation", and he notes that classical economists have never adequately solved this problem. One of the reasons that the transition to Keynes macroeconomic theory is called the "Keynesian Revolution" is recognition of a distinctive macro perspective that is not reducible to the

psychological perspective in microeconomics, the rationality postulate that is its economic psychology. An evident example is Keynes "law of consumption", which he called a psychological law, a law that is *ad hoc* with no relation to the classical rationality postulate. Sociologists do not yet recognize any distinctively macro perspective and still require motivational analyses.

Joseph Schumpeter, a Harvard University economist of the Austrian school and a critic of Keynes, was one of those older economists who were immune from contagious Keynesianism. In his *History of Economic Analysis* he regarded Walrasian general equilibrium analysis the greatest achievement in the history of economics. And 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 expositor of the Austrian school of marginalist economics regarded Keynes' theory as hardly less atheoretical than if Keynes had used data analysis. Schumpeter would accept only a macroeconomic theory that is an extension of microeconomics.

But economists could not wait for the approval of dogmatists like Schumpeter, because the Great Depression had made them desperately pragmatic. 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 hundreds of equations. And this is not the only large Keynesian macroeconometric model; there are now many others, such as the DRI model, now the DRI-WEFA model, the Moody's model and the Economy.com model. These have spawned a successful for-profit 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 some econometricians still 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 alternative business-cycle models is unabashedly pragmatic; it is their forecasting performance that enables these consulting firms to profit and stay in business.

1970 Nobel-laureate economist Paul Samuelson, who wrote in *Keynes General Theory: Reports of Three Decades* that it is impossible for modern students to realize the full effect of the "Keynesian Revolution" upon those of brought up in the orthodox classical tradition. He noted that what beginners today often regard as trite and obvious was to us puzzling, novel and heretical. He added that Keynes' theory caught most economists under the age of thirty-five with the unexpected virulence of a disease first attacking and decimating an isolated tribe of South Sea Islanders, while older economists [like Schumpeter] were immune.

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 1970's created embarrassments for macroeconomic forecasters who relied upon 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 of the 1970's to the sudden quadrupling of crude oil prices in October 1973 imposed by the Organization of Petroleum Exporting Countries (O.P.E.C.). But some economists chose to ignore the fact that the quadrupling of oil prices had induced pervasive and perverse cost-push inflation, which propagated throughout the nation's transportation system from local delivery trucks to sea-going container ships and thus affected every product that the system carries. Commercial econometric consulting firms addressed this problem by introducing oil prices into their macroeconometric models, a solution mentioned by Haavelmo in his 1944 paper; they had to be pragmatic to retain their clients. These conditions were exacerbated by Federal fiscal

deficits that were relatively large for the time and by the Federal Reserve Board's permissive monetary policies under the chairmanship of Arthur Burns, which stimulated demand-pull inflation. These macroeconomic policy actions became targets of criticism, in which the structural-equation type of models containing such fiscal and monetary policy variables was attacked using the rational-expectations hypothesis.

1995 Nobel-laureate economist Robert E. Lucas (b. 1937) criticized the traditional structural-equation type of econometric model. He was for a time at Carnegie-Mellon, and had come 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 the missing variables responsible for the failure. Curiously, however, in his presidential address to the American Economic Association in 2003, five years before the onset of the Great Recession, Lucas proclaimed that macroeconomics has succeeded, because its central problem of depression prevention has been solved. And in October 2008 with the onset of the Great Recession he is quoted by *Time* magazine as saying that everyone is a Keynesian in a foxhole.

2011 Nobel-laureate Thomas J. Sargent, an economist at the University of Minnesota and an advisor to the Federal Reserve Bank of Minneapolis joined Lucas in the rational-expectations critique of structural models in their jointly authored "After Keynesian Macroeconomics" (1979) reprinted in their *Rational Expectations and Econometric Practice* (1981). They state that Keynes' verbal statement of his theory set forth in his *General Theory* (1936) does not contain reliable prior information as to what variables should be excluded from the explanatory right-hand side of the structural equations of the macroeconometric models based on Keynes' theory. This is a facile statement since Keynes' theory stated what explanatory factors should be included. Sargent furthermore stated that neoclassical theory of optimizing behavior almost never implies either the exclusionary restrictions the authors find suggested by Keynes or those imposed by modern large macroeconometric models. The authors 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 specifically criticism of the structural-equation models based on Keynesian macroeconomic theory. The authors tacitly leave open the possibility that non-Keynesian structural-equation business-cycle econometric models could nevertheless be constructed that 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 a new structural-equation model. They reject the use of expectations measurement data, and proposed a distinctive type of rational-expectations macroeconometric 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 in 1976, 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", which he maintains cannot be observed directly for an individual consumer. He says that only actual income expenditures and receipts during some definite period can be observed, and that these observed measurements are *ex post* empirical data, although verbal *ex ante* statements made by the consumer about his future expenditures may supplement these *ex post* data. Friedman explains that his theoretical concept of permanent income is understood to reflect the effect of factors that the income earner regards as determining his capital value, i.e., his subjective estimate of a discounted future income stream.

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 the **VAR** type of model in contrast to the structural-equation type of econometric model.

Muth does not follow Friedman's neopositivist dichotomizing of the semantics of theory and observation. In his rational-expectations hypothesis he simply ignores the idea of establishing any correspondence by analogy or otherwise between the purportedly unobservable theoretical concept and the statistical concept of expected value, and heroically makes the statistical concept of "expected value" the literal meaning of "psychological expectations." In 1960 Muth published "Optimal Properties of Exponentially Weighted Forecasts" in *American Statistical Association Journal.* He 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 income 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 in production planning and inventory planning by business firms, 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 – and imaginative – economic relevance. Ironically, however, the forecasting success or failure of these models does not test the rational-expectations hypothesis, because they have no relation to the neoclassical theory based on maximizing rationality theses with or without expectations.

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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 all of the variables in the model. This is called the "unrestricted vector autoregression" model. It implements Muth's thesis that expectations depend on the structure of the entire economic system, because all factors in the model enter into consideration by all economic participants in all their economic roles. The **VAR** model dispenses with Haavelmo's autonomy concept, since there is no attempt to identify the factors determining the preferences of any particular economic group, because on the rational-expectations hypothesis everyone 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-equation 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 merely a testing method based upon application of the 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. Therefore the semantics of the VAR model describes the much wider range of factors considered by the economic participants, a range that Simon deems humanly impossible. Rational-expectations thus 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. Sargent also notes 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 principally accurate forecasting.

2011 Nobel-laureate Christopher A. Sims of Yale University makes criticisms of structural-equation models similar to those made by Lucas and Sargent. Sims, a colleague of Sargent while at the University of Minnesota, advocates the rational-expectations hypothesis and the development of **VAR** models in his "Macroeconomics and Reality" in *Econometrica* (1980). 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 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 data, and then develops an understanding of economic structure from simulation analyses with the model. He thus uses **VAR** model interpretation for discovery rather than just for testing.

In the *Federal Reserve Bank of Minneapolis Quarterly Review* (Winter, 1986) Sims states that **VAR** modelers 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 models for policy analysis and evaluation. 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. For this purpose he uses shock simulations with the completed model. But shock simulations admit to more than one structural form for the same **VAR** model, and he offers no procedure for choosing among alternative structures.

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, who has since moved to Wall Street, says that

the original idea to use a **VAR** model for macroeconometric forecasting at the Minneapolis Federal Reserve Bank came from 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** models are not successful. In his "Are Forecasting Models Usable for Policy Analysis?" Litterman noted that the unrestricted **VAR** model is overparameterized, i.e., attempted to fit too many variables to too few observations. This overparameterization of regression models is a well known and elementary error. Avoiding it led to his development of the Bayesian **VAR** model, which 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 ordinary least squares estimation of the coefficients of the model, and the parameters that are the elements in this prior matrix thereby influence the values of the estimated coefficients. This prior matrix is an *a priori* imposition on a model like economic theory in the conventional structural-equation econometric model as described by Haavelmo, because it has the desired effect of restricting the number of variables in the model. But the prior matrix is systematically revised as part of the constructional procedure. Litterman argues that in the construction of structural-equation models the economist rarely attempts to justify the exclusion of variables on the basis of economic theory. 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 than in some *a priori* economic theory. Thus his objective is explicitly discovery by data analysis.

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 degrees of uncertainty are assumed to decrease as the time lags increase. 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 extreme is the multivariate unrestricted **VAR** containing all the variables in each equation of the model. By varying such hyperparameters and by making successive 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 matrix 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 continuum between univariate and unrestricted **VAR** models. Litterman calls this procedure a "prior search", which resembles Simon's heuristic-search procedure in that it is recursive, but Litterman's is explicitly Bayesian. 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 romantic philosophy of science, according to which the language of theory must describe the rational decision-making process in the economic participants' attempts to maximize utility or profits. In other words the semantics of the theory must describe the motivating mental deliberations of the economic participants 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, except that behaviorists do not make economic models. Both views are similar in that they have semantic concepts of theory.

The contemporary pragmatists on the other hand admit any semantics/ontology into theory, but reject all *a priori* semantical and/or 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: according to 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, according to the contemporary pragmatist philosophy of science, Litterman's system is a discovery system, because it produces economic theories, i.e., models proposed for testing.

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 romantic philosophy of science practiced in economics, their accomplishment in creating the **BVAR** model is a radical revolution in the philosophy and methodology of economics, because ironically there is actually no connection between the rational-expectations thesis and the **BVAR** model. Rational expectations play no rôle in the specification of the **BVAR** model. Empirical tests of the model could not test the rational-expectations "hypothesis" even if it actually were an empirical hypothesis instead of merely an economic dogma. And their exclusion of empirical expectations measurement data justifies denying that the model even describes any mental expectations experienced by the economic participants. The rational-expectations hypothesis associated with the **BVAR** models is merely a decorative discourse, a fig leaf giving the pragmatism of the **BVAR** models a fictitious decency for romantics.

The criterion for scientific criticism that is actually operative in the **BVAR** model is perfectly empirical; it is 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. He reports that during the period 1981 through 1984 this **BVAR** model demonstrated superior performance in forecasting the unemployment rate and the real GNP during the 1982 recession, which up to that time was the worst recession since the Great Depression of the 1930's. The **BVAR** model made more accurate forecasts than three leading structural models at the time: Data Resources (DRI), Chase Econometrics, and Wharton Associates (WEFA). However, he also reports that the **BVAR** model did not make a superior forecast of the inflation rate as measured by the annual 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 three large leading structural-equation models named above. 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 that competing structural models had performed better for 1986. Several economists working in regional economics have been experimenting with **BVAR** modeling of state economies. Such models have been 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.

Reports in the Minneapolis Bank's *Quarterly Review* 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, the important changes are those 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 this type of model, but rather is a concession to computational limits of the computer, because construction of the Bayesian prior matrix made great demands on the computer resources available at the time. In contrast the structural-equation models typically contain hundreds of different descriptive variables interacting most often as simultaneous-block-recursive models. Improved computer hardware design will enable the **BVAR** models to be larger and contain more driving variables in the core. 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 feedback information.

Unlike Simon's simulations of historically significant scientific discoveries, Litterman does not separate the merit of his computerized discovery procedures for constructing his **BVAR** models form the scientific merit of the **BVAR** models he makes with his Bayesian-based discovery system. Litterman is not recreating what Russell Hanson called "catalogue-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 some 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 likely 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.

The bottom-line takeaway from the rational-expectations succession of pragmatic modeling experiments in economics is that data-driven model construction can produce more accurate forecasting models than the traditional structural-equation modeling with its presumptuous a priori romantic "theory" that still haunts the halls of academic economics.

Simon, Thagard and Langley

Hickey's Metascience or "Logical Pragmatism"

Thomas J. Hickey was a graduate student in the philosophy department and in the economics department of the University of Notre Dame, South Bend, Indiana. After receiving an M.A. degree in economics and completing his philosophy coursework he intended to develop his computerized discovery system for a Ph.D. dissertation in philosophy. But the Notre Dame philosophers were obstructionist and Hickey got out. Notre Dame has always been better at football than philosophy. After leaving Notre Dame he developed his **METAMODEL** computerized discovery system at San Jose City College in San Jose, California. Today development of such discovery systems is recognized as "computational philosophy of science". For more than thirty years thereafter Hickey used his discovery system occupationally, working as a research econometrician in both business and government. He used his system for Institutionalist macroeconometric modeling and regional econometric modeling for the State of Indiana Department of Commerce. He also used it for Institutionalist econometric and sociodemographic modeling projects for various business corporations.

Hickey described his **METAMODEL** discovery system in his *Introduction to Metascience: An Information Science Approach to Methodology of Scientific Research* (1976). Since publishing this monograph he has also referred to metascience as "Logical Pragmatism", meaning the contemporary pragmatist philosophy of science. His "Logic" is emphatically *not* the irrelevant Russellian "symbolic" logic. More recently the phrase "computational philosophy of science" has also come into use thanks to Paul Thagard. Hickey's intent in using the term "Metascience" is to recognize that philosophy of science is becoming empirical and breaking away from metaphysical foundationalism, just as the modern empirical sciences have done historically. The first half of *Introduction to Metascience* set forth Hickey's the "pragmatist" part of his Logical Pragmatist philosophy. The second half described his **METAMODEL** discovery system, his computational or "Logical" part of Logical Pragmatism, and exhibited his system with a simulation of the Keynesian revolution in economics. His ideas have naturally evolved since *Introduction to Metascience* was published nearly forty years ago. The current rendering of his metascience is very briefly summarized above in **BOOK I** titled "Introduction to Philosophy of Science", and in his e-book *Twentieth-Century Philosophy of Science: A History (Third Edition)*.

BOOK I, which is now also an e-book titled, *Philosophy of Science: An Introduction (Third Edition)* with hyperlinks to this web site.

Logical Pragmatism may be contrasted with the alternative psychologistic approach, which descends from Simon and is exemplified in the more recent efforts of Langley and Thagard. The contemporary pragmatist philosophy of science is in the analytic-philosophy tradition, which originated with the historic "linguistic turn" in early twentieth-century philosophy. In the United States this linguistic-analysis tradition has since evolved considerably into the contemporary pragmatist philosophy of language due in no small part to the writings of Harvard University's Willard van Quine. The contemporary pragmatism supersedes the classical pragmatism of Peirce, James and Dewey. Hickey prefers the linguistic-analysis approach because he believes that the psychologistic approach reveals an inadequate appreciation of the new pragmatist philosophy of language, and he notes that advocates of the psychologistic approach typically include some residual positivist ideas. Furthermore Hickey's metascience agenda with its computerized linguistic constructionalism makes no claims about representing human psychological processes. Thus no experiments are needed to validate any psychological claims associated with the computer-system designs. In fact the computational philosopher of science need not understand the intuitive human discovery process, in order to produce a system 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. Computational philosophy of science more closely resembles computational linguistics than psychology.

Hickey's Linguistic Analysis

Hickey's contemporary pragmatist philosophy of language is detailed above in **BOOK I** in this web site.

Hickey's Functional Analysis

Hickey organizes philosophy of science into four functional topics: the aim of science, discovery, criticism and explanation. His pragmatist philosophy of science is detailed above in **BOOK I** in this web site.

Hickey's METAMODEL Discovery System

Hickey's **METAMODEL** discovery system antedates Simon's applications of his problem-solving theory of heuristic search to the problem of scientific discovery by about ten years. Initially Simon did not apply artificial-intelligence systems to scientific discovery. Hickey developed an original combinatorial generate-and-test design that differs from the heuristic-search design used by Simon and his colleagues at Carnegie-Mellon or by their later followers including Langley, Zytkow and Thagard. The second part of his *Introduction to Metascience* 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** performed revisionary theory construction of Keynes theory, an episode now known as the "Keynesian Revolution" in economics. The applicability of the **METAMODEL's** revisionary theory construction is already known in retrospect. As 1980 Nobel-laureate economist 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 Keynes' predecessors.

Hickey firstly translated Keynes' theory into mathematical form. His translation was informed by J. R. Hicks' in "Mr. Keynes and the Classics" in *Econometrica* (1937). In the *Journal of the History of the Behavioral Sciences* (1979) Walter A. Sedelow, professor of computer science and sociology, and Sally Y. Sedelow, professor of computer science and linguistics, both at University of Kansas, wrote that Hickey's mathematical explication of Keynesian theory reveals a useful way of formalizing the history of science. And they add that Hickey shows how the history of science in the course of such formalization may contribute to the enhanced effectiveness of science itself by means of computer-implicated procedures.

The **METAMODEL** performs an extensive cognitive exploration of the revisionary theory-constructional possibilities that are latent in the system's input state description. The principal disadvantage of this combinatorial generate-and-test design is its extensive utilization of computer resources. On the other hand the principal advantage is that unlike heuristic search and others that are more efficient, it minimizes the risk of preemptively excluding theories that are worthy of consideration. Some employers have allowed Hickey unlimited mainframe computer resources

after demonstrating successful computer runs for market analysis. The system is not a satisficing system, but rather is an optimizing system severely constrained by several statistical testing criteria that outputs a small number of constructionally generated and empirically tested theories. As computer hardware technology continues to improve (e.g., supercomputing, quantum computing) the trade-off between efficiency and thoroughness will move far toward thoroughness.

The discovery system's inputs and outputs are called "state descriptions". To simulate the Keynesian revolution with the **METAMODEL** Hickey developed a cumulative input state description containing the descriptive variables in the object language. The input state description also contained the measurements for the associated time-series historical data. He researched the literature of the economics profession that pertains to the trade cycle problem for the interwar years prior to 1937. The American Economic Association's *Index of Economic Journals* was a useful bibliographic source. The examination of the relevant professional literature yielded ten economic theories of the national trade cycle, which Hickey also 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 highly redundant, and yielded a set consisting of eighteen unique variables.

The data for these variables are annual time series for the period 1921 through 1934, which were available to economists in 1936, the year Keynes' book was published. The selected time series measurement data were originally published prior to 1937 in annual issues of the U.S. Department of Commerce *Statistical Abstract* and in the U.S. Department of Commerce *Historical Statistics of the United States* (1976). The input state description contains these time series data converted to index numbers of period-over-period change ratios to minimize collinearity together with variable symbols including one time lag.

The output state description contains an econometric model of Keynes theory constructed by the discovery system. The original 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

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seven equations with three exogenous variables.

Operating the **METAMODEL** requires 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 in the input state description are the problematic variables, i.e., those that identify the problem the theory is to solve. 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 contains these two problematic variables and equations determining their numeric values.

Secondly the user must designate which among the current-valued variables are exogenous variables. These variables have their values inputted to the system and not generated by it, because the values were determined independently by economic policy decisions of political authorities. The three exogenous variables designated in the trade cycle application are real aggregate Federal fiscal expenditures, real aggregate Federal 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, which is the first record read when the system is run. Records containing the character symbols of the input variables with separate identifiers for current values and lagged-valued variables follow the control record, which in turn is followed by the data records.

The **METAMODEL** discovery system is a **FORTRAN** computer program with an architecture consisting of a main program, **SLECTR**, and two subroutines named **REGRES** and **SOLVER.** **SLECTR** is the combinatorial procedure that selects nonredundant combinations of language elements. The system has a control switch, which is initialized as open. When its control switch is open, **SLECTR** selects combinations of time series from the input file initially read by the system. For each selection if it had an unsatisfactory triangular correlation matrix for the equation's independent variables then control is returned to **SLECTR** for another selection. Otherwise it calls **REGRES,** which 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

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SLECTR. If the estimated equation does not have a satisfactory R^2 coefficient-of-multiple-determination statistic associated with it as well as a satisfactory Durbin-Watson statistic and satisfactory Student t-statistics, control is returned to **SLECTR** for another selection. But when all these statistical criteria are satisfied, the equation and its statistics are stored as a record in an interim accumulation file, and control is returned to **SLECTR** for more selections.

With its switch closed **SLECTR** makes nonredundant selections of sets of estimated equations from the accumulation file generated by **REGRES.** For each selection it calls subroutine **SOLVER,** which solves each multi-equation model with the Gauss-Jordan simultaneous-equation algorithm, and then executes the model to generate a reconstruction of the historical data. In order to accomplish this, there are certain criteria that every selected set of equations must satisfy, and **SOLVER** checks for four 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 just as many current-valued endogenous variables as there are equations. *Thirdly* the model must be recursively executable to generate a time series, 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 except for the problematic variables it contains no current-valued variable that is not needed to evaluate a lagged-valued variable describing the same phenomenon.

When **SOLVER** finds an equation set that does not satisfy all these criteria, it returns control to **SLECTR** for another set of equations. Models that do satisfy all these criteria are capable of being solved, and **SOLVER** then solves and recursively iterates the model both to recreate the history with synthetic data for the years 1921 through 1933. The simulation must capture all the critical points in the time-series history. If it does this, it then must 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 retrodictive out-of-sample 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 a file for printed display in conventional mathematical form with each equation listed together with its associated statistics. The output lists the synthetic data generated by the iteration of the model with the forecast values for its endogenous variables.

Four years after designing and testing his **METAMODEL** discovery system with the simulation of Keynes' macroeconomic theory, Hickey had occasion and opportunity to use the system to address a contemporary problem. At that time he was a senior economist in the Analysis and Statistics Bureau of the Finance Department of United States Steel Corporation. He had completed a conventional Keynesian quarterly macroeconometric forecasting model using Haavelmo's procedures, but found that the model did not perform satisfactorily. This occurred during the years following the large increase in crude oil prices imposed by the Organization of Petroleum Exporting Countries (OPEC), and no macroeconometric models available at the time had the consequences of this unprecedented shock in the sample data available for statistical modeling. Many economists reacted to the structural breakdown of their models with patience, and updated their databases as new data became available and revised their models. Others, however, believed that more than oil prices were at fault, and that there are more basic reasons for dissatisfaction with their Keynesian models. One such group was the rational-expectations economists, and they had their distinctive agenda, as described above.

Hickey also believed that more was involved than inadequate sample data. But unlike the rational-expectations advocates, he views structural breakdown in the same manner as did Haavelmo, who maintained that the problem is remedied by introducing into the model new variables for missing factors, the absence of which had caused the breakdown. Initially this suggests a theory-elaboration approach. But unlike Haavelmo, Hickey agrees with the Institutionalist economists like Mitchell 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 prefers the explicitly pragmatic orientation of the American Institutionalist economists, who were influenced by the classical pragmatists.

However, historically Institutionalists did not make econometric models. Even today most of them are more interested in the historical evolution of economic institutions. Hickey ventured beyond conventional Institutionalism and decided to integrate functionalist sociology into his econometric model, even though functionalist sociologists do not make econometric models either. Functionalism in sociology is an equilibrium thesis that all institutions of a national society are interrelated. Therefore he

used his **METAMODEL** discovery system to investigate how variables representing each of five basic institutions of the American society can be related by statistically estimated equations of the type used in econometric models.

The discovery system generates many alternative equations and models that are empirically acceptable thus exemplifying the contemporary pragmatist's thesis of empirical underdetermination of language and the thesis of scientific pluralism. For romantic philosophers of science this is an argument against development of hypotheses by data analysis, and thus an argument for invoking some prior semantics/ontology with its preconceived concepts of causality. But for the contemporary pragmatist, pluralism is simply an inevitable fact of life in basic-scientific research routinely encountered in the history of research science. Einstein had called this pluralism an "embarrassment of riches."

Hickey used his **METAMODEL** to make macrosociological models with eleven current-valued input variables with each 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. 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.

The functionalist macrosociometric model generated by the **METAMODEL** was used as a guide for integrating sociological, demographic, and human ecological factors into an integrated model of the U.S. national society for the Indiana Department of Commerce. A description of the resulting integrated macromodel was published in "The Indiana Economic Growth Model" in *Perspectives on the Indiana Economy* (March, 1985). Later in the September 1985 issue of the same publication 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 **METAMODEL** and compared it with some **VAR** models and with the **BVAR** system constructed by the rational-expectations advocates.

In addition to using his system for the State of Indiana Department of Commerce, Hickey has used a commercial version of the **METAMODEL**

system for many other Institutionalist econometric and sociodemographic modeling projects for various business corporations including USX/United States Steel Corporation, 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 were 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 large 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. 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

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Institutionalist economists like Hickey have exploited financially.

Hickey also used his **METAMODEL** system to develop a macrosociometric Institutionalist model of the American national society with fifty years of historical time-series data. From 1978 to 1982 Hickey submitted a paper describing his macrosociometric model developed with his **METAMODEL** system to four sociological journals. The paper was acceptable on empirical grounds. But to the chagrin and dismay of academic sociologists it is not a social-psychological theory. Hickey was unable to break through the sociologists' obstructionist complacency barrier, and all of the journals rejected the model for publication. The paper is reprinted in **Appendix I**, and the referees' critiques and Hickey's rejoinders are in **Appendix II**. **Appendix III** is a critique of the sociological literature.

Hickey describes his macrosociometric model as a "post-classical" functionalist theory. The term "classical" when applied in a science is not a proper name for an historical period like "mediaeval". It is better described as the name for a style of thought or more precisely for analyses using certain basic premises. It is a relative term like "liberal" or "conservative", which change with shifts in the political spectrum. Yesterday's liberal has often become today's conservative. Likewise in science "classical" refers to the immediately preceding view that has been superseded by a new and current one due to a scientific revolution. Furthermore "classical" cannot be assigned to an historical period, because like an artistic style its residual characteristics often linger about for many decades. In economics "classical" originally referred to pre-marginalist economists, but Keynes referred to the marginalists economists that preceded his macroeconomics as "classical". The classical premises he rejected included Say's Law, which says that supply creates its own demand, and the full-employment equilibrium outcome of the optimum allocation of resources that was postulated by relative price theory, later known as microeconomics. Likewise in physics Neils Bohr referred to "classical physics" that included relativity theory as well as Newtonian physics but which preceded the Copenhagen quantum theory. The premise of classical physics included determinism whereas the indeterminacy equations of quantum theory are stochastic.

Similarly Hickey uses "classical" to describe sociological thought in same the manner that sociologist Donald Black used it in his address to the American Sociological Association in 1998 reported in his "The Purification

of Sociology" article in *Contemporary Sociology*. Black stated that sociology is classical, because its explanations of social behavior are (1) teleological, i.e., in terms of means and goals, (2) psychological, i.e., in terms of subjective mental motivations, and (3) individualistic, i.e., in terms of individual persons. Black proposed a scientific revolution in sociology in the manner described by Thomas Kuhn, and notes that sociology has never had such a revolution in its short history. In his macrosociometric modeling Hickey dispenses with all three of these premises of classical sociology. Hickey refers to the romantic sociology with its social-psychological reductionism as "classical", because his macrosociological quantitative functionalist theory supersedes the prevailing social-psychological reductionism, and manifests a basic discontinuity in sociological thought as evidenced by the criticisms by orthodox journal referees, who recognize Hickey to be a heretic.

Hendry and Doornik's AUTOMETRICS Discovery System

In the "Introduction" of their *Empirical Model Discovery and Theory Evaluation: Automatic Selection Methods in Econometrics* (2014) David F. Hendry and Jurgen A. Doornik of Oxford University's Program of Economic Modeling Institute for New Economic Thinking write that automatic model selection has "come of age." Indeed computational philosophy of science is the future that has arrived, even if it is called by other names as practiced by scientists working in their special fields instead of philosophy or cognitive psychology.

But the news has been slow to get around. For example in April 2009 the journal *Science* reported that robotics engineer Hod Lipson of Columbia University and computational biologist Michael Schmidt of Cornell University's Creative Machines Lab had created a symbolic regression and genetic algorithm that they call the "**Eureqa Machine**" (pronounced eureka). Their computer system found invariants in the motion of the double pendulum and outputted Newton's second law of motion, **F = ma**, in just a few hours of run time. It was later given data on yeast cells and developed equations that made highly original and successful predictions that do not relate to existing knowledge in microbiology. The achievements were also reported in the *Guardian*, which naïvely announced that **for the first time** a machine has independently made scientific discoveries. The *Guardian* reporter was blithely oblivious to the several routinely functioning discovery systems developed over the last fifty years.

Hendry was head of Oxford University's Economics Department from 2001 to 2007, and is presently Director of the Program of Economic Modeling Institute at Oxford's Martin School. Doornik is a colleague at the Institute. These authors explore mechanized determination of the equation specifications for econometric models with their automated computer system **AUTOMETRICS**, which is contained in their *PcGive* software package. The authors' automatic model selection takes econometrics beyond the Haavelmo agenda, which viewed econometrics as merely empirical testing of economic theory. In their summarizing "Epilogue" the authors write that much of the effort of an empirical study is devoted to theorizing about the relevant joint density to explain the economic behavior of interest, selecting the measured variables, incorporating the historical and institutional knowledge of the epoch, and building on previous empirical findings. But they add that without unjustifiable assumptions of omniscience, these steps are insufficient, and they maintain that empirical model discovery inevitably requires search outside the pre-existing framework.

Hendry and Doornik state that an automatic program can outperform experts in formulating models when there are many candidate variables, possibly long lag lengths, potential non-linearities, outliers, data contamination, or parameter shifts of unknown magnitudes at unknown points of time. It also outperforms manual selection by its ability to explore many search paths and thus handle many variables, yet have high success rates. Furthermore despite selecting from a large number of candidate variables, an automatic selection method can achieve desired targets for incorrectly retaining irrelevant variables, and still deliver near unbiased estimates of policy relevant parameters. They call their **AUTOMETRICS** discovery-system design a "structured path search", which is controlled by a variety of model selection criteria. Their structured-path-search design is more efficient than a combinatorial approach. Like Simon, they maintain that a combinatorial design is too extensive to be feasible, although they observe that feasibility is in conflict with generality.

Hendry and Doornik's aims are modest and conservative; they express no plan or expectation to revolutionize theoretical economics. They write that empirical model discovery aims to provide an extension of and improvement upon many existing practices in applied economics, but add that it is not a replacement for analytical reasoning or theory, which they say offers too many crucial insights to be sidelined. But they also note that it is unwise to *impose* today's theory on data, because tomorrow's theory may be

more complete and different, and new theory may lead to earlier theory-based evidence being discarded. Thus they say that their strategy is for available theory to be "embedded" in the modeling exercise, to be retained in its entirety when it is complete and correct, while at the same time by including a far larger number of candidate variables they allow for the possibility that aspects absent from an abstract theory can be captured. They suggest that embedding both Friedman's monetarist theory and Modigliani's Keynesian theory in a general model would have allowed a rapid resolution of the disagreement, perhaps with neither having the complete answer. Hendry reports in his "Modeling UK Inflation, 1875-1991" in the *Journal of Applied Econometrics* (2001) that almost every theory – excess demand, monetary, cost push, mark-up, imported, etc. – played a rôle, but even when combined they failed to account for many of the major episodes of inflation and deflation experienced historically.

Parsons' Romantic Sociology

Given the demonstrated importance of Institutionalist economics and functionalism, let us turn next to 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, firstly by the physicists and then eventually by the philosophers of science. But sociology has had no advancements even remotely comparable to physics, and its oppressively conformist peer-reviewed literature has made it sclerotic. This is because instead of practicing the contemporary pragmatism, as did the physicists with the development of quantum mechanics, sociologists merely reworked both positivism and romanticism, which philosophers of science today view as anachronistic.

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 academic sociology. The first and more influential of these is the romantic sociologist, Talcott Parsons of Harvard University. Parsons' romantic philosophy of science is very uncongenial to such modern ideas as computerized discovery systems, but his philosophy is still widely practiced and enforced by the editors and their chosen referees of the periodical literature of academic sociology. This overview of sociology's romanticism

is included here to explain contemporary sociologists' rejection of quantification and their Luddite hostility to mechanization.

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 then studied under the anthropologist Bronislaw Malinowski at the London School of Economics. Parsons received his doctorate from the University of Heidelberg University, where he was influenced by the views of Max Weber of Heidelberg, even though Parsons had 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 tome that examines the sociologies of four writers: Alfred Marshall, Vilfredo Pareto, Emile Durkheim, and Max Weber.

This *magnum opus* is as much an historical study in philosophy of social science as a study in sociology. 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 social theory that Parsons calls the "voluntaristic theory of social action", a type that is unmistakably romantic. This sociological theory 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 Max Weber, whose social theory and *verstehen* philosophy of scientific criticism is represented in Parsons' work as a later phase in the 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 academic sociologists.

Weber's *verstehende soziologie* starts with the concept of "action", which Weber defines as any human attitude or activity, to which the participant or participants associate a subjective meaning. "Social action" in turn is action that according to its subjective meaning to the participants involves the attitudes and actions of others, and is oriented to them in its

course. Finally, sociology is the science that attempts an empathetically based "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 motivations, which Weber defines as a meaning complex that to the participant or to the observer appears to be an adequate ground for the participant's attitudes or actions. A correct causal interpretation of action is one in which both the objective course and the subjective motive are correctly grasped, and their relation to each other is "understandable" to the sociologist as *verstehen*. The object of *verstehen* in Weber's methodology is to uncover the **motivations** that are the causes of social 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 participants, and it defines the term "theory" in social science exclusively as language describing this ontology. *Secondly* it requires the *verstehen* or empathetically 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 references subjective ontology but is not required to satisfy the *verstehen* criterion, because the *verstehen* explanations based on the social scientist's personal experience or empathy have been known to differ widely from one social scientist to another. The romantic social scientists that accept the weaker thesis deny that the social scientist should have to find an explanation convincing by reference to his own personal or imaginatively vicarious experience.

Historically the philosophy of science that evolved in reaction against the romantic philosophy is positivism, which requires exclusion of the subjective experience required by romantic philosophy. Positivists either redefine the meaning of "theory" to exclude any such mentalistic semantics or just forbid all theory. On the other hand the contemporary pragmatist philosophy of science rejects the thesis common to both the romantic and the positivist philosophies that either semantical or ontological considerations may operate as criteria for criticism, and it defines "theory" by its function in empirical testing rather than by any reserved semantics or ontology.

Weber's philosophy of social science is a variation on the distinction between natural science and social science that originated with the Kantian philosophical idealism and that gave rise to the Hegelian and historicist views of explanation. In explicit contrast to the German Historicist school, 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 those others, *i.e.*, by "interpreting" their actions and words as expressions of motives. He maintains that social scientists similarly use their access to this subjective aspect of human action, and furthermore that this access carries immediate evidence or certainty.

In Weber's view, therefore, the natural and social sciences differ in that the former rely on observation of external regularities or *begreifen*, while the latter have the benefit of the introspective subjective knowledge of subjective motives, i.e., *verstehen*, which are not present in the phenomenal sense data of events considered in natural science. Weber thus postulated different aims for the natural and social sciences: The aim of natural science is the formulation of universally applicable general laws, while the aim of social science is the *verstehen* 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.

Also in Weber's view there is selectivity that every scientist brings to his subject, which is determined by the interest of the scientist. Specifically the basis for selectivity is the relevance of the subject matter to the values of the scientist. Furthermore 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 place 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. Thus it may be said that Parsons anticipated in important respects the contemporary pragmatist semantical theory of observation two decades before the pragmatist philosophers took it over from the physicists. Parsons says that the principle of value-relevance applies both to natural and to social sciences, and that both use laws therefore making both natural and social sciences analytical instead of

historical sciences.

While Parsons may have anticipated the contemporary pragmatists' philosophy of observation, he had nothing like their metascience of criticism. He notes that for Weber *verstehen* is not just a matter of immediate intuition, and that Weber subordinates the immediate evidence from *verstehen* to other considerations: *verstehen* must be "checked" by reference to a logically consistent system of concepts. Parsons says this 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. This is a coherence concept of criticism. Ironically the reverse practice prevails in contemporary sociology today, because the sociologist judges the conceptual scheme in terms of *verstehen* acceptability, *i.e.*, what the particular sociologist happens to find intuitively "convincing".

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 on the one hand and the empirical facts that existed before the historical event on the other. 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 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" by his *verstehen* criterion. Parsons explains that they must adequately describe a potential concrete entity, an objectively possible case, in terms of the action frame of reference. Parsons says that there are two types of scientific laws involved in this process, both of which may occur in either the natural or the social sciences. They are empirical generalizations and analytical laws. The problem of adequate causal explanation in social science is one of imputing causes to make empathetically based analytical laws.

There remains the problem of the relation of empirical generalizations to analytical laws. The empirical generalizations are judgments of probable behavior under given circumstances of the type elements. The analytical laws are statements of general modes of interaction among the type elements

known by *verstehen*. 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 general laws that relate these elements may be either empirical generalizations or analytical laws. 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 he adds that 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 is not adequate, it may become necessary to resort to more generalized theoretical categories and laws. In the progression from empirical generalizations to analytical laws to more general analytical theory, the less general statements are not dispensed with, but the analytical laws serve as a "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.

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 that requires all realistic scientific theories to be reduced to one if they are not to be regarded as fictional. Parsons therefore proposes his own ontological thesis, which he calls "analytical realism", in which the general concepts of science are not fictional but adequately grasp aspects of the objective external world. This suggests what some earlier philosophers had called this "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 mistakenly in Parsons' view regarded as fictional. Parsons furthermore rejects any reductionism in the relation between natural and social sciences, and explicitly affirms the thesis of emergent properties. This emergentism is the consequence of value relevance, and it is the basis for his frame-of-reference thesis and his architectonic for the sciences.

Parsons identifies three reference frames in his architectonic for the sciences that he calls the three great classes of theoretical systems. They are

the systems of nature, the systems of action, and the systems of culture. He 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. Parsons says that he chooses not to consider the cultural type in his *Structure of Social Action* (notwithstanding that his book itself is a history of the evolution of philosophy of social science, a blatantly cultural subject). 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.

Parsons claims that the action frame of reference is fundamental to the 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 the defining emergent properties characteristic of each. 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 evolving 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.* This property is emergent, because an attempt to analyze the system further results in its disappearance. Neither economic rationality nor common-value integration is a defining 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 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 due to 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 *ad hoc*. 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 in fact it is not consistent with Parsons' perspectivist analytical realism which he opposes to monistic realism. Parsons is unprepared to accept the contemporary pragmatists' ontological relativity and scientific pluralism, because he incorrectly believes 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 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 vicariously based imputation of motivations for social action. In the *Social System* he calls this implementation of *verstehen* "motivational analysis", which he also calls "dynamic analysis". Motivated behavior is action that is oriented to the attainment of gratifications or to the avoidance of depredations according to the participant'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. **He defines an institution as** *a cluster of interdependent rôle patterns, which are integrated into the personalities of the social members by motivational processes or "mechanisms" called socialization. And tendencies to deviance from these rôle 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 rôles and institutions,

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and these structures in turn operate to satisfy the functional prerequisites of the whole social system. Parsons identifies four basic institutional rôle 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 rôles and the stratification of the society, (3) politics, which functions to organize the rôles pertaining to power, force, and territoriality, and (4) religion, which functions to integrate value orientations, cognitive orientations and personality. Parsons refers to his sociological theory as "structural-functional". The motivational dynamics induces voluntary conformity to prevailing rôle patterns and thereby produces a tendency to social equilibrium by changes within the existing structures of the social system.

But there are also changes of the structures of the social system itself, which are 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. Thus Parsons 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. 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 motivational analyses, and may not merely be a system of empirical generalizations.

Parsons had failed to follow through on his emergentist thesis for social sciences. Classical economists like Schumpeter had demanded a macroeconomics that is an extension of economic psychology – the "mechanisms" of the rationality psychology. But after Keynes macroeconomic theory economists recognized that macrolevel analyses cannot successfully be reduced to microlevel analyses, because it incurs the logical fallacy of composition. In Parsons' terms, macrosociology is "emergentist" and cannot succeed as a reduction to an individualistic microlevel social psychology of motivational analysis. Therefore motivational analyses of members cannot explain the behavior or outcomes of the whole social system; this requires the system of empirical generalizations – a model – that Parsons could not accept.

Habermas on Weber

Weber's problematic views on the aim(s) of social science have continued to exercise social scientists as well as 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 of the Frankfurt school of social thought discusses an ambiguity in Weber's literary corpus about the problem of irrational purposeful action. 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 participant chooses means that are adequate to realize his motivating purpose, the sociologist can grasp the participants' meaning and motive from the participants' behavior, and relate the behavior to its outcome in valid empirical explanations. But when the social participant's choice of means is not effective and therefore not "rational", the sociologist may be able to observe an explanatory empirical uniformity between observed behavior and observed outcome, but may not be able to impute a valid interpretative understanding. This is an unsolved fundamental problem for the romantic philosopher of social science.

Habermas notes that in *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 the validity of empirical explanations that lack interpretative understanding, and he says that the rôle of interpretation of subjective meaning is merely to open the way to empirical social facts. Thus Habermas states that in the context of the controversy about value judgments Weber subordinates the requirement for interpretative understanding to empirical explanation.

On the other hand Habermas observes that in another context 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 character of

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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 them understandable, which he says is autonomous from the empirical investigation. Weber had a positivist idea of the natural sciences, but his ambiguity about method principally originates in the conflicting aims of social science as both empirical and 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 for the social sciences. But irrational purposeful action is not exceptional. Social participants routinely fail to realize the consequences of their motivated actions, because they intend other consequences. In his *Realism and the Aim of Science* Karl Popper defines social science as the study of the unintended consequences of social behavior. Similarly the 1974 Nobel-laureate economist Frederich A. von Hayek of the Austrian School of economics, a romantic, similarly recognized the importance of unintended outcomes in his thesis of "spontaneous social order". In his *Counter-Revolution of Science* he states that the very task of social science is to explain the unintended social regularities in the social order, ***because otherwise there would be no need for social science*.** Social science would be reduced to merely an extension of social psychology.

Merton's Critique of Parsons

Robert K. Merton (1910-2003) has an insightful way of addressing the problem of unintended consequences. He studied 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 not the focus of this history of twentieth-century philosophy of science.

Here the focus of interest is given in Merton's magisterial *Social Theory and Social Structure* (1949, 1968), where he departs from Parsons'

romanticism with his own rendering of functionalist explanation in 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 disclaims having 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 which he means developing a "paradigm" for it. He notes that some sociologists may 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 is the bane of the romantics, who can only treat him dismissively. This is because he maintains 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. Thus 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, because the subjective disposition may but need not coincide with the objective consequence; the two may vary independently.

This objective 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 also 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, but 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 other authors independently. For example William H. McNeill, who is not a sociologist but is a historian of medicine, illustrates what sociologists would call "latent functions" in his *Plagues and Peoples* (1977), an historical study

in epidemiology. McNeill writes that a large-scale outbreak of bubonic plague, also known in earlier Europe as the "Black Death", had occurred in Manchuria in 1911. Investigators discovered that the disease had been contracted from marmots, which are large burrowing rodents with pelts that commanded a good price on the international fur market. The indigenous nomad tribesmen of the steppe region, where these animals live and are hunted, 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. Therefore 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 immediately strike its tents and move away to avoid misfortune. Such customary practices and proscriptions reduced the incidence of human infection with plague to minor proportions. But in 1911 inexpert Chinese migrants, who knew nothing of the tribesmen's "superstitions", hunted the marmot for their furs, trapping both sick and healthy animals. Thus plague broke out among the Chinese and then spread along the new railroad lines of Manchuria. In Merton's terms the manifest function for the native nomads, which is a superstition, is the proper treatment of possibly reincarnated ancestors, while the latent function is a hygienic hunting practice that protected the indigenous hunters from the contagion.

In his "The Unanticipated Consequences of Purposive Action" in *American Sociological Review* (1936) Merton noted five contributing factors that produce unintended outcomes. They are (1) ignorance of the nature of the relevant conditions, (2) error in selecting the appropriate course of action, (3) the primacy of immediate interests, (4) the ideological imperative of basic values, and (5) the self-fulfilling prophecy. The failure of the Chinese hunters includes (1) their ignorance of the presence of the plague contagion and (2) hunting unhealthy marmots.

Merton describes heuristic purposes for his distinction between manifest and latent functions. The distinction not only precludes confusion between motive and function, which he emphasizes 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", but that persist even though their intended 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. This is clearly more sophisticated than the *verstehen* requirement that hypotheses based on the sociologist's empathy so they are common sense explanations.

Furthermore Merton 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: ***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 during change, is to indulge in social ritual rather than social engineering.

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. Its explanatory value consists not in a functional factor being either manifest or latent but in its being consequential for the larger structures in which the functions are contained, regardless of whether or not the social consequences are either recognized or intended by the social participants. 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 occasionally as a heuristic device for developing functionalist hypotheses.

Merton's attack on Parsonsian sociology is not a frontal assault on romanticism, but is part of his agenda for sociological research. His 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 and the Parsonsian-like all-inclusive systems.

Unlike romantics, who define theory in terms of the semantics of a vocabulary referring to subjective meanings and motives of social participants, 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. This is a positivist view. 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. Merton distinguishes theory from the empirical generalization saying that the latter is an isolated proposition summarizing observed uniformities of relationships between two or more variables. As it happens, 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 Benjamin Lee Whorf, Merton also notes that his conceptual apparatus fixes the empirical researcher's perceptions, 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 actually isolated semantically. His distinction is therefore unsustainable. Had he approached this problem by an analysis with the contemporary pragmatist 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, because semantics, logical syntax, and belief are interdependent.

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 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 in the early post-World War II era. A contemporary philosopher of science would likely view this paper as an effort to deromanticize neoclassical economics. Although this paper sets forth a somewhat naïve semantical thesis, its semantical metascience is more sophisticated than the neopositivist view in Friedman's *Theory of the Consumption Function*. In the *Essays* his phrase "positive economics" 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 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. He notes that businessmen do not actually calculate marginal cost or marginal revenues or 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 he says that businessmen must act *as if* they have compared marginal costs and marginal revenues, because they will not succeed in business if their behavior is not consistent with the theory of rational and informed maximization of profit. In philosophers' terms, this means the economist is not a romantic examining what the entrepreneur thinks, but is a pragmatist examining the consequences. 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 for profit maximization per neoclassical microeconomics.

Lundberg's Positivist Sociology

Parsonsian romanticism has not been without its critics. And not surprisingly the science that was founded by the founder of positivism, namely Auguste Comte, has also spawned positivist critics to oppose Parson's romanticism. A principal protagonist in this critical rôle, who was a 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 Parsonsian path for several decades thanks in no small part to Parsons' advantage of the prestigious forum of Harvard University. The following brief rendering of Lundberg's criticism reveals the philosophy which for many years American academic sociologists viewed as their philosophical alternative to Parsons.

Lundberg explicitly traced 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*, which he says suggest the principal survivals from Comte's work that he regarded as contemporary positivism in sociology. The *first* quotation is a statement of the principal aim of science, which 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 thus places himself at variance with Parsons, and he quotes antipositivist comments from a lengthy footnote in Parsons' *Structure of Social Action*, in which Parsons states that all positivisms are untenable.

Lundberg's 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). Later the 1964 edition of the *Foundations* monograph contains an "Epilogue" as a new chapter, in which Lundberg maintains that the Parsonsian 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? (1947, 1961) and in 1953 he co-authored *Sociology*, a textbook with a methodological discussion of his views.

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 of 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 like the Vienna Circle Positivist philosopher and sociologist Otto Neurath, Lundberg discovered that 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 Pickwickian positivism.

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 subjectivist view of knowledge occasions the question of how the human mind knows the external or extramental real world. One naïve answer to this problem is the copy theory of knowledge, according to which the ideas reveal reality, because they are copies of reality. Another is to deny the external world of material substances, and the result is a solipsistic idealism 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 ersatz version. 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 thus nonrealist, because it makes subjective experience instead of extramental reality the 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 everything from these responses. His positivism thus acknowledges

extramental realities beyond the known symbols, even if reality is not the object of knowledge. Furthermore he claims that this "inference" is not deductive, but consists of operational 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 romantic 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. He says that 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.

Curiously Lundberg's version of positivism includes rejection of the naturalistic philosophy of the semantics of language. In discussing measurement he rejects any distinction between natural and artificial units for measurement, and 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. This 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. Social behavior is not easily described in terms of phenomenal shapes, colors, sounds, or other purportedly elementary sense data. 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 referencing subjective experience as opposed to observably objective behavior. His philosophy of the semantics of language does not admit his subjective/objective distinction.

Simon, Thagard and Langley

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 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. He says that something is explained or understood, when the situation is reduced to elements and to correlations among the elements, which are so familiar that they are accepted as a matter of course, such that curiosity is then put to rest. And he defines an "element" as any component that is not in need of further explanation. 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 habitual 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 made 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 the scientist seeks an explanation, he seeks to associate data reporting the problematic experience with his familiar knowledge as described by his established habitual 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. But Lundberg replies 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?* he further comments about the word "motives" in relation to frames of reference. He says that "motives" is a word used to designate those circumstances to which it seems reasonable to attribute a behavior, 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. He opposes this type to those 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 *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. Dodd was chairman of the Sociology Department at the American University in Beruit, Lebanon. Lundberg takes Dodd's work to be exemplary of the natural science approach in sociology, and 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 book and its sequel, *Systematic Social Science: A Dimensional Sociology* (1947), describe 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 any transformation, as are mathematically expressed theories in natural science. American sociologists did not accept Dodd's unworkable *S*-theory.

Parsons and Lundberg Compared

Parsons and Lundberg offer surprising ironies in their ersatz philosophies of science. Each for reasons of his own surpassed the naturalistic thesis of the semantics of language that is commonly found in 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 an 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 relative to the academic philosophers preoccupied with the philosophy of physics. Unfortunately, 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 realism.

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 operational definition is a more contemporary epistemology, 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 operational definitions. Operational definitions are not inferential, do not establish realism, and 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 and/or by the processes of perception. 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. Thus Parsons' philosophy of science is truly postpositivist, as he had claimed, and it suggests the contemporary pragmatist theses of relativized semantics and ontological relativity.

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 the social scientist must impute from 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 cannot 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 *ad hoc* and inconsistent with the artifactual-semantics thesis for language.

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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. Parsons' claim is erroneous because 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." Popper also expressed this by saying that observation is "theory-impregnated." And 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 contributed to the intelligibility for the observed 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 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. But on the pragmatist view it is the whole theoretical system of beliefs including the *verstehen* analytical laws that is subject to being "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 the natural sciences. 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 semantical and ontological criteria for scientific criticism. On the contemporary pragmatist philosophy of science only the empirical criterion may operate in scientific criticism. The artifactual-semantics thesis makes all ontologies as dispensable as the empirical theories that describe those ontologies, and it makes all theories subject only to empirical criticism without regard to how improbable or counterintuitive the empirically adequate theories may seem.

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Sociologists are inevitably naïve philosophers of science. But Lundberg was more than naïve; he was muddled. Given the negligible track record of sociology as a science, his motivation for adopting positivism to model sociology on the natural sciences is understandable. But his Pickwickian positivist philosophy of science is not understandable in the sense of excusable. Positivism was a poor choice from the outset. Its epistemology confronts the scientist with abundant pitfalls, and the more deeply Lundberg became ensnared, the more convoluted and muddled became his efforts to escape them. He succeeded only in obscure and incoherent eclecticism. Nonetheless his proposal for functional equations relating measurements places him ahead of his time and in this respect superior to the romantics both then and now, who are preoccupied with social-psychological "mechanisms" consisting of motivational analyses.

The METAMODEL System Applied to Sociology

The acid test of a theory is prediction, and for a discovery system prediction is production beyond the current boundaries. Thus Hickey tested his **METAMODEL** discovery system by applying it to contemporary sociology, to predict and thereby to produce a new sociological theory that is empirically superior to any currently available. In 1976, three years after he developed his **METAMODEL** discovery system, Hickey used the system to develop a functionalist macrosociometric theory of the American national society estimated from historical time series describing fifty years of American history. He submitted his pragmatist project as a paper setting forth his macrosociometric model and his findings to four sociological journals, all of which rejected the paper. The paper, "A Quantitative-Functionalist Theory of Macrosocial Change in the American National Society" is reproduced in "**Appendix I**". The paper was rewritten in the successive submissions, but the **model itself is unchanged from the first submission**. The referees' stated reasons for rejection together with Hickey's rejoinders are given in "**Appendix II**".

One central thesis of Hickey's paper is that a distinctively macrosociological perspective is needed to address the interinstitutional relations that structure macrosocial change. This is a repeat of the experience of economists in the 1930's, when they found that a distinctively macroeconomic perspective is needed to address the sector relations that structure the business cycle. The romantic philosophy of science, which still prevails in sociology, explains the creation of institutions by social-

psychological "mechanisms" of socialization and social control, but these cannot explain the interinstitutional pattern of macrosocial changes. The macrosociological perspective is therefore incomprehensible to most academic sociologists including the editors and referees that attempted to critique Hickey's submission to the sociology journals. Those sociologists dogmatically demand a romantic analysis of motives, which is a social-psychological reductionist agenda.

Hickey finds Merton's concept of functionalism emphasizing objective consequences over subjective motives to be more enabling for quantitative analyses in the macrosociological perspective than the romantics' social-psychological reductionism to motivational analyses. More importantly he found the pragmatist philosophy of science to be enabling and the romantic philosophy of science to be retarding for academic sociology.

A Pragmatist Critique of Academic Sociology's *Weltanschauung*

This and the following section are relevant to philosophy of science in both the twentieth and the twenty-first centuries, because they illustrate how enforcement of romanticism has retarded the maturation of academic sociology into an empirical science. These sections are a philosophical pathology disclosing how both sociology's romantic social-psychological reductionist agenda and its anachronistic positivist concept of scientific theory sometimes incorrectly called "formal" theory have impeded the development of quantitative analysis and obstructed computational theory construction.

Just as medical pathologists study diseased subjects to understand better the functioning of healthy organisms, so too philosophers of science can and do make pathological examinations of retarding dysfunctionalities in astrology or sociology, in order to understand better the productive functioning of successful sciences. **Appendix II**, "Rejections and Rejoinders", reports the referees' attempted criticisms that the editors of the journals referenced to reject Hickey's paper, which is displayed in **Appendix I. Appendix II** is a pathological diagnosis of sociology's retarding dysfunctions. Hickey has retained the postal receipts and the dated original correspondences to document his priority as well as the original texts of the criticisms of the referees.

The issues exposed herein pertain to contemporary academic sociology's chronic legitimacy crisis: **Is sociology real science or pseudoscience?** The Royal Swedish Academy of Sciences awards a Nobel Prize for economics and for the physical and life sciences, but the Academy understandably has never awarded their Prize for sociology. Clearly – and understandably – the Academy does not recognize sociology as a valid science. The referee criticisms sent to Hickey by the editors are interesting historically because they are **original documents** exhibiting the criteria that are operative in academic sociology's institutionalized *Weltanschauung*. Hickey's rejoinders had no effect on the decisions of the editors, but contrary to these editors Hickey regards his paper as still worthy of publication as of this writing forty years after it was first submitted. He also affirms that his rejected paper was not a Trojan Horse intended to be an exposé of the technical incompetence and philosophical obduracy of academic sociologists, even if their referee criticisms and editor decisions are embarrassing for their academic occupation. But for all journal editors serving retarded academic occupations like sociology, Hickey offers a parody of Virgil's belated advice to the unfortunate Trojans: "Beware of philosophers of science bearing contributions", to which Hickey adds, "especially if they publish the incompetent reasons for rejection."

By way of preface to **Appendix II** Hickey makes the following comments: Consider **firstly** the sociologists' technical incompetence. 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 sociologically relevant time series for inputs to his system for constructing a macrosociometric model. He also wanted an 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 longitudinal model. Empirical work in sociology consists almost exclusively of survey research by written questionnaires, interviews and casual observation. One consequence is that any sociologist selected by an editor to be a critic could not reference any previously published models much less one that was empirically superior to Hickey's.

Another consequence of the unprecedented character of Hickey's macrosociometric model is that it reveals that academic sociologists are not educationally prepared to cope with the mathematics in Hickey's model.

Hickey's professional education is in economics as well as philosophy. Since the publication in 1939 of "Interactions between the Multiplier Analysis and the Principles of Acceleration" in *Review of Economics and Statistics* by 1970 Nobel-laureate economist Paul Samuelson, multi-equation longitudinal models like Hickey's have become a staple technique in mathematical economics and econometrics. And as early as 1933 the 1969 Nobel-laureate economist Ragnar Frisch first introduced the concept of shock simulations, which Hickey used to exhibit his findings and draw his conclusions.

Sociologists' technical competence is pedestrian, because the needed skills are not taught in the sociology curriculum. Any undergraduate economics student sufficiently motivated to search *Historical Statistics of the U.S.* and issues of the *U.S. Statistical Abstract* in a public library or a college library could replicate Hickey's model and simulations. But the sociology referees were suspicious of the findings drawn from the simulation and shock analyses in Hickey's paper, and ignorantly dismissed the paper as "unconvincing". The sociologists deemed by the editors to qualify as referees for Hickey's paper are too incompetent ever to be convinced. And Hickey found no evidence that the editors who selected them are any better.

Consider **secondly** the sociologists' philosophical inadequacies. When referees do not know what to do in their attempts to cope, they do what they know, whatever it may be. And what the sociologists know is romantic social-psychological reductionism, which even today often involves *verstehen* criticism, *i.e.*, what the particular critic finds empathetically "convincing". The editors of sociology's academic journals reject submitted papers that their chosen referees criticize as "surprising", "amazing", "bizarre" or "nontraditional", and accept only conformist hackwork that their referees say are "convincing" and "traditional", empiricism be damned. Authors like Hickey, who are not content merely to repeat platitudinous conventional wisdom, find their submissions rejected, and are labeled "ambitious". The ersatz philosophy of science enforced in conformist academic sociology is consequently so intellectually inbred and isolated that sociology's information pool is as degenerate as the gene pool of an incestuous hereditary dynasty. As a result academic sociology has become intellectually sterile, recognizably decadent, and practically impotent for social policy.

Inevitably as its decadence has become evident, romantic sociology is exhibiting classic *Sturm und Drang*. In his "Sociology's Long Decade in the Wilderness" Joseph Berger reported in the *New York Times* (28 May 1989) that universities such as the University of Rochester, 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 this *New York Times* article shows that since the 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. Admittedly demand for sociology Ph.D. degrees is influenced by many factors not specific to sociology, such as cyclical and secular changes in economic conditions and changes in the general population's size and demographic profile.   But the effects of such 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 the sciences are available from the United States Department of Education, Office of Educational Research and Improvement.   Graphs of the percent of earned doctorates in sociology (1) relative to the number of earned doctorates in economics and (2) relative to the number of earned doctorates in physics corroborate the secular decline of academic sociology reported by Berger in the *New York Times*.

The recent graduate with a Ph.D. in sociology today finds that there is little demand for what he has to teach, and may expect that he must pursue another occupation to earn a living.   Recently in "Education for Unemployment" Margaret Wente reported in the *Globe and Mail* (15 May 2012) that there are currently three sociology graduates for every sociology job opening, and she concludes that sociology students have been "sold a bill of goods".   Truly any student who assumes heavy financial debt for an academic degree in sociology is tragically naïve; he has mortgaged his future earnings for a white elephant. And she also lamented the fate of sociology professors who are fooled into believing that they might have a shot at the ever-shrinking tenure track, and who even if successful will merely be "masters of pulp fiction".

The incestuous peer-reviewed literature is a filter that only perpetuates sociology's inbred decadence.   Were a recent graduate with a Ph.D. in sociology lucky enough to find any academic employment but submitted a paper setting forth a thesis that is contrary to the dominant social-

psychological reductionism, he will find that no sociology journal will accept the submitted paper. Quality control in academic sociology is actually social control as described approvingly by the sociologist Warren O. Hagstrom in his *The Scientific Community* (1965). And were the rejected author so audacious as to submit rebuttals to the referees, he will likely find himself reading a contemptuously dismissive rejection letter as exemplified by a rejection letter Hickey received from William H. Form, the editor of the *American Sociological Review*. In the rejection letter Form sent Hickey, Form references the "folkways of the profession" saying that it is not "normative" for an article to be resubmitted once it is rejected, and claiming that otherwise an editor would spend his life rereviewing the same manuscript. Hickey believes that Form's view of "normative" editorial practice is fatuously distorted. Janice M. Beyer reports in her article titled "Editorial Policies and Practices among Leading Journals in Four Scientific Fields" in the *Sociological Quarterly* (1978) that her survey of editors of several leading academic journals reveals that **for sociological journals the percent of <u>accepted</u> papers that had been <u>resubmitted</u> to journals is forty-three percent.** Hickey believes that contrary to Form's statement, Form often published resubmitted papers.

Berger also quotes Stephen Buff, identified in the article as 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 Buff's excuses public opinion is not operative in these decisions made against academic sociology. Decisions to enroll or not to enroll in sociology graduate schools are made by undergraduate majors in sociology; decisions to support or close sociology departments are made by well informed 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 the unfavorable assessment of academic sociology originates within sociology itself; it does not lie with an ignorant general public.

Berger then quotes professor Egon Mayer, a Brooklyn College sociologist, who said that sociologists are still teaching what 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 should replace it. And here is the crux of the problem: if academic sociology were to purge its ranks of its reactionary traditionalism, they simply have is nothing to replace it, because

sociologists are literally too ignorant.

In a more recent *New York Times* OP-ED article (21 July 2013) titled "Let's Shake Up The Social Sciences" Yale University sociologist and cognitive scientist Nicholas A. Christakis, co-director of the Yale Institute for Network Science, wrote that while the natural sciences are evolving, the social sciences have stagnated, as manifested by the fact that social sciences offer the same set of university departments and disciplines that they have for the last nearly one-hundred years, thereby constraining engagement with the scientific cutting edge and stifling creation of new and useful knowledge. He wrote that such inertia reflects insecurity and conservatism, and helps to explain why social sciences don't enjoy the same prestige as the natural sciences. Hickey maintains that sociologists must firstly adopt the pragmatist philosophy of science with its empirical criteria, discard their romantic dogmatism of social-psychological reductionism, and focus on the outcomes of social behaviors instead of explanations describing motivational "mechanisms".

In a letter "To The Editor" published in the *New York Times* (29 July 2013) Constance A. Nathanson of Columbia University took "strong exception" to Christakis' demand that social scientists "reinvent" themselves as "half-baked natural scientists". Hickey comments that sociologists need not reinvent themselves for their occupation to become "half-baked"; sociology already is and has been "half-baked" for a very long time. In another letter "To the Editor" in the same *New York Times* issue Stony Brook University sociology professor Michael Kimmel is blatantly romantic. He says Christakis would shake up the social sciences by "myopically turning them into a subsidiary of the natural sciences". He claims that the "strength" of social science lies in its rôle as "a bridge between science and other pillars of the liberal arts" concerned with "interpretation and meanings". Hickey comments that doctrinaire fidelity to this purported "strength" is a disabling flaw that has greatly retarded sociology's maturation into a competent and productive empirical science.

The retarding effect of sociology's romantic dogmatism and social-psychological reductionist agenda is not limited to academia. Sociology had once been expected to serve as a guide for the formulation of effective social policy. But its neglect of unforeseen outcomes demonstrated its impotence in applied sociology. Berger's *New York Times* article cites disillusionment resulting from the failures of the Great Society programs of the 1960's, and

reports that sociologists have since lost Federal funding, must scale down their projects, forsake new inquiries, and disguise their work as anything-but-sociology.

Likewise 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 President John F. Kennedy and Lyndon B. Johnson writes that the optimistic vision of sociology guiding policy by use of its knowledge has faded considerably. Glazer observes that in trying to deal with the breakdown of traditional structures and particularly the family, social policies have weakened them further and have made conditions worse. He cites the welfare system, which undergoes continual expansion and repeated correction with input from social scientists, but which nonetheless damages the family, fosters family breakup, and encourages fathers to abandon children – even though many of the changes in the system were designed to overcome just these untoward outcomes. He notes that due to ignorance such *unintended* outcomes occasioned rejection of the government's attempts at social engineering. As Merton has stated, motivational analyses cannot account for unintended outcomes.

Sociologists' doctrinairism has indeed kept them ignorant. However, sociology's failure in the crucible of real-world social policy is not due merely to ignorance that can be remedied by more research in compliance with romantic philosophical dogmatism and its social-psychological reductionist doctrinairism. Sociologists will never understand these symptoms of their failure, until they recognize the pathogen infecting their own professional culture that operates in their criteria for criticism and imposes *a priori* restrictions on theorizing. As it happens, the eighth chapter of Glazer's book "'Superstitions' and Social Policy" is an exposé of sociologists' failure to recognize latent functions and unintended outcomes, and it amounts to a vindication of Merton's theorem of social engineering.

Academic sociologists will perpetually fail to contribute to effective social policy as long as they accept only "formal" theories that reduce to motivational social-psychological "mechanisms", much less to romantic theories that "make substantive sense" in compliance with the *verstehen* criterion. They will fail as long as they ignore romantically inexplicable latent functions and suppress publication of empirically superior theories that seem "surprising", "amazing" or "bizarre" relative to the sociologist's

platitudinous *verstehen*; and most importantly as long as contemporary pragmatism remains a *terra incognita* to academic sociologists. And they will also fail to establish their profession as a contributing and modern empirical science instead of a philosophically retrograde academic occupation parasitical on a halo effect from their universities' reputations. Twentieth-century academic sociology is a caricature of real science that has earned its chronic legitimacy problem. And to date the twenty-first century looks no better.

The optimism of the 1960's Great Society social programs, to which Glazer referred, has long ago passed into history, even as sociologists continue to bundle romanticism and social-psychological reductionism into their criteria for scientific criticism at the expense of empiricism. Glazer's use of the term "optimism" in his *Limits of Social Policy* is an understatement. Today only a government of incorrigibly naïve Candides would again entrust sociologists with a guiding rôle in the formulation of social policy. Before Panglossian professors of sociology can restore their credibility with policy administrators, they must overcome their philosophical dogmatism. They would greatly benefit were they to accept contemporary pragmatism, which rejects *a priori* commitment to any semantics or ontology – romantic or positivist – as a criterion for scientific criticism. Contemporary pragmatists recognize relativized semantics and ontological relativity, because they make acceptance of any semantics or ontology depends ***only*** on a theory's empirical adequacy. And this requires that sociologists must heed Merton's admonition and look to testable outcomes instead of motivations.

The "Last Sociologist"

In March 2001 Lawrence Summers, formerly U.S. Treasury Secretary with a Harvard University Ph.D. in economics, who as a Harvard University faculty member received tenure at the remarkably young age of twenty-eight years, was appointed Harvard's twenty-seventh president. His was not a caretaker administration. In his first year as President the changes made by this nephew of Nobel-laureate economists Paul Samuelson and Kenneth Arrow occasioned no little controversy. In "Roiling His Faculty, New Harvard President Reroutes Tenure Track" the *Wall Street Journal* (11 Jan. 2002) reported that Summers 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 were not overlooked in Harvard's sociology department. One unnamed faculty member was 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 in sociology. The old guard is not leaving quietly, as they are being pushed toward the exits.

A published example of sociologists' resistance to change appeared four months later in a *New York Times* OP-ED-page article (19 May 2002) with the apocalýptic title "The Last Sociologist" by Harvard sociology professor Orlando Patterson. Essentially Patterson's article is a defense of the German romantic dualism between the natural and social sciences, *i.e.*, *Naturwissenschaft* and *Kulturwissenschaft* with its doctrine that sociology is the subjective interpretative understanding of culture. "The Last Sociologist" article amounts to a reactionary jeremiad in defense of romanticism. Patterson complains that in their anxiety to achieve the status of economists 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 alleges that the methods of natural science are "inappropriate" and "distorting".

Patterson illustrates the kind of scholarship that characterizes his romantic vision of the golden age of 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 he claims 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. Contrary to Patterson such authors have no monopoly on such aims. But Patterson's plea defies parody; imagine a Harvard University physicist appealing in the *New York Times* to pursue basic research in the physics of an earlier tradition!

Patterson goes on to say 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. This is classic *verstehen*. Patterson laments that today

sociologists "eschew" explanation of human values, meanings, and beliefs. He claims that sociologists disdain as reactionary any attempt to demonstrate how culture explains behavior, while the 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. It is therefore unsurprising that in his article "How Sociologists Made Themselves Irrelevant" in the *Chronicle of Higher Education*, Patterson laments that sociologists have been excluded from inputting to social policy studies such as President Obama's "My Brother's Keeper" initiative of 2014.

But it must be conceded to Patterson that such articles as his "Last Sociologist" betray his recognition that the romantic agenda, which dominated Harvard sociology in the days of Parsonsian classical sociology, is now a spent force and is in its twilight. Changes at Harvard have begun thanks in no small part to inevitable (and blessed) attrition. The *Wall Street Journal* article reported that Summers' hiring policies received 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 classical and contemporary pragmatisms, Summers' influence augurs well for future academic sociology at Harvard even after Summers' departure.

Such nostalgia as Patterson's notwithstanding, American society needs an empirical quantitative sociology that enables forecasting, optimization and simulation for policy formulation, even if academic sociologists are still too technically incompetent and philosophically reactionary either to produce such work or to accept it when it is served up to them. Thus American academic sociology is still a missed opportunity, because the Federal Government offers a huge watershed of neglected sociologically relevant longitudinal data, some of which may be conveniently found in the recently published six-volume *Historical Statistics of the United States* (Cambridge University Press, 2010). Most fundamentally a scientific sociology requires substituting the pragmatic empirical criterion, for the romantic semantical and ontological criteria for criticism. American academic sociology might soon graduate to the status of a modern empirical science were sociologists like Patterson, Nathanson and Kimmel actually doomed dinosaurs. But notwithstanding their published laments they are not the "last sociologists". The criticisms displayed in **Appendix II** were written by referees like Patterson who complain that Hickey "eschews substantive reasoning", and whose criticisms

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were accepted by like-minded editors that rejected Hickey's quantitative empirical macrosociological theory.

But the genie is out of the bottle. Changes at Harvard have begun thanks in no small part to inevitable (and blessed) attrition. The *Wall Street Journal* article reported that Summers' hiring policies received 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 classical and contemporary pragmatisms, Summers' influence may augur well for future academic sociology at Harvard even after Summers' departure.

Interestingly Donald Black, a professor of sociology at the University of Virginia, has called for a scientific revolution in sociology. In 1998 he read a paper at an American Sociological Association meeting in San Francisco, which was later published in *Contemporary Sociology* (2000) under the title "The Purification of Sociology". Referencing Kuhn's *Structure of Scientific Revolutions* Black maintains that modern sociology is still classical, because its theory is classical, and that no scientific revolution can be expected in sociology until it abandons the classical tradition to which it still clings. He states that sociology is classical, because its explanations of social behavior are (1) teleological, *i.e.*, in terms of means and goals, (2) psychological, *i.e.*, in terms of subjective mental motivations, and (3) individualistic, *i.e.*, in terms of individual persons.

Black calls the needed revolutionary sociology "pure sociology", because these three characteristics of classical sociology will be recognized as nonessential. He says that "purifying" sociology of its classical tradition is a necessary condition for its needed revolutionary advance. He expects that this new purified sociology will differ so fundamentally from the prevailing classical sociology, that most sociologists will undoubtedly resist it for the rest of their days – declaring it "incomplete, incompetent and impossible". He adds that sociology has never had a revolution in its short history, that classical sociology is all that sociologists have ever known, and that sociologists "worship dead gods of the past" while viewing disrespect as heresy. With respect to the requirement for romantic social-psychological reductionism Black should have said, "purging" instead of "purifying", because romantic sociology is the sustaining ideology of the academic-sociology guild.

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Such exhortations as Black's are almost never effective in the absence of actual development of the needed revolutionary theory. Hickey's 1978 paper exhibited in **Appendix I** is explicitly a post-classical theory, just as Black describes it in his article, and just as Hickey's describes it in his paper. And Hickey's theory has been rebuffed by the kind of "classical" sociologists that Black criticizes, as exhibited in **Appendix II**. Hickey adds that another necessary condition for progressive change is the passing of the old generation. There will be no scientific revolution in academic sociology until a new generation becomes so rebelliously disenchanted with the status quo that they reject the complacent old guard's dogmas. Today the most promising ideas are contemporary pragmatism and mechanized analysis.

Yet not all of today's academic sociologists are apish troglodytes that drag their knuckles as they walk. The computational revolution in sociology has been active in sociology for half a century. Below the reader will find description of a truly pioneering computerized discovery system developed by John Sonquist, a promethean vanguard who blazed the path for a real science of sociology for the twenty-first century.

Sonquist on Simulating the Research Analyst with AID

John A. Sonquist (b. 1931) received a Ph.D. in sociology from the University of Chicago in 1936. Sonquist was a professor of sociology and the Director of the Sociology Computing Facility at the University of California at Santa Barbara, California. Previously he was for many years on the faculty at the University of Michigan at Ann Arbor, and was Head of the Computer Services Facility for the University's prestigious Institute for Social Research. He is also a past chairman of the Association for Computing Machinery. For his Ph.D. dissertation he developed a computerized discovery system called the **AID** system. "AID" is an acronym for "**A**utomated **I**nteraction **D**etector" 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 **AID** system is widely used for marketing-list scoring and also for risk scoring by financial lending institutions and by all three of the major national credit bureaus, Experian, Equifax and TransUnion, and the Fair-Isaac consulting firm. The **AID** system performs a type of statistical analysis often called "segmentation modeling" but with reference to a dependent variable, which serves as a relevance criterion for the chosen

segments. Sonquist's system, which is described in his *Multivariate Model Building* (1970), uses a well known statistical segmentation method called "one-way analysis of variance." Jay Magidson of Statistical Innovations, Inc. has developed a variation of **AID**, which is based on the equally well known segmentation method called chi-squared ("χ^2") analysis, and the system is now called **CHAID** (**Chi**-squared **A**utomatic **I**nteraction **D**etector). It is commercially available in the **SPSS** statistical software package and in the **SAS** system it is called **SY-CHAID**.

In the "Preface" of his *Multivariate Model Building* 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 performing 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 another course, and then secondly formalizing the decision-making procedure and programming it 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 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 that are much more sophisticated, because simulating the human being retrieving information is one of the objectives of the system designer. These more sophisticated information retrieval applications combine both a considerable data processing capability and logic for problem solving, such that the whole system is oriented toward the solution of a class of problems without human intervention.

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. But there is no evidence that he is advocating the *verstehen* philosophy of scientific criticism, because on the *verstehen* view a computer cannot "make sense" of social data, since it is not human and therefore cannot empathize with the human social participants. 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 such model construction by 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 interviewing and 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" means the existence of an interrelating 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 a problem that statisticians call "collinearity", which 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 statistically independent, so that the effects of each variable on the dependent variable can be isolated and treated as additive. In "Finding Variables That Work" in *Public Opinion Quarterly* (Spring, 1969) Sonquist notes that interaction among explanatory variables in a regression can be represented by combining them multiplicatively prior to statistical estimation to eliminate collinearity. This is also called creating cross products.

But there still remains the prior problem of discovering the interacting variables. One technique for detecting collinearity is to develop a correlation matrix for the independent variables, to determine which ones are actually not independent. A factor analysis will also accomplish this determination. The **AID** discovery system may be used in conjunction with such techniques as regression or multiple classification, in order to detect

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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 is distinctive in that it is an analysis procedure that uses a dependent variable as a criterion.

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 their **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 such that at each step in the procedure the arithmetic mean of each subgroup account for more of the total sum of squares (i.e., reduce the predictive error more) than the mean of any other pair of subgroups. This is achieved by maximizing a statistic called "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 a succession of binary splits for an analysis of personal 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 consisting of 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 in the sample. 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 in the sample, 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.

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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 in the sample. 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 **AID**.

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 philosophize, since the application of his **AID** system is less often philosophically controversial among sociologists. 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 interviewing and survey research consisting of verbal responses revealing respondents' mental states such as attitudes, expectations or preferences. In such applications a conflict occurs only with those extreme romanticists requiring the *verstehen* truth criterion.

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 rôle 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 addressed either arbitrarily or by making untestable assumptions.

Later he considers the rôle for discovery systems 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 well specified for statistical estimation.

Sonquist draws upon an introductory text, *An Introduction to Logic and Scientific Method*, written in 1934 by two academic positivist philosophers of science, Morris R. Cohen and Ernest Nagel. Cohen (1880-1947) received a Ph.D. from Harvard in 1906, and Nagel (1901-1985) studied under Cohen at City College of New York and received a 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, methods 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 invariant order among different 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 that Sonquist views as constituting middle-range sociological theory and that 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 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 rôles and social organization of computing in social science departments. Hickey maintains that while the authors' 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 romanticism. Reorganizing rôles can do no more for sociology than could reorganizing the deck chairs for the sinking *R.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 disinterring 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, i.e., by its function, in what Hanson called "research science" as opposed to "catalogue science". And the pragmatist realism practiced by Galileo, Einstein and Heisenberg and formulated as "ontological relativity" by Quine, bases every causal claim exclusively on the empirical adequacy of a tested theory. Discovery systems therefore make causal theories.

Comment and Conclusion

Pragmatism vs. Romanticism

At the opening of the twentieth century the prevailing philosophy of science was positivism with its philosophy of language. Positivism is based on reflection on Newtonian physics. The appearances of relativity theory and then quantum theory revised physics, and in due course revised philosophy of science to produce the contemporary pragmatism, which appeared as a critique of positivism. Contemporary pragmatism also differs fundamentally from romanticism, and ironically for the same reasons: the pragmatist theses of relativized semantics and ontological relativity. These ideas about language have their origin in Heisenberg's conversation with

Einstein in 1935 and on his own reflections on quantum theory the next year.

Romanticism has an a priori commitment to a mentalistic semantics and ontology as a criterion for scientific criticism, such that any proposed explanation not describing mental states is rejected out of hand regardless of its demonstrated empirical adequacy. Pragmatism on the other hand accepts only empirical criteria for scientific criticism, and rejects all prior semantics and ontologies as criteria for scientific criticism. Thus pragmatism *permits but does not require* mentalistic semantics and 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 specific semantics or ontology. Like Popper who said the science is "subjectless" pragmatists will accept any theory as a law that operates in an explanation that has been empirically tested and not falsified regardless of its semantics or ontology.

Pragmatism vs. Psychologism

Is computational philosophy of science conceived as cognitive psychology a viable agenda for twenty-first century philosophy of science? Simon recognized the lack of empirical evidence needed to warrant claims that their computational cognitive systems model the structures and processes of the human mind or brain. In fact he 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 replicated the historic scientific discoveries. Recognition of this deviation 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 psychological claims were tacitly ignored, while he and his colleagues including Langley continued to develop their systems without independent empirical research into psychology to guide new system development. Simon had a conflict of aims.

Others have also found themselves confronted with this conflict. 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

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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 pragmatic goal. It also notes that large AI systems have been installed in various major American corporations.

This alignment is incidental, 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 investment in artificial-intelligence systems pragmatically as it does every other investment including computer-system investments. Business 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 same pragmatic justification applies in basic-scientific research, because scientists will not use AI systems to replicate the human limitations. They will use AI to transcend these limitations, in order to enhance performance. Artificial intelligence may have outgrown its original home in academic psychology. The functioning of discovery systems to facilitate basic 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 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 that he made of wax. But Icarus flew too close to the hot sun, so that he fell from the sky as the wax melted, and then 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 the Icarus' design concept the "wing-flapping" technology. In fact in the 1930's there was a company called Gray Goose Airways, which claimed to have developed a wing-flapping aircraft called an "ornithopter". But pity the investor who holds equity shares in Gray Goose Airways today, because his stock

certificates are good only for folded-paper toy-glider airplanes. A contemporary development of the wing-flapping technology might serve well for an ornithological investigation of how birds fly, but it is not the technology used for modern flight, which has evolved quite differently.

When proposed imitation of nature fails, pragmatic innovation prevails, in order to achieve the practical *aim*. Therefore when asking how a computational philosophy of science should be conceived, it is necessary firstly to ask about the aim of basic science, and then to ask whether or not computational philosophy of science is adequately characterized as "normative cognitive psychology", as Thagard would have it. 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 includes law language that had earlier been a proposed theory and has not been falsified when tested. 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. The computational philosopher of science should feel at liberty to employ any technology that achieves this aim with or without any help from psychology.

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 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 human flight. In retrospect the thesis that it should, might be labeled the "Icarus fallacy." In computational philosophy of science "cognitive psychology" and "artificial intelligence" are as inessential to basic science as "engineering ornithology" is to manned flight.

It is furthermore noteworthy that to date 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. But if they have not been cognitive psychologists, nor have they been academic philosophers.

Sonquist was a practicing research sociologist. His inadequacy in contemporary philosophy of science led him to turn to 1930's-vintage positivism, to evade the romanticism prevailing in academic sociology. Pragmatism would have served him better. Now known as the **CHAID** system, Sonquist's system is the most widely used of all discovery systems.

For Litterman, evasion of the romantic philosophy was easier, despite the fact that he is the 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. 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. He would not have viewed the theories outputted by his system as "atheoretical". At this writing the Minneapolis Federal Reserve Bank still uses his system.

Hickey was more fortunate, since he is both an Institutionalist econometrician and a contemporary pragmatist philosopher of science. During the thirty years following his development of his **METAMODEL** discovery system, he had applied his system for market analysis of both consumer and industrial products, for consumer credit risk analysis, for macroeconomic business cycle analysis and regional economics, and for macrosociology in an Institutionalist macroeconometric model for economic development analysis.

The practical discovery systems developed by Sonquist, Litterman, and Hickey also reveal a distinctive strategy. *Their designs, procedures, and computer languages are mechanized automations of 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 philosophical issue between the

ordinary-language and the ideal-language philosophers earlier in the twentieth century. What may be called the ordinary-language computational philosophy-of-science approach is based on the analytical techniques that are ordinary in the respective sciences, and their applications have advanced new findings.

Computational philosophy of science is the wave of the future that has arrived, and information technology predictably grows exponentially over time. Some philosophers of science will make needed adjustments in their views. But most others will never acquire the necessary computer skills to contribute to this new line of development, and they will supply the profession's abundant share of latter-day Luddites for a generation or more. Possibly the philosophers' psychologistic turn has been in reaction against the doctrinaire nominalism built into the Orwellian newspeak that is the Russellian symbolic logic. Yet nothing precludes a linguistic computational philosopher of science who views the discovery systems as language-processing systems from recognizing a three-level semantics enabling philosophers to speak about semantics without having to make psychologistic claims. Cognitive psychology of science is still merely a promissory note, and science awaits evidence of its cash value.

APPENDIX I

A Post-Classical Quantitative-Functionalist Theory of Macrosocial Change in the American National Society

by

Thomas J. Hickey

Simon, Thagard and Langley

ABSTRACT

This post-classical quantitative-functionalist macrosociological theory of social change in the American national society is a recursive, first-degree, higher-order difference-equation system having parameters estimated statistically from annual time series 1920 through 1972. The model is developed by a computerized artificial-intelligence discovery system and implements the contemporary pragmatist philosophy of science.

Post-classical quantitative functionalism is here contrasted with classical functionalism, which is based upon social-psychological motivational mechanisms. Quantitative functionalism describes outcomes rather than motives, an emphasis also in Merton's functionalism, and it exhibits the macrosociological outcomes of exogenously initiated endogenous institutional changes.

The macrosociological outcomes are exhibited in static and dynamic analyses. The static analysis is exhibited by the equilibrium solution of the model, in which all variables are set to the current period t. The solution shows that the U.S. macrosociety has no stable equilibrium and is thus institutionally malintegrated.

The dynamic analyses are simulations made by iterating the recursive model. The simulations show that when per capita real income growth is high and if the national demographic profile is stabilized, the macrosociety tends toward macrosocial consensus equilibrium due to the educational institution, which is a distinctively macrosociological negative feedback mechanism. And a shock simulation shows that a sudden large internal migration surge from farms into cities disintegrates the institutional social order.

CLASSICAL FUNCTIONALISM

For purposes of contrast the functionalist sociological tradition represented by sociologists such as Durkheim, Parsons and Moore is here referred to as "classical functionalism". Classical functionalism is concerned with the institutionalization of rôle concepts, norms and value orientations (Parsons, 1951, p. 552). It explains social order and stability by the analysis of motivational processes called "integrative mechanisms" consisting paradigmatically of socialization and social control. These integrate social participants' need dispositions with sanctioned socially functional values. Parsons calls this process of integration the "fundamental dynamic theorem of sociology" and the "core phenomenon of the dynamics of social systems" (*ibid*. p. 42).

The resulting social stability or "equilibrium" is a complementary behavioral interaction of two or more social participants, in which each participant ("ego") conforms to the cultural value orientations and rôle

expectations of the other participant ("alter"), such that alter's reactions to ego's actions are reinforcing positive sanctions motivating continued conformity (*ibid*. p. 204). When this conformist stability extends throughout the society, the result is a stable consensus equilibrium that characterizes a highly integrated macrosociety. Classical functionalism is social psychology.

Classical functionalist theory does not explain endogenous initiation of social change. The institutionalization of cultural values, norms and social rôles by the operation of the integrative mechanisms of socialization and social control relate to social change only as forces of resistance except to the degree that the macrosociety is "malintegrated" (*ibid*. p. 250). The phenomenon of malitegration is viewed pathologically, because it is a condition of the cultural value system that permits deviant behaviors to have legitimating institutional valuation thereby creating "structured strains", which are impediments to the emergence of stable macrosocial consensus equilibrium throughout the social system (*ibid*. p. 493).

Thus in classical functionalism (and contrary to conflict theories) the initiating factors that produce social change are viewed as exogenous to the social system (*ibid*. p. 219,). For example Parsons says that he is less interested in the initiating factors than in tracing the repercussions throughout the social system of a change once initiated, including what he calls the "backwash" that modifies the original direction of change, because this shows how the concept of a social *system* is crucial. And he advocates more empirical investigation to address this problem (*ibid*. p. 494).

QUANTITATIVE FUNCTIONALISM

That needed empirical investigation is implemented by quantitative functionalism. Contrary to Parsons the paradigm of motivational "mechanisms" is not adequate for the analysis of the structure of the social system for the explanation of social change. It is not possible to trace the interinstitutional pattern of "repercussions" and redirecting "backwash", what Gustavson (1955, p. 28) calls "social forces", by focusing on integrative mechanisms viewed as the social-psychological processes by which common value patterns are integrated into the personality. Quantitative functionalism does not invalidate classical functionalism. But as in macroeconomics, macrosociological theory has a distinctively macro perspective that is not social-psychological reductionist.

In this quantitative theory Merton's functionalism is more applicable than Parsons' is, because Merton's describes observable objective consequences rather than subjective dispositions such as aims, motives, or purposes, and most notably it furthermore describes the effects on the larger macrosociety in which the functions are situated (Merton 1967, p. 51). In this study the larger macrosociety is the system of different **types** of internally organized institutional groups in the U.S. national macrosociety. Attention is therefore directed to another type of integrative mechanism consisting of negative-feedback relations due to the interinstitutional cultural configuration of value orientations that pattern the propagation of social change through the system of types of institutional groups. And the further empirical investigations required to identify these interinstitutional cultural patterns proceed by examining their effects in aggregate social data using statistical inference by constructing a quantitative theory consisting of a system of equations. 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 models have become a staple technique in mathematical economics and econometrics, and they apply no less so to sociology.

This quantitative macrosociological theory uses analysis of aggregate data for its method of construction, and does not use Parsonsian motivational analysis as is typically required by classical functionalists. The constructional process for this model was carried out with the assistance of a computerized discovery system (note: **not** a stepwise regression), which is described in this author's *Introduction to Metascience* (Hickey, 1976). When the constructed dynamic macrosociometric model is iterated, it propagates a time-series pattern of index numbers of growth rates of per capita rates. The resulting successive solutions track the progression of growth rates and the directions of changes in degrees of consensus as measured by per capita rates of voluntary institutional-group associations.

Unlike other classical functionalists Merton recognized that social interaction may have consequences that are not intended or even recognized by the members of the social system. His view of functionalism takes the objective standpoint of the observing social scientist and not the subjective standpoint of participant. He refers to the unintended beneficial consequences as "latent functions" and attributes them to "latent structures" in contrast to "manifest" functions and structures having consequences that are intended by the members (Merton, *ibid.*).

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Some of the relationships set forth in this functionalist theory seem clearly to be manifest structures enabling manifest functions such as the reinforcing effect of religious affiliation on compliance with criminal laws proscribing homicide. But there are also latent structures and functions. Latent outcomes are exemplified in Keynesian macroeconomics by the "paradox of thrift". And simulations with this macrosociometric model reveal the unintended and unrecognized consequences of social behavior, which are latent for the participants and are furthermore also likely hidden from classical sociologists. The evidence for the empirical validity of the model is: (1) the satisfactory statistical properties of the equations estimated over more than fifty years of historical sample data, (2) the successful capture of the patterns of the time-series sample data when the model is iterated, and (3) the accurate retrodictive testing performance of the model.

PHILOSOPHIES OF SCIENCE

Classical functionalism exemplifies the German romantic philosophy of science that Parsons brought from Weber's Heidelberg University, while quantitative functionalism exemplifies the contemporary American pragmatist philosophy of science. In academia contemporary pragmatism has superseded not only the positivist philosophy but also the romantic philosophy. Some of the relevant differences between the contemporary pragmatist philosophy of science and its predecessors are as follows:

1. There are different definitions of "theory". Romantics and positivists both define "theory" semantically, while pragmatists define "theory" pragmatically by its function in basic research. As Yale University's pragmatist philosopher of science Hanson (1958) wrote, ideas such as *theory*, *hypothesis* and *law*, if drawn from what he calls the finished "catalogue-science" found in textbooks will ill prepare one for understanding "research-science". The pragmatics of theory is empirical testing, and a theory can have any semantics. Thus while for the romantics sociological "theory" describes subjective motivations, and for the positivists sociological "theory" has a certain formal structure, for the pragmatists all "theory" in research-science is defined as any universally quantified discourse proposed for empirical testing. With respect to statistical models, for the pragmatist the theory is the model and the model is the theory, *so long as it is untested – either proposed for empirical testing or actually being tested*. A scientific law is a tested and nonfalsified theory.

2.   There are different aims and criteria for scientific criticism, and different concepts of explanation. For the romantics the aim for social science is explanation consisting of "interpretative understanding" of the conscious motivations deemed to be the causes of observed behavioral outcomes. And on the *verstehen* version the romantic sociologist must furthermore share in the participants' understanding empathetically, so that it is "convincing" for the sociologist, i.e., folk sociology. Pragmatists reject all semantical presuppositions as criteria for criticism. For the pragmatists **only** empirical adequacy demonstrated in testing may operate as a criterion for the acceptance or rejection of theories. Unlike positivists, pragmatists *permit* description of subjective mental states in the semantics of social science explanations, but unlike romantics they **never require** it.

3.   There are different views about semantics. For the romantic classical functionalist the semantics of terms such as "values" is fully defined prior to development of his theory. Indeed, it is defined in social psychology. For the pragmatists the tested and nonfalsified sociological theory and its test-design language define the semantics of its constituent terms. This is the pragmatist thesis of "relativized semantics" described by the contemporary pragmatist Quine (1981) and anticipated by both the microphysicist Heisenberg (1971) and the structuralist linguist De Saussure (1959). Relativized semantics implies that there is a semantical change in the descriptive vocabulary common to an earlier theory and its newer successor, as noted by Kuhn (1962) and Feyerabend (1962).

4.   There are different criteria for ontological claims. Ontology is the aspects of extralinguistic reality – including causality – described by the semantics of discourse. In science the most realistic ontological claims are those described by the semantics of tested and nonfalsified theories. For the romantic sociologist the participants' conscious subjective motivations are deemed to be the causes of their observed social behaviors and outcomes. Thus the romantic believes he firstly knows intuitively or introspectively the operative causal factors, and he then creates and evaluates any constructed theory (or model) accordingly. In romantic sociology this prior ontological commitment results in the fallacy of social-psychological reductionism. On the other hand the pragmatist firstly examines the empirical test outcome, and then uses the empirically adequate, i.e., tested and nonfalsified theory to identify the causal factors. This is Quine's pragmatist thesis of "ontological relativity" (1969), which was anticipated by Heisenberg (1958 and 1974).

THE VARIABLES IN THE THEORY

Except as otherwise noted the data for the variables in the quantitative functionalist theory are from the U.S. Commerce Department's *Historical Statistics of the United States* (1976) for the years 1920 through 1972. The internal reference series in the source are noted together with the variable symbols used in the equations. Where the data are not released as per capita rates by the source, the aggregates are transformed into per capita rates.

Some of the variables such as technological invention and voluntary exposure to mass media relate to the information content in the culture. Some others refer to demographic, economic, ecological or international conditions. But from the viewpoint of functionalist macrosociology the most significant variables in the model are the institutional variables. These institutional variables represent aggregate voluntary group-associational behaviors in the population, which consist of voluntary membership in, or formation of internally organized characteristically institutional groups, notably a family, a school, a church, a business enterprise, or the civil society. The fact that they represent ***voluntary*** behaviors means that they manifest value orientations. The fact that they represent ***aggregate*** behaviors means that the values are cultural values that are widely shared. And the fact that they represent ***group-associational*** behaviors means that the cultural values are characteristic of particular types of institutional groups.

When these institutional data are made ***per capita rates***, they reveal ***degrees of consensus*** in the total population about the cultural values of the particular institution, just as per unit price rates reveal economic values about particular products and services according to the "revealed preference" thesis set forth by Nobel-laureate economist Samuelson in his "Consumption Theory in Terms of Revealed Preference" (1966). Thus the per capita rate for a particular institutional variable is a measure of the population's level or degree of consensus about the values characteristic of that particular type of institutional group. When a per capita membership rate is near its maximum, there is a high degree of consensus in the population about the values characteristic of the particular type of institutional group. And a low per capita membership rate shows a low consensus about the values characteristic of the type of institutional group.

The data are averages over four-year periods. Like the economists' price elasticities the coefficients in the equations are dimensionless due to transformation of the per capita time series data into period-to-period change ratios, which also minimizes collinearity in the independent variables of the equations. Then the change ratios are transformed into index numbers with the base = 1.0 assigned to the out-of-sample last data period in each of the time series. The system executes each trial model through successive time periods to make predictions of the out-of-sample last period, so that all of the predicted values are compared to the uniform base period to determine accuracy. Like economists' elasticities the models' coefficients measure the impact of change in one institution upon change in another. The variable with the largest coefficient in an equation dominates the effectiveness of any positive or negative feedbacks. Also the coefficients' associated algebraic signs reveal relationships of value reinforcement or value conflict depending on whether the signs are positive or negative respectively.

This quantitative macrosociological functionalist theory contains endogenous variables for groups representing the five basic institutions of a modern macrosociety as follows:

♦ The *family institutional group* is represented by the annual change ratio in the marriage rate (Series B3) and denoted **MR**. There are several statistical series available that describe group associational behavior relevant to this institution, sich as family size or divorce rates. The marriage rate represents new family formation.

♦ The *governmental institutional group* is represented by the annual change ratio in the reciprocal of the homicide rate (Series H792) and denoted **LW**. The group for this institution is not the government apparatus or a political party, but rather the whole civil society and the law governed interaction among the citizens. What is of interest is the voluntary compliance with criminal law that maintains the minimal and necessary degree of social order, the breach of which is a crime. Of the various crime statistics available the homicide rate is clearly a measure of deviation from the minimum and necessary conditions for social order. Homicide is a violent crime and is also the most reliably reported, since there is usually an evident corpse. The **LW** variable measures voluntary compliance to criminal law.

♦ The *economic institutional group* is represented by the annual change ratio in the per capita rate of the formation of new business enterprises (Series V20) and denoted **BE**. This institution offers the greatest range of choice for measurements for the economic sector. Participation in a business enterprise is selected, because it is the principal group association for the capitalist economy, and business formation is voluntary.

♦ The *religious institutional group* is represented by the annual change ratio in the per capita rate of total religious affiliation (Series H793) and denoted **RA**.

♦ The *educational institutional group* is represented by the annual change ratio in the percent of seventeen-year-olds who graduate from high school (Series H599) and denoted **HS**. Unlike primary school, high school completion is voluntary, and it is the broadest measure of voluntary educational attainment.

The theory also contains four other endogenous variables representing factors that the discovery system identified as statistically significant. These other endogenous variables are:

♦ *Urban population* is represented by the change ratio in the percent of the population living on farms (Series K2) subtracted from one hundred percent and denoted **UR**.

♦ *Technological invention* is represented by the change ratio in the per capita number of patent applications for inventions (Series W96) and denoted **IA**.

♦ *Demographic profile* is represented by the change ratio in the national crude birth rate (Series B5) and denoted **BR**.

♦ *Mass communications media* is represented by personal consumption expenditures for newspapers, books, periodicals and cinema plus income from radio and television broadcasting companies. The source is the U.S. Commerce Department's historical *National Income and Product Accounts* (1976). The importance of each medium is weighted by the dollar value of the expenditure or income amounts. The sum of the dollars is deflated by the consumer price index to remove distortions due to inflation over time, and then the deflated series is made a per capita rate, then a change ratio, and finally an index number. The result indicates the change rate of voluntary exposure to various mass media and is denoted **CM**.

There are also three exogenous variables relevant to social change:

♦ *Macroeconomic conditions* is represented by the change ratio in the per capita rate of real income, the constant-dollar gross national product (Series F17) and denoted **GP**. This variable would not be exogenous were this quantitative functionalist theory integrated with a macroeconometric model of the U.S. national economy.

♦ *Military mobilization* is represented by the change ratio in the per capita number of armed forces personnel on active duty (Series Y904) and denoted **AF**.

♦ *Foreign immigration* is represented by the change ratio in the per capita number of immigrants from all countries (Series C120) and denoted **IM**.

EQUATIONS OF THE THEORY

Each equation of the theory is displayed below together with its coefficient of determination (R^2), Durbin-Watson statistic (D-W) and statistical variances.

Change Rates in Per capita Birth Rates:

(1) $BR_t = 0.48 + 0.373*LW_t + 1.079*MR_t - 0.928*CM_t$
                     (0.0061)   (0.0297)   (0.0314)

$R^2 = 0.8688$ D-W $= 2.4321$

The change rates of the crude birth rates (**BR**) increase with increases in the change rates of the per capita rates of conformity with criminal law (**LW**) in the same four-year period, with increases in the change rates of the per capita marriage rates (**MR**) in the same four-year period, and with declines in the change rates of per capita voluntary exposure rates to mass communications media (**CM**), all in the same period.

Change Rates in Per capita Marriage Rates:

(2) $MR_t = 0.82 + 0.638*GP_t + 0.015*AF_{t-1} - 0.495*BR_{t-2}$
                     (0.0092)   (0.0000)   (0.0087)

$R^2 = 0.9582$ D-W $= 2.0527$

The change rates of marriage rates (**MR**) increase with increases in the change rates of real per capita income (**GP**) in the same period, with increases in change rates in per capita armed forces active duty personnel (**AF**), four to eight years earlier, and with declines in change rates of the crude birth rates (**BR**) eight to twelve years earlier. The average age of first marriage during the fifty-year sample is twenty-one years (Commerce, P. 19). Thus this equation relates the peaks of the marriage rates to the troughs of the earlier birth rates instead of relating the peaks of the marriage rates and the peaks of the still earlier birth rates, to maximize the degrees of freedom. The time lag between the marriage rates growth and mobilization change rates (**AF**) is due to wartime postponements of marriage.

Change Rates in Per capita Criminal-Law Compliance Rates:

$$(3) \quad \mathbf{LW}_t = -4.78 + 2.955*\mathbf{RA}_t + 1.705*\mathbf{HS}_{t-1} + 1.042*\mathbf{BR}_{t-1}$$
$$\qquad\qquad (0.8509) \qquad (0.1263) \qquad (0.0333)$$

$$R^2 = 0.9165 \qquad D\text{-}W = 1.5671$$

The growth rates of the per capita rates of compliance with criminal laws proscribing homicide (**LW**) increase with increases of the change rates of religious affiliation rates in the same period (**RA**), with increases in the change rates of high school graduation rates of seventeen-year olds in the previous period (**HS**), and with increases in change rates in birth rates (**BR**) in the prior period. This equation reveals the institutional reinforcement between the civic value orientation and those of the religious and educational institutions. The positive relation between compliance with criminal law and birth rates suggest Ryder's comment that nothing makes a young generation settle down faster than a younger one coming up (1965).

Change Rates in High School Graduation Percentage Rates:

$$(4) \quad \mathbf{HS}_t = 1.55 - 0.343*\mathbf{LW}_{t-2} - 0.341*\mathbf{BR}_{t-2} + 0.396*\mathbf{GP}_{t-2}$$
$$\qquad\qquad (0.0269) \qquad (0.0147) \qquad (0.0255)$$

$$R^2 = 0.9519 \qquad D\text{-}W = 2.2653$$

The change rates of the percent of seventeen-year-olds who graduate from high school (**HS**) decrease with increases in change rates of the per capita rates of compliance with criminal laws proscribing homicide (**LW**), with increases in change rates in birth rates (**BR**), and increase with increases of the change rates of real per capita income (**GP**). All these operate with a time lag of eight to twelve years. These lengthy time lags suggest that the effects on high-school age students are mediated by the socializing efforts of adults such as the school authorities and/or parents. Aberle reports that the socializing function of parents implies a prospective attitude toward their children, and that the children's futures as envisioned by the parents will be influenced by the parents' experiences, as these are affected by conditions prevailing in the adult world at the time of their socializing efforts (1963, p. 405). Thus the equation identifies compliance rates with criminal law as influential conditions in the adult world.

Change Rates in Business Formation Per capita Rates:

(5) $BE_t = 1.39 - 0.688*IA_t + 0.164*IM_t + 0.047*IM_{t-2}$
            (0.0178) (0.0006) (0.0003)

$R^2 = 0.9669$ D-W $= 2.8134$

The change rates of the per capita rates of net new business formation (**BE**) increase with decreases in the change rates of per capita patent applications (**IA**), and increase with increases in the growth rates of per capita immigration (**IM**) with lags from zero to eight years.

Change Rates in Religious Affiliation Per capita Rates:

(6) $RA_t = 0.76 - 0.070*HS_{t-1} + 0.450*BE_{t-1} - 0.111*IA_{t-2}$
            (0.0027) (0.0030) (0.0006)

$R^2 = 0.9861$ D-W $= 1.8646$

The change rates of the per capita rates of religious affiliation (**RA**) increase with decreases in the change rates of the percent of seventeen-year-olds who graduate from high school (**HS**) in the preceding period, increase with increases in the growth rates of per capita net new business formation (**BE**) in the preceding period, and increase with decreases in the change rates of per capita applications for inventions (**IA**) two periods earlier. The negative algebraic signs show the conflicts of education and technology with religion and the reinforcement between the religion and business.

Change Rates in Technological Innovation Per capita Rates:

(7) $IA_t = -5.05 - 2.519*RA_t + 8.450*UR_t$
            (0.7570) (2.5359)

$R^2 = 0.8697$ D-W $= 2.5601$

The change rates of the per capita rates of technological innovation (**IA**) increase with decreases in the change rates of the per capita rates of religious affiliation (**RA**) in the same period, and increase with increases in the change rates of per capita rates of urbanization (**UR**) in the same period.

Change Rates in Urbanization Percentage Rates:

(8) $UR_t = 1.18 - 0.100*HS_t - 0.059*CM_{t-1} + 0.003*AF_{t-1}$
              (0.0018)   (0.0010)   (0.0000)

$R^2 = 0.9831$ D-W = 1.3162

The change rates in the percent of the population not living on farms (**UR**), i.e., the rate of urbanization, increase with decreases in the growth rates of the percent of seventeen-year-olds who graduate from high school (**HS**) in the same four-year period, increase with decreases in the growth rates of per capita exposure to mass media communications (**CM**) in the prior four-year period, and increase with increases in the growth rates of per capita memberships (**AF**) in the prior four-year period.

Change Rates in Mass Communication Per capita Rates:

(9) $CM_t = 1.89 - 1.624*RA_t + 0.611*GP_t + 0.250*GP_{t-1}$
              (0.2610)   (0.0110)   (0.0105)

$R^2 = 0.9555$ D-W = 2.6126

The change rates of per capita exposure to mass media communications (**CM**) increase with decreases in growth rates of the per capita rates of religious affiliation (**RA**) in the same period, and increase with increases in the growth rates of per capita real incomes (**GP**) in the current and prior periods.

STATIC ANALYSIS

In quantitative functionalism the term "equilibrium" means a solution of a model such that the values of each variable remain unchanged for successive periods of iteration. This is displayed by making all time subscripts current (**t**=0 for all) and then solving the equation system. Since the values of this model's variables are index numbers of change ratios of per capita rates, the equilibrium solution is one of constant change ratios of the per capita rates for all variables, and they may be positive, zero or negative. The classical consensus equilibrium is represented by constant per capita rates that are near the maximum for all the institutional variables.

However examination of the mathematical equilibrium solution of the model reveals that a static or zero-growth solution for all the institutional variables in the statistically estimated empirical model cannot exist, and therefore that the classical functionalist consensus equilibrium does not exist for the U.S. national macrosociety. Some institutional variables must increase in order for others to maintain a zero growth rate at any per capita level. Thus if the former institutional variables are forced to represent zero change, as when the maximum consensus per capita rate is encountered as its upper limit, then the latter must decline away from the per capita maximum or consensus equilibrium. This condition is illustrated by equation (3) where an increasing per capita rate of religious affiliation (**RA**) is necessary to produce stabile constant per capita rates of compliance with criminal law prohibiting homicide (**LW**).

Furthermore since the algebraic signs for some of the coefficients relating the institutional variables are negative, were a static equilibrium to exist, it might better be described as what Moore called a "tension-management" equilibrium rather than Parsonsian consensus equilibrium (1963, p. 10, 70). In summary: in classical functionalist terms the American national macrosociety is what Parsons called "malintegrated".

DYNAMIC ANALYSIS

In quantitative functionalism the term "dynamic" refers to the macrosociety's adjustment and stability characteristics as exhibited by successive iterations of the macrosociometric model. This meaning of dynamics is not unrelated to that found in classical functionalism, since changes in per capita rates are changes in measures of consensus about institutional-group values and reflect the effects of socialization and social control. However, the problem addressed by the model is not the problem of explaining the operation of the social-psychological mechanisms of socialization and social control, and the macrosociological theory does not implement a social-psychological reductionist agenda. Rather the relevant problem is the macrosociological problem of tracing the pattern through time of the interinstitutional adjustment dynamics of the macrosociety. To this end three types of simulation analyses are made, in which the upper and lower limits of the per capita rates are ignored, and the values of the variables are allowed to be unrealistic to display their adjustment patterns.

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Type I Simulation: In the first type of simulation the model was iterated with all of its exogenous variables and all of the initial lagged-values assigned their index number equivalents to represent zero change in their per capita rates. When the model is thus iterated, it propagates a time path that oscillates with increasing amplitude and a phasing of eight four-year periods, i.e., it generates an explosively oscillating intergenerational cycle of between twenty-eight and thirty-two years. This is due to the exogenously fixed constant real per capita GNP, so that there is no negative feedback to living standards (**GP**) that would dampen such explosive decline and growth rates in birth rates as occurred during the dire Great Depression years and the affluent post-WWII "baby boom" years. Capturing this feedback requires integrating this macrosociometric model with a macroeconometric model.

Furthermore examination of the structure of the model reveals that equations (1), (2) and (3), which determine the growth rates of the birth (**BR**), marriage (**MR**) and criminal-law compliance (**LW**) rates, are interacting to capture an intergenerational cyclical pattern in the national demographic profile. With historical birth rates gyrating from 15.7 in 1933 to 21.7 in 1947 to 14.9 in 1972, the empirical model has captured a cycle in the national demographic profile and shows its sociological effects. Thus when a new generation born at the peak of a "baby boom" is in their infancy, the simulation shows a coincident peak in the per capita rate of religious affiliation reflecting the practice of infant initiation. When they are in their teens, it shows a peak in the crime rate. When they are in their late twenties, it shows a peak in the marriage rates, and then the birth rates come full circle for another demographic cycle. Also when they are in their later twenties the simulation shows a peak in new business formation.

Another simulation was run with the criminal-law compliance change rates variable (**LW**) set exogenously to its index equivalent of constant zero-growth rate representing a continuing stable level of law-abiding social order. And the real per capita income change rate variable (**GP**) is set to its index equivalent of an atypically high annual growth rate of twelve percent, as occurred between the depths of the Great Depression in 1933 and peak production and employment levels of World War II in 1945. When the model is thus iterated, all of the institutional variables and the birth rate variable quickly settle into a stable moving equilibrium pattern of constant positive change rates in the direction of consensus equilibrium. But as noted in the static analysis above, the U.S. macrosociety cannot achieve stable consensus equilibrium due to its institutional malitegration.

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Type II Simulation: As with the term "dynamic", so too with the phrase "integrative mechanism", its meaning in quantitative functionalism is different from but related to its meaning in classical functionalism. For a macrosocial negative feedback in the model to be compatible with a classical functionalist integrative mechanism, it must produce a tendency to stabilize the rates of social change in constant positive growth paths for all the institutional variables, and thus trend upward toward macrosocial consensus equilibrium, even if such consensus is unattainable.

In order to isolate and make evident the interinstitutional integrative mechanisms, the birth-rate equation is removed from the model in these simulations, and the **BR** change rate is exogenously set to its index equivalent of zero making the per capita birth rate constant. As in the prior simulation all the exogenous and initializing lagged-values are assigned their index number equivalents representing zero change in their per capita rates. When the model is thus iterated but with the real per capita income change rate (**GP**) set to its index number equivalent of a high annual growth rate of twelve percent, then the model propagates a damped eight-year oscillating time path that converges into constant positive growth rates toward consensus equilibrium in the per capita rates of all the institutional variables.

The operative integrative mechanism is a dampening negative feedback due to equations (3) and (4), which determine the change rate of the compliance rate (**LW**) and the change rate of the high-school completion rate (**HS**). The model shows that an increase in social disorder as indicated by rising rates of noncompliance with criminal law proscribing homicide calls forth a delayed reaction by the socializing educational institution, which in turn tends to restore order by reinforcing compliance with criminal law. This negative feedback produced by the educational institution (**HS)** results in the positive growth paths toward consensus equilibrium; it is a macrosocial integrative mechanism. But these positive growth rates of all the institutional per capita rates need not necessarily result from the effective operation of this negative-feedback mechanism. As it happens, if all the exogenous variables are assigned index-number equivalents to zero-growth values, including the per capita real income variable (**GP**), then the resulting equilibrium is one in which the change in the criminal-law compliance rates (**LW**) is negative. That is because the zero-growth rate of the per capita real income variable represents a divisive social condition that Lester Thurow in 1980 called a "zero-sum society" with destabilizing effect.

Type III Simulation: The third type of simulations examines the stability characteristics of the growth equilibrium by disturbing it with shocks. In the shock simulations the magnitude of the shock is unrealistically large and the upper and lower boundaries of the per capita rates are ignored, in order to display the dynamic properties of the model. The results are thus intentionally eccentric to exhibit adjustment patterns.

Some sociologists such as Ogburn have cited technological invention as an initiating cause of social change. Thus a simulation was made in which the growth rate of the per capita rate of patent applications for inventions (**IA**) was increased from zero growth to one hundred percent growth for only one iteration. This one-time shock is an improbable permanent doubling of the per capita rate of inventions. When the model is initially iterated, the per capita rate of technological invention is kept constant at the index-number equivalent of zero-growth rate for fifteen iterations, i.e., sixty years, so that the model can adjust and settle into a long-term constant change-rate equilibrium solution. Then in the sixteenth iteration the shock, the onetime permanent doubling of the per capita rate, is made to occur. The result is a damped oscillation, a shock wave that propagates through the social system with a phasing of four four-year periods generating a sixteen-year cycle and having a small amplitude that nearly disappears after two cycles to return to the initial per capita change-rate equilibrium levels for all of the institutional variables. This is suggestive of a Schumpeterian economic-development cycle scenario of the economy's reaction to technological innovations, save for the noteworthy fact that the real GNP variable has been exogenously held constant, and thus can receive no reinforcing positive feedback raising the economy to a higher equilibrium level through a consequent shift in the macroeconomy's aggregate production function.

Similar simulations using the other variables as shocks yielded comparable results. But a very different outcome occurs when the shock is a permanent doubling of the change rate of the per capita urban residence rate (**UR**). As in the other shock simulations, the model is initially iterated with the index-number equivalent of zero change for fifteen iterations, i.e., sixty years, before the one-time doubling of the urban per capita rate is made.

The constant proportion of urban population during the initial fifteen iterations produces accelerating positive change rates of the all the institutional per capita rates but the educational institutional variable (**HS**),

which exhibits accelerating decline. The permanent agrarian share of the population makes the other institutional variables accelerate in the direction of consensus equilibrium with no cyclical reversals, because the educational institution's negative feedback is ineffective. This phase of the simulation scenario suggests the traditionalism of an agrarian society having a low valuation for education and a tendency toward high macrosocial integration.

But when the one-time doubling of the growth rate of the urban residents' share of the population is made to produce a sudden permanent doubling of their share of the macrosociety in the second phase, the opposite outcome happens. The sudden surge into cities that the shock represents sends the variable representing civil order (**LW**) together with all the other institutional variables except the educational variable into accelerating decline. The negative feedback from the educational variable's positive change rate is overwhelmed and cannot effectively function as an integrative mechanism to reverse the accelerating negative change rates of the other institutional variables. In other words the model describes a **macrosociety disintegrating** toward the Hobbesian chaos that Parsons says institutions exist to preclude. Such is the lot of a failed and collapsing society.

SUMMARY OF FINDINGS

The static and dynamic analyses with the quantitative functionalist theory of macrosocial change yield four findings about the American national society based on the fifty years of history following World War I:

1. Static mathematical equilibrium analysis shows that the interinstitutional cultural configuration of value orientations is malintegrated, such that macrosocial consensus equilibrium theorized by classical functionalists does not exist for the American national society.

2. Dynamic simulation reveals that fluctuations in the growth rate of the birth rate exhibit an intergenerational demographic life cycle which is explosively oscillating in the absence of a negative feedback reducing the level of per capita real income measured by per capita real GNP.

3. If the birth rate is exogenously made constant, the national society exhibits movement toward macrosocial consensus, when per capita real income grows at the historically high rate of twelve percent annually. This movement is due to **an interinstitutional cultural configuration**

that constitutes an integrative mechanism consisting of a negative feedback reaction to criminal social disorder operating through the socializing functions of the universal public educational institution.

4. Finally a static urban/rural share of the national population suggests a traditionalist agrarian society with all of the institutional variables except education exhibiting ***growth* toward consensus macrosocial equilibrium**. But a very large and sudden inundation of population from the nation's hinterlands into the cities sends the institutional variables into accelerated ***decline* producing disintegration of the institutional order and apocalyptic social disorganization**.

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APPENDIX II

Rejections and Rejoinders

Prologue

In science as in life generally, discovery produces novelty, which is a disturbance that produces negative reaction. And the greater the novelty, the greater the disturbance and consequent reaction. Hickey's computational metamodel produced a disturbingly novel macrosociological theory, and the disturbance produced a predictable reaction from the conformist academic sociology establishment, which was incapable of assimilating it. The reaction is exhibited below.

Hickey submitted his "A Post-Classical Quantitative-Functionalist Theory of Macrosocial Change in the American National Society" to four peer-reviewed sociology journals in succession. All four rejected the paper. In this appendix he describes his correspondence with the editors of the journals, the attempted criticisms written by their chosen referees, and his rejoinders to the attempted criticisms. The chosen referees are an editor-selected sample presumably representing the best and the brightest that American academic sociology has to offer. But the sample is a dismal exposé of sociologists' technical incompetence and their philosophical *naïveté*, the fact that sociology is manifestly retarded. A preface for the following criticisms and rejoinders is set forth above in the sections titled "**A Pragmatist Critique of Academic Sociology's *Weltanschauung***" and "**The 'Last Sociologist'**".

Sociological Methods and Research

The first academic sociological journal to which Hickey had 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 U.S. Postal Service receipt documents that the paper was received on 18 December 1978. The macrosociometric model itself was actually developed in the latter half of 1976, which is the year of the paper's registered copyright and the year that Hickey uses to document his priority.

It is also the year in which the U.S. Department of Commerce published the statistical compendium *Historical Statistics of the United States*, which is the principal source of input data for Hickey's computerized discovery system. On 22 May 1979 – five months later – Hickey received a letter from the editor, a Mr. George W. Bohrnstedt of Indiana University, rejecting the paper for publication. In his letter Bohrnstedt says he is in agreement with the criticisms. With the letter were enclosed the following two referee criticisms, which are paraphrased in detail below together with Hickey's rejoinders.

Bohrnstedt's first referee attempted criticisms and Hickey's rejoinders

Bohrnstedt: Bohrnstedt's first chosen referee stated that the paper is "theoretically a reification of the worst type", and that nothing is said about how values are constituted within the population or how they change.

Hickey: The term "reification" is highly ambiguous. But given the romantic character of the remainder of this attempted criticism Hickey can guess at Bohrnstedt's first referee's jargon. In Georg Lukács' *History and Class-Consciousness* "reification" disapprovingly refers to objectifications of human activity that become estranged from the subjects who produced them thereby masking their social genesis. His agenda is Marxist, self-consciously social psychological and antipositivist. Reification understood as objectification is not always viewed pejoratively. In a more neutral sociological context reification is merely the objectivity of the social world's institutions, because institutions are independent of and beyond the average individual's ability to change them. Institutions are indeed real, they change slowly, and Hickey's macrosociometric model describes the interinstitutional progression of those changes through history.

Talcott Parsons opposed the "reification" that he believed he found in the writings of the positivists. Apparently this referee concluded incorrectly that because Hickey's paper sets forth a quantitative model built with measurement data, the model must be positivist in spite of Hickey's discussion of pragmatism in his paper. Parsons considered reification to be fallacious and objectionable, because he viewed it as "monistic" realism requiring that all scientific theories be reduced to one, if they are not to be regarded as fictional. The monistic view is the Unity-of-Science agenda of the Vienna Circle positivists. Hickey is a pragmatist, not a positivist.

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Parsons proposes his own alternative ontological thesis of "analytical realism", according to which the concepts of science grasp aspects of reality. Hickey's pragmatism is also realist, and Parsons' "analytical realism" suggests the contemporary pragmatists' thesis of ontological relativity, which also admits multiple ontologies. But Bohrnstedt's chosen referee is apparently innocent of this pragmatist thesis, which enables empirical scientists to avoid the fallacy of overriding empirical criticism with one or another prior ontological prejudice. However Parsons is inconsistent with his analytical realism, because he is also a romantic that demands a mentalistic ontology.

This referee is a romantic, because he demands description of how values are constituted and how they change. His claim that nothing is said about how values are constituted within the population or how they change is actually two claims. Firstly the demand about how values are constituted is a throwback to social-psychological reductionism, and to the classical "mechanisms" of socialization and social control, which as Hickey made clear in his paper, are incapable of addressing the macrosociological problem addressed by his paper. The critic is attempting to force the author to change the topic of his paper to what the critic understands, because he evidently knows little about the modeling techniques set forth in the paper. As the author of this paper Hickey claims the right to decide what he will write about, and rejects this critic's attempt to dictate his own favorite topics.

Secondly the demand that Hickey describe how values change is irrelevant to the validity of the model. Hickey's model is very much about values, institutions and consensus. And some values associated with the various institutional groups undoubtedly have changed over the fifty years of the sample period. But that fact does not invalidate the sociological significance of the per capita voluntary group associational rates as measures of consensus. If this referee's criticism were valid, then every economist's market model would also be invalid, because products such as automobiles change over time, but the price, quantity and value relationships described by the supply and demand equations are still valid without having to describe how automobiles have changed. This naïve critic does not understand that his criticisms are irrelevant. To repeat the language in Hickey's paper: *the macrosocial changes that Hickey's model describes are changes in the population's degrees of consensus about values that are characteristic of the represented types of institutional groups as measured*

by their aggregate voluntary institutional group-associational behavior relative to the aggregate population.

Bohrnstedt: Bohrnstedt's first chosen referee rejects equation (6) complaining that it is a reversal of Weber's causal ordering, saying it is "offensive" and calling Hickey's causal claim "trite".

Hickey: *Webster's Dictionary of the English Language* defines "trite" as boringly obvious, but why that applies to Hickey's reference to Weber is mysterious given the referee's criticism. There is nothing in Hickey's paper that any sociologist could soberly call "trite" beyond Hickey's opening recitation of classical functionalism. Bohrnstedt's chosen referee rejects Hickey's reference to Weber, saying that Hickey has the direction of causality reversed. Does Bohrnstedt's chosen referee think this is trite? Furthermore Hickey is well aware that Weber says in *Protestant Ethic and the Spirit of Capitalism* that religion influenced the values of the capitalist economy. The relation Hickey mentions in connection with Weber was occasioned by Weber's thesis of "elective affinity", which has been interpreted by many Weberian scholars to imply reciprocity or mutual reinforcement. Hickey thinks that Weber was more sophisticated than this referee. Causality in human interactions is not a simple unidirectional influence. Causal factors are always located in a network of relationships wherein there is reciprocal interaction.

In Hickey's model this interaction is demonstrated in the execution of the model through its iterations for its simulations, where **the religion variable affects every other variable <u>and vice versa</u>.** Furthermore since the equations are linear and therefore monotonic, each endogenous variable can be transformed to be expressed as a function of any other, thus showing that Bohrnstedt's first chosen referee's presumption of unidirectional causality is naïvely simplistic. But in future versions of the paper Hickey deleted any reference to Weber, because his paper is not a gloss on Weber, and because for critics such as this referee issues about interpretations of Weber are needless distractions.

Bohrnstedt: Bohrnstedt's first chosen referee claims that Hickey's "value-based modeling" is "inferior" to the demographic accounting framework developed by Kenneth Land, because he believes that Hickey's model does not generate interpretable structural parameters. He also objected to Hickey's not referencing Land, and claims that interpretability of

parameters is one of the biggest advantages of Land's demographic accounting approach.

Hickey: Kenneth C. Land is a Duke University sociologist with his own agenda for sociological modeling. Hickey's model can indeed be described as "value-based". It uses demographic data but is nothing like Land's agenda. **Hickey since discovered that Bohrnstedt had previously published an article by Land titled "A Mathematical Formalization of Durkheim's Theory of the Causes of the Division of Labor" in** *Sociological Methodology* **(1970). This fact occasions Hickey's belief that Bohrnstedt is a patronage-driven editor, that he is not objective.**

Bohrnstedt's first chosen referee calls Hickey's value-based modeling "inferior". This claim is false. Bohrnstedt's chosen referee clearly recognizes that Hickey's value-based modeling is a competitive alternative to Land's agenda, and the critic therefore seems to find Hickey's modeling threatening. In fact Bohrnstedt's chosen referee objected to Hickey's not referencing Land. But Hickey's paper did not reference anything by Land, because Hickey's modeling has no need for it. Hickey is not indebted to Land or to Land's approach much less to any of Land's analyses. And Hickey is unwilling to be conscripted by Bohrnstedt and his chosen referees to support Land's agenda as a condition for publication. Bohrnstedt and his chosen referee are playing intermural academic politics.

Hickey's construing per capita rates as measures of consensus enables giving sociological significance to the vast watershed of data collected and released by the several cognizant Federal government agencies. The sociological relevance of these time series gives national demographic data more than just demographic significance, because it enables distinctively macrosociological modeling. For example Hickey finds sociological relevance (*i.e.*, interpretability) in the high-school graduation rates in his model, because the completion of high school reveals voluntary group-associational behavior in response to values characteristic of the educational type of institution. Obviously voluntary high-school school dropouts do not value education. Thus the high-school graduation rates measure degrees of consensus about this educational institution's characteristic values.

As for interpretability of the coefficients: *the statistically estimated coefficient measures the causal impact of changes in the phenomenon described by each associated explanatory variable of the equation upon changes in the phenomenon described by dependent variable of the*

equation. This practice of relativized or contextual semantics is characteristic of contemporary pragmatism as well as linguistics. It justifies a macrolevel representation that identifies macrosociology as a perspective in sociology separate from classical social psychology, just as starting with Keynes macroeconomics became recognized as a perspective in economics separate from classical economic psychology.

Furthermore the coefficient for each independent variable in an equation is interpretable as an "elasticity coefficient" as understood by economists, because the relation of the dependent to the independent variables can be expressed as the ratio of the two change rates to each other. Understanding this interpretation is not "rocket science", but amazingly Bohrnstedt's chosen referee is in obdurate denial of this evident interpretation, and with Bohrnstedt's complicity has had Hickey's informative empirical findings suppressed.

At the time Hickey was not aware of Bohrnstedt's blissful innocence of the contemporary pragmatist philosophy of science, until he later found that Bohrnstedt is a co-author of an undergraduate-level textbook titled *Statistics for Social Data Analysis.* The textbook effectively advocates an ersatz version of Haavelmo's structural-equation agenda first published in *Econometrica* in 1944, which implements the neoclassical economists' romantic philosophy. Like Haavelmo these authors distinguish unobserved "conceptual variables" from observable "indicators" thus revealing ignorance of the contemporary pragmatist thesis of relativized semantics.

Furthermore in his textbook Bohrnstedt demands identifying causality prior to statistical modeling and testing thus revealing ignorance of the contemporary pragmatist thesis of ontological relativity. This amounts to announcing the findings of the modeling before the modeling construction is performed. Bohrnstedt's textbook as well as his selection of referees reveals that he is an agenda-driven editor, and unfortunately for his journal and its readers his agenda is technically inadequate and philosophically retarded.

Bohrnstedt: Bohrnstedt's first chosen referee says that Hickey's system is a step-wise, "self-cooking" program that generates a model without researcher intervention, and that such "atheoretical routinization" is "inappropriate" for structural-equation modeling.

Hickey: If this rant is not by Bohrnstedt himself, it suggests that Bohrnstedt's chosen referee is or has been an undergraduate student taking a sociology course using Bohrnstedt's textbook. The phrase "structural-equation" modeling is code language from the Haavelmo agenda, which this critic is intent upon imposing on Hickey's modeling, although this sociologist has probably never heard of Haavelmo. Hickey readily agrees that his approach is "inappropriate" for structural-equation modeling, because Hickey does not subscribe to the Haavelmo agenda, and therefore *his macrosociological theory is not a structural-equation model.*

Furthermore Hickey's paper did not say that he used step-wise regression, and Bohrnstedt's critic's claim that Hickey uses it is presumptuously wrong. Had this referee read Hickey's *Introduction to Metascience*, he would know better than to make such a distorting misrepresentation. Bohrnstedt's critic's "self-cooking" rhetoric is a Luddite's diatribe. In the "Introduction" to his *Models of Discovery* Nobel-laureate economist Herbert Simon, a founder of artificial intelligence wrote that "dense mists of romanticism and downright know-nothingness" have always surrounded the subject of scientific discovery and creativity. What can an author think of an editor of a peer-reviewed journal who chooses a Luddite for a referee for a paper like Hickey's? He must conclude that the world still awaits the disenchantment of academic sociology.

Bohrnstedt: Bohrnstedt's first chosen referee believes that he perceives autocorrelation of the residuals in a number of the graphic plots included with this version of the paper.

Hickey: There is only small serial correlation in Hickey's equations, which is innocuous due to the very small residuals. Furthermore Hickey's model is very acceptable for showing the interinstitutional adjustment patterns in social change. But Hickey included the Durbin-Watson statistic and removed the graphic plots in future versions of the paper submitted to other journals to forestall such an objection, which was not made by any other referee.

Bohrnstedt: Bohrnstedt's first chosen referee also claims that there is "unnecessary inefficiency" caused by the pooling of data into four-year units, and prefers single-year observations.

Hickey: Bohrnstedt's chosen referee should invest in a dictionary of

the English language; his "inefficiency" rhetoric is as misconceived as his claim of "triteness". In fact the combination of observations into four-year periods enhances efficiency in the use of computer resources by reducing computation in Hickey's very computer-resource-intensive discovery system using the generate-and-test design. The four-year periods also remove the need for any distributed lags, which greatly reduce degrees of freedom.

But as it happens, Hickey's use of four-year-period time increments is entirely incidental due to the fact that one of the inputs to the discovery system was the political party of the U.S. President, who holds office in four-year terms. But this political variable is a social-conflict variable, and was not selected by the discovery system for any outputted model.

Bohrnstedt's second criticism and Hickey's rejoinders

Bohrnstedt: Bohrnstedt's second chosen referee says that he can't quite figure out whether or not Hickey's paper is "a put on".

Hickey: Hickey thought the first referee was bad, but the second is a grotesque parody. The "put on" comment is a defamatory slur. Sociologists who study the behavior of crowds know that anonymity fosters irresponsibility. Hickey believes that Bohrnstedt's releasing such a comment as criticism exposed *Sociological Methods and Research* as disreputable, and he consequently refrained from submitting any rejoinders to Bohrnstedt. Hickey believes that Bohrnstedt's selection of such a referee for his *Sociological Methods and Research* journal is a misfortune for his journal's reputation and a disservice to its readers. In fact Hickey believes that selection of Bohrnstedt to be the editor is a misfortune for this journal's reputation and its readers.

Bohrnstedt: Bohrnstedt's second chosen referee demands a "theory" to inform the specification of the models (i.e. the choice of explanatory variables) and to explain why equations of the particular form employed were actually used. He demands "justification" for the particular variables introduced, and says that statistical inference is totally ignored, that serial correlation is neglected, and that the results are not addressed to any "depth". He claims therefore that the findings are "almost without meaning", and that if there is meaning, Hickey does not communicate it. He concludes that he wishes that there were some improvements that he could suggest, and says, "I really can't understand what the paper is trying to do."

Hickey: The *Encyclopedia of the Social Sciences* defines functionalism as a theory of how major social patterns operate to maintain the integration or adaptation of larger social systems. It is remarkable that Bohrnstedt's chosen referee should not recognize sociologist' conventional view that functionalism is a sociological theory. The critic's belief that there is no "theory" reflects his romantic concept of theory. But Hickey's paper sets forth the findings from original research, and therefore implements the contemporary Pragmatist philosophy of science, according to which **empirical testing is the pragmatics of theory, such that <u>the model is the theory and the theory is the model</u>, because the model is the language that is proposed for testing**.

The equation specifications are extracted from the data, and the selection of variables is justified by the empirical adequacy of the equations that have been estimated over fifty years of American history. Does this referee really believe that the model represents fifty years of coincidence? Contrary to Bohrnstedt's chosen referee's comments statistical inference is not ignored; it is employed. Hickey addressed the erroneous claim of serial correlation by adding the Durbin-Watson statistic in future versions of the paper sent to other journals.

In this version of the paper Hickey had set forth only the estimated equations to display the interinstitutional structure of the national society. This is sufficient information to preclude this critic's complaints that the findings are "almost without meaning". But to prevent future such nonsense Hickey added the static equilibrium analysis to display the macrosociety's malitegration and the three types of dynamic simulation analyses to display the national society's interinstitutional adjustment patterns through time. These analyses offered additional insights and occasioned improvements in the paper but occasioned no change to the model itself.

Nonetheless as the paper says (to repeat for the obdurate sociologists), *the meanings of the variables are that they measure voluntary group associational behavior and therefore represent degrees of macrosocial consensus about institutional values characteristic of the different types of institutional groups*. If this critic's phrase "depth" is anything but obscurantism, it is code language for social-psychological reductionism. The critic's failure to understand what Hickey's paper is "trying to do" is the result of the critic's asking the old questions, questions that have social-

psychological answers usually obtained by small-group investigations. Such is not the kind of question addressed by a macrosociological analysis, so it is not surprising that the critic cannot understand Hickey's answers or their significance. Hickey also admonishes Bohrnstedt that since this referee admits that he cannot understand the paper, Bohrnstedt should have selected one that can. No referee can be persuaded by what he cannot understand.

American Journal of Sociology

The second sociological journal to which Hickey sent his paper is the *American Journal of Sociology* published by the University of Chicago Press and edited by an Edward O. Laumann, then the Sociology Department Chairman at the University of Chicago. The journal's stationary lists a Winfred Hunt Benade as editorial manager. The journal acknowledged receipt of Hickey's paper on 19 October 1979, and on 21 November 1979 Hickey received a rejection letter signed by Laumann together with two criticisms cited as reasons for rejection. Hickey submitted rebuttals to the journal, which yielded another referee criticism together with a second rejection letter dated 30 July 1981 and signed by Laumann stating that "several internal" referees had reviewed and rejected Hickey's rebutting rejoinders. The Internet web site for the University of Chicago identifies Laumann as a 1964 Ph.D. sociology graduate of Harvard University, and the site lists Parsons as the first among his teachers who influenced him. The criticisms by Laumann's chosen referees are the most dogmatically romantic that Hickey had received from any of these sociological journals. Laumann's choice of such referees led Hickey to believe that Laumann is another agenda-driven editor.

The first sociology department in the United States was founded at the new University of Chicago by Albion W. Small, and the *American Journal of Sociology*, the first sociology journal in the United States, was founded by Small in 1895. In his *A Short History of Sociology* Heinz Maus reports that under Small the journal always carefully avoided narrowness and one-sidedness in its outlook. The criticisms made by Laumann's selected referees show that Laumann has not followed in Small's diverse outlook. The criticisms of Hickey's paper reveal that Laumann enforces social-psychological doctrinairism. By selecting his anachronistic referees and their narrow outlook Laumann has marginalized this University of Chicago's flagship sociology journal. The three referee criticisms enclosed with Laumann's rejection letters are paraphrased below.

Simon, Thagard and Langley

Laumann's first criticism and Hickey's rejoinders

     Laumann: Laumann's first chosen referee says that Hickey's paper is "useless" because: **(1)** the data are not sufficient to the purpose, **(2)** the theory is not developed, **(3)** the indicators are not discussed to justify their theoretical use, **(4)** no other types of data or theorizing are brought to bear to assess the general value of the theory developed, and **(5)** the "general kind of enterprise" of developing theories automatically "without thought" is "not generally fruitful".

     Hickey: Firstly with respect to "useless" – The referee's statement is false. Utility is validating for applied research, but it is not necessary for basic research. Much basic research (*e.g.*, astronomical cosmology) is useless. Laumann's chosen referee does not understand basic research, and perhaps should have chosen social work instead of sociology for his occupation. Nonetheless in the basic-science perspective Hickey's model is useful, because it is informative. If this referee had not been so careless and prejudiced in his reading of the paper, he would have seen that ***the simulation analyses demonstrate the strategic rôle both of rising per capita real income growth rates and of rising secondary-education completion rates for increasing macrosocial stability including rising compliance with criminal law proscribing homicide***. The current problems of slow economic growth and of large high-school dropout rates have had manifestly disintegrating macrosocial consequences for the American national society. Thus one useful implication of Hickey's macrosociometric model shown by the simulation analyses is that generous public funding for education is beneficial for the macrosociety. Another is that rising per capita real income growth due to macroeconomic expansion is beneficial for the macrosociety.

     Furthermore this sociometric model's finding later suggested the development of an econometric model for an optimized State public investment fiscal policy, while Hickey was the senior economist for the State of Indiana Department of Commerce. The State fiscal policy model showed the optimum level both of private-sector employment and of State fiscal revenues. It showed that the optimum expenditure allocation of increases in the State budget is for primary and secondary education. At the Lt. Governor's request Hickey drafted a speech describing his econometric findings for the Speaker of the Indiana House of Representatives in support of the Governor's successful "A+" legislative initiative to increase

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expenditures for K-12 education in Indiana. This finding also corroborated the importance of K-12 education indicated by Hickey's *Post-Classical Quantitative Functionalist Macrosociological Theory of the American National Society*, which he constructed with his **METAMODEL** discovery system.

Furthermore with respect to usefulness Hickey later incorporated the model's equation specifications into a larger macrosocio-econometric model of the national economy, which he had developed for the Division of Economic Analysis of the Indiana Department of Commerce. This model was used for long-term economic analyses for economic development of the State economy that supported increased public financing for the State government's Indiana Corporation for Science and Technology. Economists call this mixed economic-sociological type of model an Institutionalist model.

All of these five comments by Laumann's first chosen referee are either irrelevant or wrong. Hickey's rejoinders to the above five itemized objections are as follows:

(1) Firstly to say that the data are not sufficient to the purpose is false, because the sufficiency of the data is demonstrated by the satisfactory statistical properties and retrodictive out-of-sample performances of the equations that were estimated from a lengthy sample representing more than fifty years of American history. And invoking the contemporary pragmatist principle of ontological relativity, Hickey maintains that the equations describe causal influences. Hickey's equations do not describe fifty years of incredible coincidence!

Furthermore the per capita rates were transformed into first-differences, *i.e.*, growth ratios. This transformation not only scaled the data to prevent ill conditioning in the computer calculations, but also enhanced the variances in the data, so as effectively to eliminate collinearity from the independent variables in the estimated equations. This transformation enabled the equations to measure more accurately the causal impacts of the explanatory variables on the dependent variables. This transformation to first differences also increases the difficulty in obtaining satisfactory statistical properties for a model, yet notwithstanding this added difficulty the statistical properties of Hickey's equations indicate that they are empirically acceptable stochastic models.

(2) Secondly to say that the "theory" is not developed reflects the critic's romantic concept of "theory". Laumann's chosen referee is ignorant of the pragmatic concept of theory that is actually operative in successful basic research. Hickey rejects the social-psychological reductionist agenda operative in this critic's classical concept of sociological theory. Hickey's paper is a contemporary pragmatist project, in which ***the theory is the model and the model is the theory, because the model is the language that is proposed for testing and that is tested. The pragmatics of theory language in science is empirical testing.***

(3) Thirdly to say that the "indicators" are not discussed to "justify" the "theoretical" use made of them is more social-psychological reductionism. Were this referee's reductionist demands carried to another level, he would further require reduction of social psychology to sociobiology, which invalidates social psychology as valid theory unless "justified" by biology. And the biology in turn would have to be "justified" by biochemistry. This romantic critic would not accept these successive reductions, but why not if he can disregard the problems that motivational analyses encounter in social psychology due to unintended outcomes? In fact the variables in Hickey's model are discussed quite adequately in his macrosociological paper and are appropriate for a macro perspective and his model describing determinants of outcomes rather than motives.

(4) Fourthly the demand for other types of data is gratuitous. Laumann's chosen referee has not referenced any variables in any empirically superior models. His demand is cynical, because it can be made of any paper at any time by the obstructionist referee, and is indicative of the critic's mental state of denial. For him there could never be sufficient data or evidence, and the demand is a wild goose chase. In other words, he is "sandbagging". Furthermore no other data are necessary to affirm the model's empirical adequacy and thus its explanatory efficacy.

(5) Fifthly Laumann's chosen referee says that the "general kind of enterprise" of developing theories automatically without thought is not "generally fruitful". This rhetoric is a red herring, because Laumann's chosen referee is not referencing Hickey's model. Hickey's model is not "generally"; it is the one in his paper, and his discovery system has been fruitful. Furthermore the criticism is pretentious, because Laumann's chosen referee gives no evidence that he commands the competence or experience

with mechanized quantitative data analysis to say responsibly what has been "generally fruitful". He is manifestly ignorant of the requisite computational analysis, statistical modeling and pragmatist philosophy.

Laumann: Laumann's chosen referee further elaborates on this fifth objection. He claims that the idea of replacing thought by the automatic working of a theory-building computer program requires a computer program with somewhere near the degree of complexity of a scientist's "intuition", and he rejects developing theories "automatically without thought".

Hickey: "Intuition" is a nondescriptive weasel word invoked by ignorant persons in the pretense of explaining what they cannot explain. The word is uninformative. Imagine that you are not an automobile mechanic, but you wish to make repairs to the transmission in your automobile. So you ask a professional automobile mechanic for some free advice. In response he nods, smiles and says, "Just use your intuition!" Be satisfied that you got your money's worth of free advice. For the romantic such as Laumann's chosen referee "intuition" is an evasive escape from responsible empiricism.

Laumann's chosen referee is a latter-day Luddite. As it happens, in his *Extending Ourselves* (2004) the University of Virginia philosopher of science and cognitive scientist Paul Humphreys, reports that computational science for scientific analysis has already far outstripped natural human capabilities, and that it currently plays a central rôle in the development of many physical and life sciences. The Luddite criticism by Laumann's chosen referee explains why no such development (excluding Sonquist's **AID** system developed as a dissertation at the University of Chicago's sociology department prior to Laumann's ascension as department chairman) can be found in academic sociology. Today's functioning computerized discovery systems are existential proof that mechanized theory development for empirical science is not only possible, but is ongoing. Hickey believes that this Luddite is the kind of critic that Laumann wanted; such is sociology in Laumann's reactionary and retarded Sociology Department at the University of Chicago.

Laumann: Laumann's chosen referee makes additional criticisms. He says that inadequacy of the data is illustrated by equation (3), because the shape of the time series for the homicide rate is almost level in the 1920's and early 1930's, drops in the thirties and in the 1950's, and then rises

markedly. He thus claims that there are "two pieces of information", and that any fluctuation in any other curve, which has that general shape, will result in the crime rate being either a predictor or an effect. He refers Hickey to an "ancient paper" by Sergeant in *Review of Economics and Statistics*, and describes the data as "sticky".

Hickey: This is the kind of criticism made in the 1930's by ignorant economists who incorrectly thought that linear equations cannot produce cyclical findings. The critic's description of the time series does not invalidate the empirical adequacy of the equations containing the crime variable. On the contrary, the existence of inflection points in the time-series data is helpful for making valid statistical inferences for longitudinal equations. Furthermore as the model is iterated through its recursive structure each endogenous variable is both cause and effect. In microeconomics reciprocal causality is recognized even in a static model representing the price-quantity relations in the equations for market demand and supply.

The *Review of Economics and Statistics* started publication in 1910, and the name Sergeant never appears in any issue in the journal's publication history. This spurious reference is as bogus as Laumann's chosen referee's whole critique. Laumann has selected a referee that is not just incompetent but is actually deceptive. This attempted deception suggests the corruption in Laumann's *American Journal of Sociology* peer-review process.

The image of "sticky" data truly staggers the imagination. Hickey can only retort that his data are not nearly as "sticky" as this sociologist's lame criticism is tacky.

Laumann: Laumann's chosen referee goes on to claim that the homicide rate time series does not measure any uniform condition of the social body, because violent crime has been concentrated more in the ghetto in recent years than it used to be – i.e. the distribution of homicide has been changing. Thus he concludes that the condition of the country as a whole is not a good indicator. He claims that what the indicators mean is not discussed with an elementary sociological sophistication. He also denies that the rate of formation of new businesses is "group associational behavior in the economic institutional group", and calls it "mere theoretical hocus-pocus". He makes similar comments about the heterogeneity of the

marriage rate over time, noting that the proportion of all marriages that are first marriages has been going down.

Hickey: Hickey's model is not a regional model; it is a macro model of the national macrosociety. The variables in Hickey's model measure what they say they measure, and if this critic wants anything else, he should write his own paper instead of attempting to force Hickey to change the topic of his macrosociological paper.

The heterogeneity of the national data is irrelevant to the subject addressed by the model, because it bears no relation to the empirical adequacy of the equations based on the national aggregate variables. If heterogeneity invalidated these equations, then every macroeconometric model containing such heterogeneous variables as aggregate national consumption or aggregate national investment would be invalidated given the heterogeniety of consumer or investment goods and services and their diverse geographical distribution in the national economy. No economist today is as naïve as Laumann's chosen referee, and none would commit the logical *non sequitur* made by this critic and say that that the aggregate consumption functions or investment functions in macroeconometric models are invalid due to the heterogeniety of the aggregate national data. Furthermore experienced modelers know that the more aggregate the data, the more statistically reliable the model and the more accurate its forecasts. This referee does not know what is relevant to the **macrosociological** perspective.

Also with respect to concentration of violence in the ghettos, it is noteworthy that the discovery system did not select the urbanization rate in the equation for conformity to criminal law prohibiting homicide. But it is also noteworthy that the shock simulation shows that exceptionally rapid urbanization increases crime. Thus urbanization as such does not produce crime. But accelerated massive urbanization is a large human ecological disturbance resulting in social disorganization that does increase crime. The change rate of the demographic ratio of Negroes to Caucasians was a variable inputted to the discovery system, and it was not selected in the outputted model.

Likewise changes in the proportion of all marriages that are first marriages are another irrelevant heterogeneity; the marriage rate variable is a measure for all marriages.

Laumann's chosen referee's dismissive "hocus-pocus" rhetoric is a frivolous attempt to trivialize Hickey's sociological interpretation and findings. Referees use such rhetoric when they cannot criticize. Laumann's chosen referee is in obdurate denial of the sociological significance of the per capita rates for the institutional variables. To repeat (and repeat and repeat!): ***The per capita rates having numerators that represent voluntary group-associational behavior show degrees of consensus about the values characteristic of the relevant types of organized institutional groups.*** It is impossible to speak of voluntary group-associational behavior without reference to values, because voluntary behavior is in response to values.

Hickey adds that his interpretation of per capita rates to reveal cultural values is comparable to Nobel-laureate economist Paul Samuelson's interpretation of unit prices to reveal economic values in the latter's thesis 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). Samuelson rejected the Austrian school's romantic concept of subjective utility, which is an introspectively perceived and unmeasurable psychological experience of consumer satisfaction that motivates consumer behavior. Instead he describes consumer preferences in terms of observed relative prices. Thus a commodity's relative per-unit-price measurements reveal economic value in the observable choices of the market transaction, even though the per-unit-price data do not characterize the economic value except in association with an identified type of consumer product or service. Similarly the per capita-rate measurements of institutional group-associational behavior reveal the degree of consensus about the set of cultural values characteristic of the type of institutional group, even though the per capita rate does not characterize the cultural value except in terms of the associated type of institutional group.

Laumann: Laumann's chosen referee claims that interpretation of the coefficients in the theory is "vague", as when Hickey compares the sizes of the coefficients with different numeraires; specifically the reciprocal of the murder rate is a very large number, so when the murder rate doubles the reciprocal increases by a very large number, while the rate of business formation per capita can double without varying by such a large number. Laumann's chosen referee thus concludes that the "*dimension*" of these coefficients is left very vague.

Hickey: This is a critic who needs a dictionary, because there is no problematic vagueness here. Laumann's first chosen referee has confused dimension with magnitude. All of the data used in the model are firstly transformed into per capita rates, which are then further transformed into change ratios of those per capita rates and then into index numbers of the change ratios with the out-of-sample period set as the base period for measuring each generated model's forecast accuracy. Thus coefficients relating these change ratios to one another in the equations are nondimensional like the economist's nondimensional price elasticity coefficients that relate change rates. Each coefficient in Hickey's equations measures changes in the dependent variable in response to changes in the associated independent variables in an equation.

Laumann: Laumann's first chosen referee then returns to his problem of replacing thought by the automatic working of a theory-building computer program, and he claims that this requires a computer program somewhere near the degree of complexity of a scientist's "intuition". He again claims that Hickey's discovery system is a stepwise regression, and that it therefore cannot really be called a "meta-theoretical program". He adds that there is a lot of "windy garbage" about the language in which these equations are described, i.e., "about semantic and syntactic and whatnot."

Hickey: This exhibition of shallow know-nothingism is truly astonishing. Romantics love the term "intuition", because it is useful as a strategically uninformative weasel word that they invoke, so they can appear to explain what they cannot explain. Hickey finds no evidence that Laumann's first chosen referee has any idea what a metatheoretical computer system looks like, and finds the critic's pretension ludicrous. Hickey's **METAMODEL** system described in his *Introduction to Metascience* and referenced in his paper's bibliography produced the theory that is displayed in his paper; the theory was not created by a stepwise regression.

Discovery systems have been in use for many years including the sociologist Sonquist's **AID** system. Hickey's system has been of great value to him professionally for more than thirty years for his Institutionalist econometric modeling in both business and government. This critic's rejection of mechanization is Luddite rant betraying his dismissive mentality. The "windy garbage" conceit is another manifestation of shallow knownothingism. Generative grammar enables mechanized syntactical

construction, and Quine's contemporary pragmatist philosophy of language enables semantical interpretation of the outputs.

Laumann: Laumann's chosen referee denies that Hickey's paper describes measurements of the values by which people live, so as to construct a model of how values make the social system stable. He claims that there is not in the whole paper a single measure of values, nor is there anything about how our values have changed over time. Laumann's chosen referee recognizes only "a bunch of simulations" with the model, which show that it is "absolutely useless", because it produces a positive feedback between the birth rate and the marriage rate of a cyclical sort. He thus compares Hickey's model to the Lotka equations of "hares and foxes", which produce explosive cycles until the whole world is covered with no hares and starving foxes. He concludes that Hickey's model is eighteenth-century demography.

Hickey: Laumann's selected romantic critic has chosen to be dismissive about the semantics of the institutional variables, to be in obdurate denial about the model, and to make statements that are blatantly false. This is due to his fixation for the description of values, to which he wants Hickey to conform. To repeat still again: Hickey's paper is not a social-psychological description that is characteristic of classical sociologists like Laumann's chosen referees. Parsonsonian motivational analysis is not capable of explaining the outcomes described by the model's simulations. Hickey reserves an author's right to decide the subject of his paper, and he has been clear about his subject, which this critic ignored.

Hickey does not say that the data are measures of values; indeed there can be no such a thing as a measure of the subjective experience of value any more than there can be a measure of the subjective experience of utility used in classical economics. To repeat (and re-repeat for this obdurate referee) the relevant discourse in Hickey's paper: *The numerators of the per capita rates measure degrees of macrosocial consensus about values characteristic of the several institutional types of groups as revealed by aggregate voluntary group-associational behavior. Typically no one voluntarily joins or remains in a group, if social controls in the group enforce values that he rejects. "Voluntary" means behavior in response to values. What are measured are the degrees of consensus in the general population or other relevant denominator in the per capita rate.* The Federal government offers a vast watershed of sociologically relevant

longitudinal data, but sociologists' obdurate refusal to recognize its sociological relevance is a huge missed opportunity that has retarded the profession's development of quantitative empirical macrosociology.

Laumann's chosen referee equated the positive feedback between the change in birth rates and the change in the marriage rate to the models created by Alfred J. Lotka. The Lotka-Volterra equations are viewed as quite reputable by contemporary biologists to analyze inter-species population dynamics. If this critic's comparison were correct, it might independently corroborate Hickey's model instead of criticizing it.

But the comparison is wrong. Lotka applied his model to biology in 1920 to describe predator-prey interspecies equilibrium dynamics in the demographics of wildlife. Hickey's U.S. macrosociometric model differs mathematically from Lotka's, because the latter's is nonlinear, continuous and has only two variables and two equations, while Hickey's macrosociometric model is a first-degree, higher-order difference system of nine equations, nine endogenous variables and three exogenous variables, and his model is statistically estimated from discrete time-series data.

Hickey references Nobel-laureate Paul Samuelson's multiplier-accelerator interaction model published in 1939, the type of mathematical model most commonly used by econometricians today and used by Hickey in his paper. The behavior of Samuelson's model depends on the values of its statistically estimated parameters. Likewise when a Lotka model is empirically estimated, as Pat Langley did with his **IPM** discovery system in his "Discovering Ecosystem Models from Time-Series Data", the model's behavior will depend on the statistically estimated values of the parameters in the equations, just as the behavior of the equations in the simulations with Hickey's model. But this critic references no empirical sociological applications of the Lotka-Volterra type of model in his comparison.

As it happens the actual U.S. birth rates during first three-quarters of the twentieth century exhibited wildly cyclical fluctuations with a declining trend ending in 1975 during the sample period of the model, and the model's empirically estimated coefficients captured the fluctuations and trends. It was not until the last quarter of the century and beyond that the fluctuations stabilized, and this latter period was not in the sample data. Specifically during the sample period from 1920 to 1972, the period available at the time the model was made, birth rates dropped by 47%, while since then they have

dropped by only 15%. Had the most recent forty years following 1972 been available at the time that the model was made, the simulation results for the birth-rate equation would have been different. Curiously in his *How Civilizations Die* David Goldman maintains that in the latter half of the twentieth century birth rates have been falling in many countries (excluding Israel and the U.S.), and that like many civilizations that became extinct due to depopulation those countries are entering the "fourth Great Extinction" to occur in the twenty-first century. Hickey's model is not eighteenth-century demography; it is an accurate empirical capture of mid-twentieth-century demography. And consequently it was necessary to make the birth-rate variable exogenous for some of the simulations, in order to isolate the interinstitutional adjustment patterns that the model is designed to explore.

More relevantly the positive and negative interinstitutional feedbacks described in Hickey's model crucially depend on the values assigned to the exogenous variables that are not found in Lotka's equations. In addition to the change rate in birth rates, another relevant exogenous variable in Hickey's model simulations is the change rate of the per capita rate of aggregate real income, which is strategic for the twentieth-century Great Depression era represented in the sample data that Hickey used. Historically both resident population and per capita real income approximately doubled in the entire fifty years covered by the model. There was no long-term Malthusian starvation due to diminishing returns, because in the U.S. there were abundant natural resources, the "green revolution" in crop yields due to technological innovation, and increasing production to scale. Thus instead of Malthusian destitution there were rising living standards except during the Great Depression years due to declining aggregate demand and stagnation during the liquidity-trap period. But in one of Hickey's "what-if" simulations and **contrary to history** per capita real income is exogenously made **constant** at zero-growth rate, thus making the society what economist Lester Thurow in 1980 called a "zero-sum society", in which anyone's gain must be someone else's loss.

But most fundamentally Hickey's model does not describe any predatory demographic dynamics like Lotka's model; the macrosociological model contains no predator variables. Thus the referee's comparison with Lotka is not even wrong; it is frivolously irrelevant.

Laumann: Laumann's chosen referee wrote that Hickey's paper is not publishable in *The American Journal of Sociology* or anywhere else, "for it is thoroughly incompetent in every respect".

Hickey: *Urdummheit*!

Laumann's second criticism and Hickey's rejoinders:

Laumann: Laumann's second chosen referee starts by writing that he has a great deal of sympathy with attempts to provide large-scale explanations of change in American society.   But he next demands "**concrete**" thinking about "**specific**" mechanisms that are sources of stability and change.   He says that "clumsy abstractions" such as Hickey's latent control structures or Land's opportunity-structures approach are "too vague" to illuminate anything about social change that is "not obvious".   He says that empirical analysis needs "disciplined" and "substantively informed" investigations to evaluate conjectures about social change, and he addresses Hickey saying: "In this regard I am simply not convinced by anything you report."

He continues by saying that Hickey "eschews substantive reasoning" as a basis for the empirical models and instead let his computer program alone determine the specifications that he reports.   He rejects the findings as often "bizarre", as exemplified by birth rate varying directly with the homicide rate and inversely with expenditures on mass media, and says he finds this impossible to take seriously.   He claims there is a burgeoning literature reporting analyses of the various influences of trends of women's labor force participation rate, relative economic status, past demographic swings, and sex rôle attitudes on fertility trends.   He says that these analyses in contrast to Hickey's rest on attempts to specify "concrete behavioral mechanisms" responsible for observed trends, while Hickey's equations are *post hoc* interpretations, which are never buttressed by independent evidence and hence are highly fanciful.

Hickey: Phrases like "disciplined" and "substantively informed" are romantic rhetoric. The *Encyclopedia of the Social Sciences* defines functionalism as an explanation of how major social patterns operate to maintain the integration or adaptation of larger social systems.   In Hickey's model the larger system is the national macrosociety.   More formally stated functionalist explanations are about movements within a system toward

stable self-maintaining equilibria. Functionalism lends itself to the distinctively macrolevel perspective. Durkheim saw macrosociety as an entity *sui generis* in need of social integration, which is not explainable by reduction to individuals or their psychological dispositions or motivations however "specific" or "concrete" they may be described. The anthropologist Malinowski stressed the integrating functions of different types of institutions for maintaining social structure. Hickey agrees with the insights of these authors, and he follows in the empirical emphasis on outcomes advocated by Merton. But Hickey substitutes mathematical modeling of the social system for the traditional analogies like homeostasis within biological organisms for describing equilibrium-seeking movements. His empirical model shows that a stable equilibrium does not exist in the American macrosociety. The "**concrete**" and "**specific**" mechanisms that are sources of stability and change are revealed by the simulations made with the model's negative feedback relations.

Laumann's second chosen referee needs no introduction for economists. His fallacious criticism is familiar and has long ago been repudiated. It is the same kind of dogmatic romanticism exemplified by Joseph A. Schumpeter, a member of the classical Austrian school of economics. In his review of Keynes' *General Theory of Employment, Interest, and Money* in the *Journal of the American Statistical Association* (Dec. 1936) Schumpeter described Keynes' propensity to consume thesis as nothing but a *deus ex machina* that is valueless, if we do not understand the "mechanism". The economic historian Mark Blaug of the University of London writes in his *Economic History and the History of Economics* that Keynes' consumption function is not derived from individual maximizing behavior, but is instead a bold inference based on the known relationship between aggregate consumer expenditures and aggregate national income (P. 243). Harvard's Alvin Hansen called Keynes' consumption function his greatest contribution. Schumpeter goes on to write that Keynes' inducement to invest, his multiplier, and his liquidity preference theses, are all "an Olympus" of such hypotheses which should be replaced by concepts drawn from the economic processes and mechanisms that lie behind the surface phenomena. By "economic processes" and "mechanism" he meant the ersatz maximizing psychology of classical economics. Were it not for the Great Depression, the economics profession might have joined reactionaries such as Schumpeter in denying recognition of a distinctive macroeconomic perspective, as Laumann and his chosen referees are attempting to deny recognition of a distinctive macrosociological perspective.

Laumann's second chosen referee says that he has a great deal of sympathy with attempts to provide "large-scale" explanations of change in American society. In his book *Social Causation* Robert (P. 371) Robert MacIver had expressed the view of classical sociology by saying that the primary contrast between social causation and the causation revealed in physical and in biological phenomena is that the former involves the "socio-psychological nexus". **But macrosociology is not just "large-scale" social psychology.** The word "macro" means that the macrosociological theory describes phenomena that cannot successfully be reduced to microlevel "mechanisms" without committing the logical fallacy of composition. Unlike the old classical economists and Laumann's classical sociologists, undergraduate students of economics today recognize the fallacy of composition, which sociologists can find explained at length in the introductory textbook, *Economics*, (P. 14) written by 1970 Nobel-laureate Keynesian economist Paul Samuelson. In Parsons' terms but contrary to Parsons, macrosociological outcomes are "emergentist", and this precludes reduction to an individualistic microlevel social psychology of motivational "mechanisms". The motivations of social members cannot explain unintended outcomes exhibited by the macrosocial system. Explanation of unintended outcomes requires a macrosociometric model.

Laumann's second chosen referee is no less dogmatically dismissive than were the 1930's classical economists such as Schumpeter. Laumann's second chosen referee is just more retarded, since Hickey's paper was submitted to this sociology journal a half-century after Keynes. In the urgency of the Great Depression economists became pragmatic, simply ignored Schumpeter's classical criticisms, and instead explored, developed and applied Keynesian macroeconomics. And in the 1950's, when computers became available to universities, economists developed macroeconometric models such as the Klein-Goldberger model based on Keynes' insights, which is how Keynes is still used today.

Sociologists should likewise ignore reductionist demands for "concrete" thinking about "specific" social-psychological "mechanisms". They should not be deterred from developing empirical macrosociometric theories having a distinctively macro perspective. They should not capitulate to any editor or his favorite referee attempting to impose a dogmatic social-psychological reductionist fetish, as Schumpeter and his fellow Austrian-school reactionaries had attempted to bully economists with

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their economic-psychological reductionist fetish. Nor should Laumann's Luddite referee bully sociologists for using computerized discovery systems to explore data in order to develop empirical models pragmatically, models that exhibit relations among the institutional variables that social-psychological reductionism cannot reveal. For example the integrative mechanism exhibited in Simulation II produces a dampening negative feedback due to equations (3) and (4), which determine the change rate of the compliance rate (LW) and the change rate of the high-school completion rate (HS). *But this feedback is not due to high-school graduates' motivations to produce macrosocial stability by increasing compliance with laws prohibiting homicide; it is an unintended outcome and is not explicable in terms of the motivations of the participants. Similarly the disintegration of the institutional order and the social disorganization in the macrosociety due to accelerated massive internal migration into cities is unintended and is not the motivation of the migrants.*

Laumann's chosen referee's phrase "clumsy abstractions" is a clumsy dismissal. He writes that the clumsy abstractions are too vague to illuminate anything about social change that is not obvious. But if the findings from Hickey's model are obvious, then why does this referee write that he is not convinced by anything Hickey reports? In fact he says this because the patterns and outcomes revealed by the model's structure and simulations are **not obvious** without the model. Hickey did not invoke Ogburn or Land's thesis of "opportunity structures". But Hickey did invoke the Columbia University sociologist Robert K. Merton's thesis of latent control structures, which may be operative where the relationships described by the equations are **not obvious** to the social members and to sociologists such as this referee. One of the most difficult problems with romantic attempts to reference motivations is that the outcomes of many actions are not explicable in terms of conscious motives, *i.e.*, **"concrete"** and **"specific"** mechanisms, when the outcomes are unforeseen by the social participants.

Like the Institutionalist economist Wesley C. Mitchell, the sociologist Robert K. Merton said in his *Social Theory and Social Structure* that the concept of social function should refer to **observable objective consequences** and not to covert subjective dispositions such as aims, motives, or purposes that have enthralled the romantics. **Observable objective consequences** are what the model's simulations describe. Merton makes the concept of function involve the standpoint of the observer and not necessarily that of the participant. He adds that the social consequences of

sociological interest are those for the larger structures in which the functions are contained. This is a central thesis of functionalism, and it is central to Hickey's paper where the larger structure of interest is the U.S. national macrosociety. ***Merton wisely warned that failure to distinguish between the objective sociological consequence and the subjective disposition inevitably leads to confusion, because the subjective disposition may but need not coincide with the objective consequence; the two may vary independently.*** But Laumann's chosen referee is not just confused, his dogmatic social-psychological reductionism has made him invincibly obdurate.

Hickey's paper says that an increase in the growth rate of the birth rate with a lag of four to eight years leads to an increase in the growth of *compliance* with criminal proscription of murder. Need it be said that this is not because it is the four-year-old to eight-year-old children who stop committing murders! If one insists on speculating about motives, one might say that the increased compliance is because the adult parents, whose domestic responsibilities motivate them to look to their own and their children's futures, choose to be responsibly law-abiding parents. And it might also be because in this *cyclical* model the sixteen-year lag shows a negative relation to compliance, because teens are less socialized than adults. Criminologists know very well that national crime rates are greatly influenced by changes in the national demographic profile; teenagers are the foot soldiers in criminal gangs that engage in murderous turf wars.

Also the negative relation between changes in the per capita rate of compliance with criminal law prohibiting homicide and changes in the per capita rates of mass media exposure appears to corroborate the frequently expressed concern about the outcomes of favorable portrayals of violence in the mass communications media.

Hickey had made some computer runs with inputed data collected and released by the U.S. Department of Labor, Bureau of Labor Statistics that included women's labor market participation, women's employment and women's unemployment. In those runs none of these variables were selected for output in any of the generated models. Hickey also inputed data collected and released by the U.S. Department of Health and Human Services, National Institute for Health, National Center for Health Statistics that inputed data included fertility rates and median age of first marriage. The discovery system did not select these for any of the outputted models,

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but instead selected crude birth rates. This critic's claims about a "burgeoning literature" are too vague to be informative. Laumann's chosen referee has not referenced any superior models containing variables representing the additional factors he demands. Hickey demands that Laumann's chosen referee identify "*concrete*" and "*specific*" variables in empirical models that are more empirically adequate than those in his model.

Hickey's so-called "*post hoc*" interpretations are the result of his recognition of relativized semantics determined by the linguistic context consisting of both the definitions described by the data's source documents and by his empirically adequate equations estimated over fifty years of American history. The demand for "buttressing" independent evidence is gratuitous if not also cynical, because a demand for more evidence can be made of any paper at any time. It is indicative of the critic's dogmatic mental state of denial in his attempt to invalidate the valid empirical model. Sending a writer after still more evidence is a well known sandbagging strategy. Similarly the demand for "substantively informed investigations" is code for social-psychological analyses. The evidence for Hickey's model is its empirical adequacy, and his model is empirically adequate, even if this critic finds it "bizarre" in contrast to his dogmatic psychologistic prejudice. What this referee calls "*post hoc*", Hickey calls "a posteriori", which is to say "empirical", which is to say "scientific". Romantic sociologists like this referee are functionally illiterate in empirical science.

But Laumann's chosen referee says he is "simply not convinced by anything" that Hickey reports. This obduracy is symptomatic of the mathematical illiteracy of a sociologist who is unconvinced because he is uncomprehending due to his incompetence. The problem is not with the model but with this dismayed referee's inadequacy due to his obdurate psychologistic doctrinairism and incompetence in technique. Laumann's referee criticisms have exposed American academic sociology to be an intellectual ghetto.

Laumann's third criticism and Hickey's rejoinders

Laumann: Laumann's third chosen referee is the "internal referee", which apparently therefore is internal to Laumann's University of Chicago sociology department. Hickey believes the critic is probably Laumann himself, because his opinion is not independent. This critic says that he does not consider Hickey's reply to the other criticisms to be sufficiently

compelling to warrant reconsideration of the manuscript. He claims that Hickey "does not understand" the fundamental objection to the paper that was raised by both referees. He notes that both critics object to (1) the strategy of model building in the paper, (2) the interpretation of the variables ("indicators") used in the models, and (3) the failure to provide a "convincing" sociological rationale for the specifications which the author settles upon – "specific mechanisms" in the words of one of the referees. The critic thus claims that Hickey's reply does not satisfactorily rebut these three objections.

Hickey: The three criteria amount to politically correct ideas for the *American Journal of Sociology* under Laumann. This critic is just spouting more dogma in the "mechanisms" argot – the romantic social-psychological reduction seduction again. He is as obdurate as the other two, repeating the above criticisms and simply dismissing Hickey's rejoinders. Claiming that Hickey "does not understand" is unmitigated arrogance. It is Laumann's referee who obdurately refuses to understand.

In fact it may be said that Hickey did identify a "specific mechanism", the integrative mechanism consisting of negative-feedback relations due to the interinstitutional cultural configuration of value orientations that pattern the propagation of social change through the system of types of institutional groups. Specifically it is the macrosocial stabilizing negative feedback relation due to increases in the high school completion rate, when there is sufficient economic growth. Notwithstanding that education is recognized as socialization, the macrosocial outcome is not obvious because it is intergenerational, and furthermore its effect is commingled with the interaction among all the other variables. But its operation was exhibited in the Type II simulation described in the paper. And it is a macrosociological interinstitutional relation and not a social-psychological relation that this doctrinaire critic will only accept as a "convincing sociological rationale".

The first objection is just more Luddite ranting. The remaining two objections have been rejected in the history of macroeconomics, when economists unsuccessfully attempted to create a macroeconomics that is an extension of economic psychology, the maximizing rationality postulates of microeconomics. But economists recognized that macrolevel social analyses cannot successfully be reduced to microlevel individual analyses, because it incurs the logical fallacy of composition. Sociology is not an exception. In Parsons' terms, macrosociology is "emergentist", but contrary to Parsons it

means that macrosociology cannot succeed as a reduction to an individualistic microlevel social psychology of motivational analysis. The social system has outcomes not possessed or exhibited by its component members.

But Laumann is a Parsonsian-era Harvard sociology graduate. He attended Harvard during the apogee years of Parsonsian influence with its requirement for motivational analyses that romantics like to call "mechanisms". Any such deeply held vision comes to form part of the very identity of the believer, and any threat to the vision is experienced as a threat to the believer himself. Therefore Hickey is convinced that Laumann simply did not want a paper such as Hickey's published anywhere much less in his *American Journal of Sociology*. He believes that the referee criticisms Laumann accepted reveal that he is an agenda-driven editor, that his choice of classical romantic referees was an ambush selection for a paper like Hickey's, and that he got the kind of criticisms he wanted. Laumann's reaction to Hickey's paper is comparable to the "Last Sociologist" lament that Harvard's Orlando Patterson published as an OP-ED in *New York Times*.

But outside the defensive ramparts of Laumann's reactionary *American Journal of Sociology* things are changing. Recently as economists had seen in Keynes' "paradox of thrift", sociologists have recognized the logical fallacy of composition; just as houses need not have the rectangular shape of their component bricks, so too explanation of the macrosociety need not be in terms of the motivations of the society's component individual members. For example in their "Quest for Institutional Recognition" in *Social Forces* (1998) sociologists Keith and Babchuk report that extension of individualistic microlevel social psychology, which they refer to as the "traditional individualistic *modus operandi*", commits what they call the "individualistic fallacy." These insights are not new. In the nineteenth century Durkheim, who viewed reductionism as threatening to sociology's autonomy, went so far as to argue that whenever a social phenomenon is explained by a psychological phenomenon, the explanation is false.

Laumann's choice of classical romantic sociologists for his referees brings to mind the report by Nobel-laureate economist Paul Samuelson, who wrote in *Keynes General Theory: Reports of Three Decades* that Keynes' theory caught most economists under the age of thirty-five with the

unexpected virulence of a disease first attacking and decimating an isolated tribe of South Sea islanders, while older economists were immune. This development was due to the pragmatism demanded by the Great Depression, when there was little patience with the doctrinairism of the conceited classical economists. Likewise Laumann's classical sociologists are too ignorant of contemporary pragmatism and too inadequate in quantitative techniques – and too doctrinaire – to be inspired by the opportunities offered by new ideas, as were the young Keynesians.

Hickey expects that Laumann will impose his social-psychological reductionism, *i.e.*, "specific mechanisms", on his sociology students for the remainder of Laumann's academic career. He finds Laumann's social-psychological reductionism suggestive of the *Völckerpsychologie* movement advocated by the German historicist Wilhelm Dilthey and his mid-nineteenth century sympathizers in their self-identified "Suicide Club". Hickey believes that such classical romanticism has been so retarding as effectively to be suicidal for the maturation of academic sociology into an empirical science. Sociology at University of Chicago was once pioneering. But Hickey counts Laumann's *gefolgschaft* of referees among the "extinct volcanoes" that Harvard University President Lawrence Summers would bypass for tenure.

American Sociological Review

The third academic sociological journal to which Hickey had sent his paper was *American Sociological Review* (ASR), the official journal of the American Sociological Association (ASA), which is published by the association and edited by a 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. Form edits his journal like a factory manager, who practices standardized production quality control. The resulting conformism results in publishing hackneyed outputs.

Form's first criticism and Hickey's rejoinders

Form: Form's first chosen referee says that Hickey's "metatheoretical considerations" do not **motivate** the actual analyses effectively, that little useful theory is involved, and that the particular analyses are similarly little motivated. He notes that none of them reflect the usually long traditions of

research attempting to explain the variables involved. He called the paper "an empiricist venture" that is "utterly and ineffectively" related to the "empirical traditions" explaining the birth rate, homicide rate, etc.

Hickey: This referee's reference to motivation is ambiguous. On the one hand whether or not a referee is adequately motivated is irrelevant to the validity of the model. If Hickey's "metatheoretical considerations" do not motivate this referee to accept the paper, it is because the referee is innocent of contemporary pragmatism and inadequate to the mathematical techniques. On the other hand if the critic is demanding that Hickey describe motivations in his theory, the critic is apparently attempting to impose a social-psychological reductionist agenda on Hickey's modeling work. The inadequacy of social psychological motivational analysis has been pointed out by many social scientists such as Robert Merton and Wesley Mitchell. The social-psychological reductionist agenda fails to recognize the existence of the distinctively macrosocial perspective that captures unintended and unforeseen – and thus unmotivated – consequences. **Macrosociology is not just large-scale social psychology of motivations.**

With respect to "useful theory" Hickey notes that utility is a sufficient condition for applied research, but is not a necessary condition in basic research. The mentality of Form's chosen referee makes the referee unfit for basic research; he should have taken up occupational social work. Much basic research such as modern astronomical cosmology would be rejected as "useless" by this referee. In fact Hickey's model is useful as basic research, because it is informative; it demonstrates **empirically** the strategic rôle both of rising per capita real income growth rates and of rising secondary-education completion rates for increasing macrosocial stabilizing consensus including rising voluntary conformity with criminal law proscribing homicide. The **useful** social policy implication of Hickey's model is that increased public funding for universal public education together with progrowth Federal government macroeconomic policies increase macrosocial stability. Furthermore Hickey found his macrosociological model's equation specifications **useful** for creating an Institutionalist macrosocio-econometric model of the American national economy for long-term economic-development policy analyses for the Division of Economic Analysis, Indiana Department of Commerce.

The appeal to tradition by Form's chosen referee is truly appalling. It defies parody. It both reflects and explains academic sociology's chronic

decadence and protracted stagnation. Appeals to tradition are symptomatic of intellectual stasis, of lethargy due to indolent complacency, and of obstructing inertia of the comfortably familiar. Journals serving advancing empirical sciences seek to publish new and original research instead of tradition-bound hackwork. Sociology can never become a science, until it has become an empiricist venture. What is remarkable is not that science is an "empiricist venture", but that sociologists like Form's chosen referee need to be told that it is. This referee's implication that sociology is not an empiricist venture explains why other sciences have demonstrated the progress that sociology has not, and why sociology exhibits its chronic legitimacy crisis.

Form: Form's first chosen referee goes on to say that the empirical results reflect mainly trends in the variables, and that substantial secular trends are involved, so that some high correlations "naturally" result, which should not without much more thought be made the basis for causal inference.

Hickey: Had Form's chosen referee actually attempted modeling, he would know that the task is not so easy as his dismissive rhetoric alleges. Hickey doubts that Form's chosen referee even looked at the time-series data. Hickey had detrended the longitudinal data by transforming the per capita rates into growth ratios, to minimize collinearity among the models' independent variables. These growth ratios have higher variances thus making empirically adequate modeling more difficult. This fact would be evident to an experienced modeler, but Hickey's modeling is evidently beyond the competence of Form's chosen referee.

Hickey's models are causal models, not trend models. The critic's phrase "more thought" is argot meaning speculation about motives, which is gratuitous. There is no need for any causal "inference" other than the satisfactory statistical inference employed. Hickey's causal claims are based on the pragmatist thesis of ontological relativity with the semantics supplied by the context consisting of the descriptions in the data sources and the empirically adequate equations of the model. They do not represent fifty years of coincidence. But Form's chosen referee is blissfully innocent of contemporary philosophy. He has attempted to force Hickey to submit the only kind of work the referee can understand.

Form's second criticism and Hickey's rejoinders

Form: Form's second chosen referee wrote a shabby criticism containing many "X" overstrikes, and he appears to have given minimal time and superficial thought to Hickey's paper. Form's second chosen referee said that the paper is "ambitious", and that its results are "questionable" and "meaningless". The critic admits that while the estimated equations seem to make numerical sense in that they satisfy certain statistical criteria, they do not make "substantive sense". He claims that demographers will be "amazed" with the finding that the decrease in the homicide rate causes an increase in the birth rate, that an increase in the birth rate twelve years earlier causes a decrease in the growth of the marriage rate, and that criminologists will be "surprised" to learn that an increase in the growth rate of the birth rate eight years earlier leads to a decrease in the growth rate of the homicide rate. He says that similar "puzzling findings" can be found in all the equations.

Hickey: Form's second chosen referee's term "meaningless" is uninformative except as an expression of disapproval. The semantics is exhibited by the descriptions of the data and by the equations of the model. Empirical findings are not wrong because they are unexpected – "amazing" or "surprising". Saying that demographers will be "amazed" and that criminologists will be "surprised" is a cheap shot, which Hickey believes successfully panicked Form. In science amazing and surprising empirically adequate findings are characteristic of significant advances. Saying that the equations do not make "substantive sense" is uninformative and echoes Laumann's referee who demanded "substantive reasoning", a distinctively romantic *verstehen* thesis. It shows a failure to understand relativized semantics, the semantical thesis that is well known to linguists and contemporary philosophers of science.

Equation (1) determining changes in the crude birth rate says that changes in the crude birth rate are directly related to changes in compliance with criminal law prohibiting homicide. Increased compliance indicates increased macrosocial integration and social stability, whereas social disintegration and rising chaos discourage procreation.

This referee has merely glanced at the equations without considering the information exhibited by the simulation and shock analyses. Equation (2) says that changes in the per capita marriage rates are inversely related to

changes in the crude birth rates eight to twelve years earlier. But in this *cyclical* model the marriage rate changes are therefore directly, *i.e.*, *positively* related to birth rate changes twenty to twenty-four years earlier as exhibited in the type II simulation, because the model captures the life cycle in changes in the national demographic profile. The twentieth-century's manifest demographic cycles are well known to demographers and even to some sociologists. They are more popularly known as the "post-war baby boom", followed by the "birth dearth" or "generation X", followed by the "echo from the baby boom" or generation Y, etc. By specifying the equations as they are, the sample data offer more degrees of freedom for statistical estimation.

Again this referee has merely glanced at the equations without considering the information exhibited by the simulation and shock analyses. Equation (3) determining changes in the per capita rates of compliance with criminal law proscribing homicide might "surprise" Form's second chosen referee. But Norman Ryder, the demographer who published in *American Sociological Review* in 1965, would likely not be surprised that an increase in the growth rate of the birth rate with a lag of four to eight years leads to an increase in compliance with criminal law proscribing murder. It is certainly not the four-year-old to eight-year-old children who stop committing crimes, but as Aberle notes in *America as a Mass Society* the increased compliance is by their parents whose domestic responsibilities motivate them both to look to their own and their children's futures and to be responsible law-abiding parents. Thus Ryder memorably wrote that nothing makes a young generation settle down more quickly than the younger generation coming up.

Furthermore in this **cyclical** model the sixteen-year delay in the change rate of the crude birth rates implies a negative relation to changes in the per capita rate of compliance with criminal law, because teens are less adequately socialized than are adults. Criminologists know very well that national crime rates are greatly influenced by changes in the national demographic profile, and that teenagers are less compliant, especially where they have supplied the foot soldiers for criminal gangs engaging in murderous turf wars. Also examination of research findings reported in the Federal Bureau of Investigation's *Uniform Crime Statistics* reveals that the factors in Hickey's equations would not "surprise" the informed criminologist, because several of Hickey's factors are also in the FBI's cross-sectional regressions.

To Form's pontificating critic "ambitious" authors are heretics who must literally be **excommunicated** from sociology, *i.e.*, denied an opportunity to communicate in the peer-reviewed literature. But to attack ambition is to defend mediocrity. This referee demands hack sociology although neither he nor Form would ever admit it. In empirical science there is no reason why a new theory should not be "ambitious" or why new findings should not "surprise" or even "amaze" cognizant professionals. And in progressive sciences (unlike hackneyed sociology) that is precisely what happens – and what gets published.

This critic's demand that the model should make "substantive sense" is classic *verstehen*, hackwork in which familiarity – if not banality – is the criterion for scientific criticism, which in no small part has produced sociology's chronic stagnation. For pragmatists and linguists the semantics is what one extracts from the linguistic context including the empirically adequate model, and not some presumption that one brings to the theory a priori. Thus Hickey echoes Lundberg who said that understanding is not a method of research, but rather is the end to which the methods aim. This referee's criticism explains why sociology has been dismissed as merely "platitudes couched in jargon". Hickey views this critic's rejection of "surprising" and "puzzling" findings as professionally disreputable, because it is a disincentive for quantitative empirical research yielding new empirically "substantive" findings.

Form: Form's second chosen referee claims that the main problem is that Hickey has not explicated a "theory" underlying the causal assertions embodied in these equations, and that the causal chains, if they exist at all, are very "loose and indirect". He says that he does not deny that by computing suitable moving averages and growth rate indexes and by experimenting with lag structures, the author can find equations that seem to fit the historical record.

Hickey: What does the critic understand as "indirect", much less "loose"? A criminal conviction for murder involves establishing a motive for the crime. But is the motive the direct cause of death? Is not the gun that the murderer used more direct than the murderer's motive? But is the gun truly a direct cause? Is not the bullet discharged from the gun more direct? But is not the wound inflicted by the bullet the more direct cause of death? A physician could advance still more proximate causes thus

revealing that "indirect" is an objectionable reductionist objection to the relations expressed in Hickey's model.

Hickey's empirically adequate model is revealing and sociologically significant. As shown in the simulations, the iterations of the model exhibit the propagation patterns of influence and their outcomes thereby revealing the net interinstitutional value reinforcement or conflict within the whole macrosocial system. There is more to understanding this dynamic model than just glancing at the regression estimated equations. And incidentally, since the equations in Hickey's model are all monotonic, it is possible to express any variable as a function of any other in the model by simple algebraic substitution.

Form: Form's chosen referee questions the "meaningfulness" of the equations, and adds that the root of this unclarity is Hickey's "silly" idea of "theory". He equates Hickey's view of "theory" to saying that the empirical equations for the motion of a "spring-mass system" constitutes a theory that are distinct from the empirical equations for the "motion of a rocket leaving the earth", because in both cases, the numerical equations are models of the phenomena in question deriving from the underlying theory of Newtonian mechanics. He says that a theory might help readers to accept as valid the causal specifications in the author's model, which have very little face validity in the absence of such a theory.

Hickey: The statement by Form's chosen referee that Hickey's paper has no "theory" is odd given that functionalism has long been viewed by sociologists as a sociological theory, and the title of Hickey's paper identifies the model as a post-classical quantitative-functionalist macrosociological theory. But Hickey denies that classical functionalist sociology is scientific theory, until the discourse is rendered testable and proposed for testing, as Hickey has done with his post-classical functionalist theory. Until it is empirically testable, traditional sociological functionalism continues to be just another descriptive grand narrative like practically all the sociological "theory" taught by sociology professors.

Like nineteenth-century positivists this critic has taken Newton's mechanics as found in undergraduate textbooks as a paradigm for his concept of theory in research science. In the "Introduction" to his magisterial *Patterns of Discovery: An Inquiry into the Conceptual Foundations of Science* (1958), Yale University pragmatist philosopher of

science Norwood Russell Hanson wrote that earlier philosophers of science had mistakenly regarded as paradigms of inquiry finished systems like Newton's planetary mechanics instead of the unsettled, dynamic research sciences like contemporary microphysics. Hanson explains that such finished systems are no longer research sciences, although they were at one time. He states that distinctions applying to the finished systems, which he calls "catalogue science" as opposed to "research science", ought to be suspect when transferred to research disciplines, and that such distinctions afford an artificial account of the activities in which Kepler, Galileo and Newton were actually engaged. He thus maintains that ideas such as ***theory***, ***hypothesis***, ***law***, ***causality*** and ***principle*** if drawn from what he calls the finished "catalogue-sciences" found in undergraduate textbooks will ill prepare one for understanding research-science.

Hickey maintains that due to their almanac concepts of theory these sociology referees, not to mention the editors who selected them, are ill prepared to practice research science. Following Hanson's functional pragmatism Hickey says that in research science "theory" is language at the cusp of scientific change in basic scientific research under the regulation of empirical criticism, the kind of change that constitutes productive work and enables scientific progress. But this referee's "catalogue-science" view of theory has been an impediment to the realization of sociology as an empirical science.

Form's second chosen referee says that both the empirical equations for the motion of a spring-mass system and the empirical equations for the motion of a rocket leaving the earth are numerical equations derived from the underlying Newtonian theory. This "catalogue-sciences" view of the meaning of "theory" suggests that of the sociologist Kenneth Land. In an article titled "Formal Theory" in *Sociological Methodology* (1971) Land offers a curious eclecticism that starts with the Hempel-Oppenheim positivist deductive-nomological concept of scientific explanation and ends up with Land's version of the Haavelmo romantic structural-equation agenda for econometric modeling. In other words Land conceives theory as an organization of language as found in textbooks.

In fact his so-called "formal theory" is not formal. Its semantical interpretation is specific to his demographic-accounting agenda with its population stocks and "transition coefficients". In his 1971 work Land explicitly defines scientific theory as a set of concepts and propositions

asserting relationships among concepts instead of recognizing it as a transitory stage in achieving the aim of science. And he says that a distinguishing characteristic of a theory is that it cannot explain specific events without "prior transformation", because theoretical propositions are general statements about causal relationships among the concepts in the theory, while there must be an observation record. Hickey comments that "theory" is not a special **type** of language, but rather is a special **use** of language; it is language that is **proposed for testing** in contrast to test design language that is **presumed for testing**. The pragmatics that defines theory is empirical testing.

Land then asks how to traverse the supposed "gap" between general theory and description of specific observations of empirical events. He thus assigned to empirical models the rôle that Hempel had assigned to empirical laws (before Hempel had read Quine's "Two Dogmas of Empiricism", and consequently in his "Theoretician's Dilemma" in *Minnesota Studies* reconsidered positivism altogether). Land's alleged "gap" is a pseudo problem. Functionally the defining feature of theory language is not its level of generality or its axiomatic organization. Rather it is the language that is proposed for testing, because its claims are judged to be relatively more hypothetical than the theory's test-design language that is presumed for testing. The cognizant scientists thus agree that in the event of a falsifying test outcome the theory is the language that is in need of revision in contrast to the language describing the test design, although some scientists may revise their decision and practice counterinduction. And when the test outcome is falsification, then the theory is no longer a theory, but is merely rejected language. But if the test outcome is not falsification, then the theory is no longer so hypothetical as to function as theory language, because it has been tested, and the test outcome has made it an empirically warranted scientific law that can be used for scientific explanation and for test-design language for testing some other theory.

Land's collaborator, Alex Michalos, has a similarly anachronistic view of theory. In his "Philosophy of Science" in *The Culture of Science, Technology and Medicine* (1980) Michalos references Hanson and calls Hanson's thesis about research disciplines a "functional" as opposed to a "formal" view of scientific theory. This is a valid distinction, because Hanson's pragmatic view is indeed functional for the practice of basic empirical research. But Michalos took the reactionary turn. In a "Prologue" co-authored with Land in *Handbook of Social Indicators and Quality of Life*

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(2012) edited by Land, Michalos and Sirgy, Michalos expresses his preference for the concept of theory as an axiomatic system, which is their concept of "formal theory". Such "formal theory" is paradigmatic of what Hanson called "catalogue science", because it is merely an organization of knowledge as an axiomatic system.

Like Land and Michalos, this critic has a similarly prosaic understanding of empirical science derived from the catalogue-science usage that is found in undergraduate textbooks describing the finished research findings of completed science, often completed many many years ago. The "theory" in the textbooks is like a museum taxidermy display of an animal's stuffed carcass that cannot exhibit the repertory of animated behaviors of the living creature in its struggle for survival in the wild.

Therefore like Land and Michalos, Form's second chosen referee is therefore ill-prepared for understanding research science and ill-prepared for practicing it, much less for contributing to its advancement. He and his ilk are destined to spend their careers in pursuit of a delusional Holy Grail for macrosociology based on their textbook stereotypes supplied by Newtonian mechanics. Contrary to this critic's ridicule, Hickey's view of scientific theory is not "silly", and referring to it as such is an exhibition of egregious knownothingism. Hickey's view of scientific theory is pragmatist since *the pragmatics of theory language is empirical testing*, which is what makes theory strategically functional for advancing empirical science in the practice of research science. Were sociologists to accept the contemporary pragmatist philosophy of science, they might do less wayward and ersatz philosophical dithering and more serious and productive modeling.

Hickey does not say in his paper that he used moving averages, although moving averages would serve his purpose adequately. The critic does not recognize that four-year moving averages over annual data would produce a smoothed annually incremented time series. Hickey used four-year incremented time periods for period averages. The use of four-year periods simplifies modeling, because there is then no imperative for complicated distributed-lag structures to relate a lagged-valued explanatory variable to the dependent variable of an equation, as would often be necessary with annually or shorter incremented time series. And the smoothing effect of the four-year periods also greatly mitigates outliers and removes noise from the data for which no model could account. While annual national-level data are already so aggregate that such noisy

irregularities are usually negligible anyhow, the change ratios typically have higher variances than the measurements from which they are calculated.

In addition to simplification, there is also an incidental reason for the four-year period averages. One of the input variables to the discovery system is the political party affiliation of the U.S. President, who holds office for four-year terms. This was the only variable representing sociology's conflict thesis that was inputted into the discovery system for developing the macrosociometric model, although it was not selected by the discovery system for any of the outputted models.

Finally saying that an underlying theory like Newton's might help readers accept as valid the causal specifications in the Hickey's mode is ironic, because Newton's contemporaries, Leibniz and Huygens, had the same difficulty with Newton's gravitational theory that this referee has with Hickey's macrosociological theory. These contemporaries of Newton criticized Newton's physics for admitting action at a distance; both Newton's contemporaries were convinced that all physical change must occur through **direct** physical impact like colliding billiard balls, and Leibniz therefore rejected Newton's concept of gravity as an "occult quantity". Like this referee they too were unhappily "surprised" and "amazed", and found the theory lacking what this referee calls "face validity". In describing Newton's theory as "unintelligible" Leibniz might have said that Newton fails to make "substantive sense".

In his *Concept of the Positron* Hanson distinguishes three stages in the process of the evolution of a new concept of explanation; he calls them the black box, the gray-box, and the glass box. In the initial **black-box stage**, there is an algorithmic novelty, a new formalism, which is able to account for the phenomena. The equations in Hickey's model are a black box for this referee. After some time scientists use this algorithmic novelty, but they then attempt to translate its results into the more familiar terms of the prevailing orthodoxy, in order to provide "understanding". Romantic sociologists such as this referee could use the word "*verstehen*" here, even though he is too incompetent to use the formalism to understand the simulations. In the second stage, the **gray-box stage**, the new formalism makes superior predictions in comparison to the older alternative, but it is still viewed as offering no "understanding". Nonetheless it is suspected as having some structure that is in common with the reality it predicts. This referee has not reached this stage. In the third and final stage, the **glass-box**

stage, the success of the new theory will have so permeated the operation and techniques of the body of the science that its structure will also appear as the proper pattern of scientific inquiry. For example in the nineteenth century Helmholtz wrote that Newton's theory has become the paradigm of explanation in physics; it had become what Hanson called a glass box. With Einstein's general relativity theory, gravity has been reconceptualized again, and Newton's glass box was as it were broken except for those reactionary physicists who continued to find Einstein's theory a black box. Today Keynes' macroeconomics has the status of a glass box for economists. But Hickey's post-classical functionalist macrosociometric model is still a black box for the reactionary referees selected by Form and the other editors.

Form: Form's second chosen referee accuses Hickey of distorting Land's position, and claims that Land did not argue that it is imperative to take the equation specifications of sociological models from existing colloquially expressed theory, but rather just stated realistically that this is the typical level of most current sociological theory. With suspiciously miraculous clairvoyance the critic reports that Land would be the first to admit that a theory can be mathematically stated. For a general theory he again references Newtonian theory, as a paradigm that he says would be much better, if such could be constructed for sociology.

Hickey: This "distortion" rhetoric suggests that the critic really knows how to yo-yo an editor. In fact Hickey's issue is not about Land's views on mathematically vs. colloquially expressed language, although the critic is correct in saying that colloquially expressed theory is how romantic sociology with its motivational descriptions is typically expressed. Hickey's issue is about the source and justification of the equation specification for models, *i.e.*, of the selection of explanatory variables for any model, which is the central issue for this referee, and its semantical interpretation. Hickey affirms that extraction by data mining is a valid source for equation specification, that empirical adequacy is adequate justification, and that its interpretation is by relativized semantics.

In his "Social Indicators" article in the *Annual Review of Sociology* (1983) Land disapprovingly states that analysis of demographic time series have employed *ad hoc* combinations of econometric and time-series techniques that ignore underlying population dynamics of the social-indicator time series. Hickey finds this description suggestive of Land's attempts at macrosociological modeling in 1975, which depends on

autoregressive variables. Autoregressive variables capture the effects of missing explanatory variables, and thus are a way to make bad equations look statistically acceptable. The equations in Hickey's macrosociological model contain no autoregressive variables.

Contrary both to Land and Michalos and to Form's second chosen referee *empirical testing is the pragmatics of theory language, and is the defining characteristic of theory in research science; thus <u>the theory is the model and the model is the theory</u>.* But in his 1971 paper Land distinguishes theory and statistical model, and gave no hint that he would accept data mining as the source for the equation specifications, accept empirical adequacy as their justification, or accept relativized semantical interpretation. Meanwhile longitudinal modeling in sociology stalls.

Form's rejection letter

Hickey submitted his rejoinders to Form on 6 May 1981. On 14 May 1981 Hickey received a petulant drop-dead rejection letter exhibiting Form's hubris and saying: "Apparently you do not understand the folkways of our profession. I sent your manuscript out for review and wrote you that your article was rejected for publication. Then I received a revision of your article with the stated expectation that it should be published." Form then added that it is not "normative" for an article to be resubmitted once it is rejected, and that if this were not the practice, he would spend the rest of his life re-reviewing the same manuscript. Hickey had not revised his paper for Form.

Sociology's peer-reviewed literature actually operates as a filter to remove original theses and to disseminate hackwork. Hickey can only view Form's letter as contemptuous, and doubts that Form gave Hickey's rejoinders even a passing glance, if any consideration at all. Form's dismissive practice is neither "normative" nor normal. In fact Hickey regards Form's comment as cynically disingenuous, because in the interest of their readerships editors of scholarly journals routinely consider authors' rejoinders to referees. Hickey believes that consideration of a resubmitted paper with a view to the author's rejoinders is in practice "normative", if the editor wishes to judge a paper on its intrinsic merits, because *no referee criticisms are above or beyond criticism*. Form's editorial practice due to his rôle concept as an editor effectively operate as a filter to suppress original work and to disseminate hackwork.

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Hickey does not believe Form's claim about any such alleged "folkways". Just three years earlier in "Editorial Policies and Practices among Leading Journals in Four Scientific Fields" in the journal *Sociological Quarterly* Janice M. Beyer reports her findings from a survey of the editors of a representative sample of academic journals serving several sciences. She found that ***forty-three percent of all the papers <u>accepted by sociology journals</u> have been resubmitted***. Furthermore the American Sociological Association rents mailing lists to booksellers, and its web site offers a list of **over a thousand members' names and addresses that identify themselves as "quantitative sociologists".** Are all these sociologists so incompetent that finding an adequate referee for Hickey's paper would take a lifelong search? But perhaps they are incompetent, because the above referee criticisms strongly suggest they are, if the criticisms are not merely fatuous. Hickey gives Form's rejection letter ten out of ten points for sheer chutzpah.

Form concluded his rejection letter by saying that he hoped that Hickey would submit his revised manuscript to another journal and profit by the suggestions of their referees. Hickey believes this comment is buck-passing by an editor who had failed (either by ignorance or by intent) to obtain patronage for the submitted paper. Hickey in turn hopes that Form and his chosen referees may profit from Hickey's rejoinders published herein, which Form had contemptuously dismissed. Form titled his autobiography *Work and Academic Politics*. Hickey views Form as a politician plying his "work and academic politics" as an editor.

A librarian at the United States Library of Congress once told Hickey that journal editors choose to publish papers they like, and that their likes and dislikes can be quite biased. Therefore Hickey was not altogether surprised to discover later that Form has his own alternative approach to institutional analysis that Form described using a 1950's-vintage approach, in which Hickey found no modeling analysis. Form described his approach in "Institutional Analysis: An Organizational Approach" in a book titled *Change in Societal Institutions* (1990), which he also summarized later in his autobiography, *Work and Academic Politics* (2002). In the 1990 book Form references his earlier *Industry, Labor and Community* (1960) as an illustration of his organizational approach, which is a repeat of his still earlier *Industrial Sociology* (1951). His "organizational approach" to institutional change was Form's style of sociology long before he received Hickey's submission with the dynamic modeling approach. Such is the

politics of Form's "work and academic politics".

In his autobiographical *Work and Academic Politics* Form wrote that as the editor of the *American Sociological Review* he read every manuscript submitted to his journal and wrote his own internal review for every manuscript submission. But Hickey has found no evidence in Form's literary corpus that Form has even the minimal technical competence for critiquing Hickey's paper, much less the requisite sophistication in contemporary philosophy of science. Given Form's statement in his autobiography Hickey suspects that the first of the two critiques he received from Form, the one affirming traditionalism, was actually written by Form, and that it expresses Form's personal preferences in sociology.

Hickey thought that with Form he had reached the nadir, and that the contemptuousness of sociology editors could not be worse. But no, read on.

Social Indicators Research

Hickey lastly submitted his paper to a journal called *Social Indicators Research* edited by an Alex C. Michalos. Michalos has been a co-author with Kenneth Land, and Land is listed as a member of the editorial board, *i.e.*, a referee, for Michalos' journal. Michalos was identified on the journal's stationery as Director of the Social Indicators Research Programme at the University of Geulph in Ontario, Canada. The journal's publisher is identified as D. Reidel Publishing Company, Dordrecht, Holland, and Boston, U.S.A. The 1995 edition of the *National Faculty Directory* listed Michalos as a faculty member of the Department of Philosophy at the University at Guelph.

Michalos' rejection letter

This journal also rejected Hickey's paper, but it cannot be treated as the others discussed above, because Michalos refused to inform Hickey of his reasons for rejection. Michalos acknowledged 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 Hickey's case the reviewer "pretty well threw up his hands". Hickey just threw up, and then wrote a letter dated 12 February demanding two

written referee comments for the opportunity to submit rejoinders.

Michalos responded with a letter dated 22 February 1982 replying that sometimes his reviewers are "brutal", and that when the first reviewer is exceptionally critical, he does not go to a second reviewer. Michalos concluded his letter by saying that he had sent Hickey "all he had". This is outrageous chutzpah – like the man who murders both of his parents, and then demands the court's mercy claiming that he is an orphan. What is manifestly "brutal" is the mugging by this referee who made this editor his fawning client. If, as is said, the peer-reviewed literature is the "court of science", Michalos' journal is truly a kangaroo court, because the reader may well wonder what referee has so enthralled this editor as to motivate such a dismissively cavalier editorial practice of secret criticism. ***No referee is above criticism,*** and that Hickey should be allowed his replies.

Hickey's comments

In "Positivism versus the Hermeneutic-Dialectic School" in *Theoria* (1969), which is a critique of *Continental Schools of Metascience* by Gerard Radnitzky, Michalos identifies himself as a positivist. There he states that the aim of science is to be a coherent and well organized corpus of scientific knowledge, which he also describes as systematization of scientific knowledge. A more recent book titled *Handbook of Social Indicators and Quality of Life* edited by Land, Michalos and Sirgy contains a "Prologue" co-authored by Land and Michalos, in which "theory" is described explicitly as an axiomatic system. Michalos' positivistic definition of scientific theory as an axiom system is anachronistic philosophy of science.

Academic philosophers have long recognized that the philosophy profession is in the postpositivist era, a fact that has escaped the notice of Michalos and Land. In the "Introduction" to his *Patterns of Discovery: An Inquiry into the Conceptual Foundations of Science* Yale University pragmatist philosopher Norwood Russell Hanson calls the positivist taxonomic understanding of science "catalogue-science". "Catalogue science" contrasts with "research science", the contemporary pragmatist functional view, which defines "theory" as transitional language at the cusp of continuing evolutionary and sometimes revolutionary change that yields new laws and explanations. And in his *Observation and Explanation* Hanson ridicules positivists as "axiomatizers", who aim to formalize an explanation for exposition.

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The axiomatizers deliver the seductive psychological satisfaction that comes with a coherent description of the world, or at least of some domain. Such is the metaphysician's stock in trade. But unlike metaphysics science is empirical. So, it is no wonder that a new finding such as the quantum of action that shattered the old Newtonian system of thought may be so disorienting to the complacent professionals that it took a later generation to adjust. It is only recently that philosophers of science have recognized that – contrary to positivists – progress in science does not consist in new axiom systems, but rather consists in new empirical findings. Thus Hickey agrees with Hanson; he views axiomitizers as idlers who prefer entertaining puzzle solving to consequential problem solving. Hickey believes it is unlikely that these ersatz philosophizing sociologists recognize they are atavistic dinosaurs, whose isolation in academic sociology has enabled them to survive the extinction of positivism.

However, it may be added incidentally that in Hickey's macrosociological model none of his equations can be logically derived from any others, because each has its unique dependent variable. Therefore each of his equations is an axiom in the equation system that constitutes his model. Furthermore implicit relations among any of the variables in the monotonic linear equations may be derived mathematically as theorems by simple substitution.

Consider Land's approach to using demographic data for modeling: Land proposed his modeling approach in "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), and also later in his "Modeling Macro Social Change" in *Sociological Methodology* (1980). Land uses ideas from a 1971 monograph titled *Demographic Accounting and Model Building* by Richard Stone and published by the Organization for Economic Cooperation and Development. Conceptually Stone's demographic accounting system is an inventory accounting system as might be found in a retail business enterprise. The demographic inventory has beginning and ending population stocks, and has population inflows and outflows determining the net change in the stocks over an accounting period.

Stone proposes that the data flows may be structured analogously to Wassily Leontief's economic input-output tableaus. Land calls the calculated coefficients in the demographic input-output tableau "transition"

coefficients. Since these transition coefficients will change from period to period, Land proposes using the econometric type of longitudinal model estimated over the time series of transition coefficients, which he furthermore says in the 1976 paper could be interpreted as measures of opportunities for social benefits. He therefore calls this his "opportunity-structure" approach based on ideas originally proposed by the early twentieth-century sociologist William F. Ogburn.

On the other hand Hickey's construction of national per capita rates as measures of macrosocial consensus exhibits sociological relevance and access to the watershed of demographic data collected and released by the several cognizant Federal government agencies. The sociological relevance of these demographic time series gives data more than just demographic significance, because it enables distinctively macrosociological modeling describing interinstitutional interactions propagating changes in degrees of consensus affecting macrosocial stability through time as revealed by the model's iterations.

Having an academic philosopher for its editor might have been singularly fortunate for Michalos' *Social Indicators Research* journal as well as for the journal's readers. But Michalos is a self-confessed positivist and no pragmatist. Given that Michalos refused to inform Hickey of the reasons for rejection aside from "brutality", what Hickey encountered in his correspondences with Michalos is an editorial practice comparable to Franz Kafka's absurdist story *The Trial*, in which an accused man is arrested, tried, condemned and executed without ever having been informed of the accusations made against him. Kafka wrote stories that *Webster's Dictionary of the English Language* describes as "sordidly unreal". Hickey found the editorial practices of Michalos' *Social Indicators Research* Kafkaesque, *i.e.*, sordidly unreal.

The Internet shows Michalos has since moved to the University of Northern British Columbia's political science faculty, which is probably a beneficial transition for Geulph's philosophy students. And he is probably uniquely qualified to teach political science given his experience in academic politics as an editor.

APPENDIX III

A Critique of Sociology's Literature

Hickey's responses to the above attempted referee criticisms of his paper have been strategically naïve: he has somewhat fatuously assumed that the criticisms were in fact the operative motivations for rejecting his paper. In fact this affected *naïveté* is not altogether without validity, because there are indeed fundamental differences between the contemporary pragmatist philosophy used by Hickey in his paper and both the romantic and positivist philosophies used in the criticisms, which the editors chose to enforce. And there are also manifest differences in levels of technical competence between Hickey and both the editors and their chosen referees. Sociology is truly a backward academic occupation. But sociologists in their bubble of delusion are such useful pariahs for contemporary philosophers that if sociologists did not exist, philosophers would have to create them.

Rejected evidence

Sociologists **fail** to distinguish between contrary evidence and contrary opinion, because they adhere to irrelevant criteria for scientific criticism. The referees of Hickey's paper believe that mere recitation of their contrary personal preferences (often with ridicule) constitutes criticism of the author's valid empirical findings, and the complicit editors accepted such rhetoric as criticism. This failure enables irrelevant considerations to operate as criteria in the decisions of editors. Ostensibly any submission to a peer-reviewed science journal is evaluated only on its intrinsic merits. But there are stated reasons and there are operative motives, and the stated reasons are not necessarily the same as the operative motives. As 2002 Nobel laureate economist Daniel Kahneman says in his *Thinking Fast and Slow*, even if the stated reasons were refuted the motives would still remain and produce the same decisions. **With rare exception referees always aggressively criticize submitted papers**; in fact "vandalize" might be a better word, since many criticisms are bogus. *This referee practice gives an editor license to use his personal preferences in his decision to publish or reject a paper, while pretentiously citing the referee criticisms as the reasons for his rejection of submissions he personally dislikes.* This bias

creates a systemic dysfunctionality in sociology's peer-reviewed literature that disables the ability of the occupation to function as an empirical science.

Guild politics

In addition to referees' and editors' disabling personal preferences there are other more institutionalized operative motives. Sociologists **refuse** to distinguish between contrary evidence and contrary opinion, because *academic sociology is an exclusive guild.* The guild in academic sociology is a structural perversion that operates as a surreptitious double standard, which filters out work by outsiders, and especially work that is threatening to the guild membership. Sociology journals are for academic sociologists, *i.e.*, the "experts", and presumably everyone else should be dismissed as a meddling "laymen" having nothing of value to tell the ostensibly superior "professionals". But Hickey had identified himself as an econometrician on the letterhead of his submission correspondence to each of the four sociology journals, thus displaying the self-accusing scarlet letter "**E**" for "economist", which made him anathema to sociologists. And as a nonacademic, he was furthermore doomed to Dante's ninth circle. Hickey's submission was thus viewed with narrow and suspicious eyes that recognized a paper not to be legitimated by acceptance in their peer-reviewed sociology literature. His impudent outsider status is implied in the *American Sociological Review's* rejection letter, in which the editor, William H. Form, referred to the "folkways of **our** profession". Hickey found Form's language as effectively saying that Hickey is "not one of us sociologists". And his self-disclosure made him not only an outsider to sociologists, but also a threatening outsider, because – as the referee criticisms revealed – sociologists are not educationally prepared for the econometrician's modeling and simulation techniques, much less the mechanized discovery systems and the contemporary philosophy of science that the referees had explicitly rejected as nontraditional and even "silly".

In his autobiographical *Work and Academic Politics* William H. Form explicitly compared sociology to a guild and referred to himself as a guild "journeyman". Historically a guild was a type of trade association that originated in late mediaeval Europe. Its function was to enforce an exclusive monopoly to protect its members from threatening nontraditional ideas and new technologies practiced by competing outsiders. Clearly a journal editor like Form who thinks of sociology in terms of a mediaeval guild has a very different understanding of his rôle than an editor whose

understanding is defined by the aim of science. Every guild feared the destabilizing effects of innovation's "creative destruction", to use Schumpeter's famous phrase. In the era of the industrial revolution the threatening innovation was mechanization of the crafts, which spawned the Luddites. The criticisms of Hickey's paper by the referees portray sociology's literature as a latter-day guild that protects its members from threatening nontraditional ideas and new technologies of competing outsiders. Specifically the referees and editors are latter-day Luddites seeking to protect their academic fiefdom's turf from threatening work by heretics practicing mechanized theory construction.

The peer-reviewed literature of sociology is flawed with corruption and manipulation. Ensconced academic sociologists have a vested interest in their obstructionist guild politics that enforces their reductionist classical sociological "theory", their retarding romanticist and positivist philosophies of science, and their rejection of mechanized data-driven theorizing. These are properly called ideologies that they either naïvely or cynically enforce to defend their backwater enclaves within otherwise reputable universities. Sociologists' academic status, access to resources, budgets, privileges, paychecks and perks depend upon their academic pretensions, and they defensively seek to protect their occupational sinecures. To many scientists in other university academic departments the title "social scientist" is pretentious panjandrum, when applied to sociologists. And even some securely established academic sociologists share this disdain. For example in his "Ideology, Foundationalism and Sociological Theory" in *Sociological Quarterly* (1993) University of Buffalo's sociologist Mark Gottdiener critically examined sociological theory, and reported that it is merely about verbose language and power-games among theorists seeking to construct grand narratives to sustain their status within an intellectual community. The criticisms of Hickey's paper show that the referees persuaded the complicit editors that he should write what the referees can understand – and what thus protects their incomes and occupational status.

William H. Form is not the only sociologist to describe sociology as a guild, although he is the only one known to Hickey to have employed the comparison approvingly. But Form's approval is not surprising, since his *American Sociological Review* is the flagship journal of the American Sociological Association – the guild's embodiment. In the "Introduction" to their *Sociology on Trial* sociologists Arthur Stein and Maurice Vidich say **sociologists perform the classical functions of a guild so that the task of**

sociology as a profession "gets lost". Hickey maintains that sociology is not only "lost" but has never found itself as a real science. The reason is that s*ociologists have made sociology's guild politics control science instead of letting science's empirical criterion control sociology*.

The result is that sociology is a caricature of a scientific profession. The editors of its peer-reviewed literature are guild politicians, who care less about empirical validity and more about the reputations of their journals with the guild patronage that is their readership and sponsorship; as Form said, "academic politics".   Thus the peer-review process operates as prepublication small-sample market research with the referees operating like focus groups for marketability testing. Instead of pragmatic quality controls as practiced in real science, sociology's editorial practices are defensive social controls as explicitly described in *The Scientific Community* by sociologist Warren Hagstrom. Academic sociologists have good reason to be intimidated by the sociology guild's social controls.   As Hagstrom observes, any sociologist who deviates would have to pay the price of ostracism – denial of tenure and rejection of publication in the peer-reviewed literature – and accept a dead-ended academic career. Consequently the academic sociologist would find it safer to plagiarize Hickey rather than reference him approvingly.   Guild exclusiveness has made sociology so intellectually inbred that its information pool is as degenerate as the gene pool of an incestuous hereditary dynasty. Consequently sociology is slowly becoming sterile. Margaret Wente reported in the *Globe and Mail* (15 May 2012) that there are currently three sociology graduates for every sociology job opening. And in 2015 she lamented sociology professors who are fooled into believing that they might have a shot at the ever-shrinking tenure track.

Cynical "success"

Sociology's corrupt editorial practices fully justify the cynicism expressed by some of its members. For example an atypically candid sociology professor once confidentially told Hickey how to game the system with obsequious rituals to succeed in getting published in the peer-reviewed sociology literature. Compose a paper developing some idea that had previously been published by a living and recognized author, especially if the recognized sociologist is listed by the journal as an "editorial consultant", *i.e.*, referee. Then include in the submitted paper copious footnotes referencing the pedigree-conferring referee, and make flattering and obsequiously laudatory comments about the conferring referee and his

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ideas. This sycophancy nearly guarantees that the editor of the journal will select that referenced pedigree-conferring sociologist to be a referee for the submitted paper, who in turn finds himself the beneficiary of a submitting author who is an unsolicited but invariably welcomed *de facto* public relations agent, because published authors like favorable citations to their papers. The referee will then be motivated to approve the submitted paper for publication and extend his patronage to the submitting author. The same cynical sociology professor also stated that the peer-reviewed literature is the last place to propose any new much less threatening idea. This sociology professor was not joking; she was expressing profound disillusionment with the peer-reviewed literature of her academic occupation.

It would be fatuous to suppose that sociology's editors are clueless about this dance of editors, referees and authors. Its banality minimizes risk to the reputation of the journals, maximizes marketing potential, and makes the peer-reviewed literature a safe social ritual projecting the appearances of a valid and reputable scientific profession. Of course authors and editors who are adroit at gaming this system of guild politics, especially if they have the right affiliations, will self-righteously gush rhetoric that disguises or denies the operative patronage. But the latent dysfunction of this patronage system is that any original finding or "ambitious" idea – especially if it criticizes the conventional wisdom in which a referee or editor has a vested interest – will not get published. Thus exclusive guild politics has made the peer-reviewed literature a self-promoting patronage game that invites, welcomes and promotes academic hacks. A presumed benefit of peer review is establishing readership trust in the quality of an academic journal's published articles for advancing a science. But both the incompetence in the criticisms by the referees and the guild politics in the decisions by the editors such as Hickey found confronting him, are corrosive trust once the practices are disclosed. And the real tragedy is that until empirical adequacy becomes the sole criterion for publishing, this dance will never stop.

Proposed reforms

There have been unsuccessful internal proposals to reform academic sociology. For example in the "Appendix" to his *Coming Crisis in Western Sociology* Alvin Gouldner proposed establishing a critical sociology of sociology that he christens "Reflexive Sociology". He wrote that he aims to *transform the sociologist* and thereby to raise the sociologist's self-awareness. But Gouldner adds that such transformation would be difficult,

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because **"guild interests"** frown upon the "washing of dirty linen in public". Unsurprisingly therefore Gouldner's "Reflexive Sociology" proposal has not been recognized much less implemented in academic sociology in the nearly half century since the publication of his book.

There have also been external proposals to reform academic sociology's peer-reviewed literature. Critical examination of the peer-reviewed literatures of the sciences falls within the purview of information science. In the January 1978 issue of the *Journal of the American Society of Information Science* (JASIS) the editor wrote that referees sometimes use the peer review process as a means to attack a point of view and to suppress the content of a submitted paper, *i.e.*, they attempt censorship. This censorship due to their practice of guild politics is egregious in sociology. Hickey found that the point of view in his paper was attacked by no less than three suppressing agendas, which are the ideology of sociology. They are (1) romantic philosophy of social science, which often included *verstehen* criticism, *i.e.*, folk sociology, (2) social-psychological reductionism requiring motivational explanations, and (3) so-called "formal theory", which is the nonfunctional almanac view of scientific theory taken from the positivists. The editor of JASIS proposed that rather than reject a paper so treated, an editor should publish the submitted paper together with the referee criticisms – and Hickey adds – with the author's rejoinders. Implementation of that recommendation would promote a badly needed reform of sociology's peer-reviewed literature. Sociology editors and academicians either fail to understand or are in obdurate denial that their guild censorship is a damaging disservice to sociology's standing.

But academic sociology still operates under the gleeful delusion that the referee system exercises effective and honest quality control. Only recently have **publishers** belatedly recognized the chronic distortion in the peer-reviewed academic literature. In "Quality Control in Science is Evolving, with a Code of Ethics in Hot Pursuit", the *Economist* (digital edition, 6 February 2015) reports that the information asymmetry due to the anonymity of referees causes distortions, such as referees' "shooting down a rival's work, pinching ideas, or just plain dragging their feet". Ironically sociologists practice what they teach, because they know quite well that the anonymity afforded crowds promotes irresponsibility often seen in riot, vandalism and looting. The *Economist* article also reports that one proposed solution is "open peer review", i.e., carrying out peer review publicly online, which is essentially the proposal made by the editor of JASIS back in 1978.

Another proposal mentioned is an explicit code of ethics. The same *Economist* article reports that Faculty of 1000, an online biology and medicine publisher, has taken this tack with *F1000 Research*, its flagship journal. The *Economist* article reports that Mr. Michael Markie, an associate publisher for *F1000 Research*, has proposed a required "oath", which is a set of ethical guidelines to encourage "even-handed and helpful behavior for reviewers". The "oath" is: (1) I will sign my name to my review. (2) I will review with integrity. (3) I will treat the review as a discourse with you; in particular, I will provide constructive criticism. (4) I will be an ambassador for the practice of open science. The *Economist* article notes that already *Pensoft Publishers* and *Journal of Open Research Software* are following suit. The article also laments that there is no peer-review training, and reports that Marcia McNutt, the editor-in-chief of *Science*, proposes that every journal editor should agree to respect the author's intellectual property and disclose all conflicts of interest. Conflict of interest includes a referee's or an editor's having previously published his alternative views or used an alternative methodology to that expressed in a submitting author's paper. Such a referee is not an "expert" critic; he is a competitor.

Circumventing obstructionism

The newly emergent electronic media are singularly promising today, because they have the same circumventing effect on the sociology guild's academic censorship that they have had on petty tyrants' political censorship. Those media include Internet web sites and more recently inexpensive e-books. Self-publishing authors of e-books have negligible production costs, no inventory or delivery costs, and instant international distribution through online booksellers. And their e-books are never pulped, worm eaten or burned. The *New York Times* has called this phenomenon the "digital disruption" of print publishing. Most importantly the author has complete control over his published content, because the author's research findings are **unfiltered** and **unobstructed** by the guild's "gate guards". Furthermore e-books render sociology's guild incapable of shielding traditionalists by its suppression of new ideas, new techniques, and contemporary philosophy, which are advanced by competing and outperforming outsiders. Disingenuous lip service professing academic freedom is replaced by irrepressibly effective publishing freedom to distribute and access information including contributions that circumvent guild obstructionism.

Revolutionary purge

Hickey's issue with sociologists is wider than simply an issue between a single writer and his critics and their complicit editors, or it might well be allowed simply to drop. But his issues are relevant to philosophy of science: **is academic sociology truly real science or merely pseudoscience?** Presently sociologists' so-called "theory" is just dogmatic imaginative narrative, a complacent self-deception chronically retarding academic sociology's institutional maturation into a real empirical science. Sociologists perform survey research, and to that extent they exhibit empiricism. In fact survey research is effectively the only empiricism that sociologists know, which explains why they rejected Hickey's empirical modeling, which demands a level of sophistication in mathematics and systems analysis exceeding their technical competence. So, if sociology is neither truly real science nor merely pseudo science, call it ***"parascience"***, an embryonic science still in an incipient stage of development with the potential eventually to become real science.

But Hickey emphasizes that any reforming transformation of academic sociology must be more fundamental than the post-classical revolution in sociological theory advocated in 1998 by Donald Black in his "Purification of Sociology" address to the assembled American Sociological Association. Hickey's correspondence with the journal editors and their chosen referees reveals that *any effective maturation of sociology into a real science requires an <u>institutional</u> revolution in its philosophy of science.* Presently sociology's peer-reviewed literature is institutionally dysfunctional. Sociologists should not accept narratives because familiarity makes them seem "intuitive", "convincing" or "to make substantive sense". Nor should sociologists reject theories, because they are "surprising", "bizarre" or "nontraditional". Rather they must adopt the functional concept of scientific theory and recognize the exclusively controlling rôle for the empirical criterion. *In other words sociologists must implement the contemporary pragmatist philosophy of science.*

Contemporary pragmatism will legitimate sociologists' escape from the psychological-reductionist dogmatism that blinds them to the sociologically relevant information in the watershed of social data available from Federal government agencies. It will legitimate their use of the variables in empirical equations made from such data, and it will thereby facilitate sociology's advancement to the status of a real empirical science.

And it will liberate them from the incestuous editorial practices of guild politics. ***Only such institutional maturation can put science in control of sociology instead of putting sociology in control of science.*** BOOK I in this web site could serve as an introductory primer for the retarded sociologists' remedial instruction in philosophy of science.

A change in personnel is needed to produce a change in performance. In disregard of political correctness in sociology Hickey believes that what Donald Black called a "purification" can only be accomplished by a **purge** of sociology's intolerant obstructionist *ancien régime*, the professors and editors with their anachronistic philosophies and their guild politics. However Hickey is not optimistic about the prospects for any transforming pragmatist institutional revolution in sclerotic academic sociology. The *realpolitik* is that there is no likelihood of any such purifying purge by the universities. Too many sociologists have a vested interest in the decrepit *status quo*. Sociology is static because established sociologists are paid to teach what they have been taught, and so continue to practice the only sociology they know.

Hickey is reminded of the dismal observation made by the historic 1918 Nobel-laureate physicist Max Planck, the initiator of the revolutionary twentieth-century quantum physics, who wrote in his *Scientific Autobiography* that a new scientific truth does not triumph by convincing its opponents, but rather succeeds because its opponents eventually die off. The reactionary obstructionists such as the referees together with the complicit editors who select them and accept their attempted criticisms, will inevitably be pushing up daisies. Inexorable attrition must eventually do the purging, if change is to occur. Hickey predicts that any future maturation of sociology as real science must progress, as Planck also said, "funeral by funeral".

This entire web site, BOOKs I through VIII, is also an e-book, third edition.