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AGAINST MECHANISM: METHODOLOGY FOR AN EVOLUTIONARY ECONOMICS

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When the first economics departments were proposed at Cambridge and Oxford, the proponents thought acceptance would be improved if economics could be seen as incorporating the methods of physics. The enterprise was premised on the existence of economic laws that describe invariant relationships between events. These event regularities, like gravity, were not affected by human action. Humans could adapt and use them, but not change them. Thus the metaphor of "mechanism" seemed appropriate and became embedded in economists' language. It is common to use the term market mechanism to link prices and commodities. This suggests the economy is like turning a crank attached to a set of gears where there is a fixed relationship between the crank's motion and the last gear's motion. The gears have no ideas of their own, they don't get mad; there is no cognitive element between events and action.

But what if the economic world is not entirely like that? Tony Lawson (1997) argues that event regularities in economics are rare. There are only "demi-regs" limited to time and space. People learn and change the links between events. Preferences are not fixed. Event regularity presumes a closed system, while human affairs are open systems where the conjunctions among events are partly human artifacts. These artifacts are formed by human intent based on observation of real events.

Ontology is the theory of what is real, of what exists. Lawson is explicitly concerned with ontology because any set of assumptions about what is real fits some methodologies, but not others. Traditionally focused on questions such as the existence of God and the angels, ontology became central to philosophy of science in the early 20th century (see Appendix). Positivists asserted that only data are real; underlying causes and ordinary objects (things like cows, pigs and bushels of wheat) are not. On the positivist view, science identifies patterns of regularity in data. But how can such a view of science1 support predictions about the supply or demand of cows, pigs and bushels of wheat? To make predictions one must (and economists who were influenced by positivism did) interpret patterns in data as invariant laws, as relationships that apply universally to all data. In many instances, this strategy led economists to make serviceable predictions, but positivism ruled out any interest in the underlying causes, structures, powers, processes and tendencies that are responsible for observed regularities. Lawson's philosophy of science (which he calls *critical realism*) presumes that *some* sort of deeper cause *is* real, and that forces such as gravity or social structure have real effects (see also Bhaskar).[2]

Lawson makes three related points in favor of critical realism and against positivism. The first is that positivism has led economists to neglect topics that are of clear relevance to the behavior of economic agents and economic systems. In simply presuming that observed data can be described by universal quantitative relationships, positive economists make an ontological assumption that blinds them to important sources of dynamism in an economy—sources such as human intention, knowledge and emotion. Second, he argues that the underlying realities shaping economic behavior are highly variable, and may be relatively short-lived. Positive economists seeking universal laws have been enamored with quantitative models. Lawson (p. 70) argues that "Econometricians continually puzzle over why it is that 'estimated relationships' repeatedly 'break down', usually as soon as new observations become available." Finally, he argues that error is best attributed to inadequate knowledge of underlying causes or structures. Since knowledge itself is an underlying cause that affects social structure, it is impossible to eliminate error entirely. As such, knowledge is always contingent and subject to revision. Positivists, on the other hand, adopted an epistemology based on certainty. When one truly knows the invariant quantitative relations implicit in every set of data, any given set of observations provides the basis for logically certain predictions about future observations.

Hausman (p. 208-9) writes that everyone is a realist in one form or another. The disagreement is over what reality is and whether to pursue deep causes. Should applied economists pursue these causes? Is the proof in the pudding? How has economics been doing? If you are completely happy with the progress of economic knowledge, you can stop reading now. If not, then Lawson may have something to offer.

Implications for Economic Practice

All of us remember Friedman's dictum that the realism of assumptions is irrelevant as long as the model predicts. A realist such as Lawson objects. Friedman must assume the existence of spontaneously occurring closed systems. All models are partial and abstract by definition. But, Lawson argues that an appropriate abstraction must be concerned with real, not idealized processes; and must be concerned with the essential rather than merely the most general attributes of things.

In experimental designs with randomization it is not necessary to understand what variables might be controlled by the randomization. Randomization means that whatever else might affect Y is held constant. So if we vary X and find it associated with change in Y, we are confident in saying that we have discovered some event regularity of the form "If X, then Y." The other variables, whatever they are, have been isolated. It is not necessary for engineering to make any deep inquiry into other causes to be confident that X causes Y and therefore recommend that if you want Y to change in a particular direction, then change X. However, in the case of a non-experimental science like economics, inquiry into deep causes is necessary and in some sense never ending. If X is found to be the cause of Y, we can always ask what caused X and so on.

Positivist irrealism and the neglect of driving forces has led economists to overlook some striking anomalies in the past. Consider the case of the effect of a price increase on quantity demanded. If the conjunction of a given price and quantity demanded is invariant, then if the price returns to its previous level, the quantity demanded should also return to its previous level. Empirically, predictions of quantity demanded deduced from the coefficients discovered on the upward price movement are often wrong. Consumers often learn and grow to like the substitute for the more costly good and do not return to their old level of demand. Rationality with fixed preferences is not the only possible behavior that fits budget constraints and market clearing (Arrow). Path dependence also occurs on the supply side. Investment in an immobile asset may become fixed in use even if output prices decline such that the total cost is not recoverable (Johnson). A methodology aimed at generalized, timeless law-like relationships is inappropriate to a world that is changing. An adequate methodology must accommodate both learning and path dependence. The process of investment and disinvestment must be investigated and the result cannot be deduced from the output price and cost relationships observed during expansion. The learning process must be investigated to get behind the surface phenomena of consumer behavior.

The investigation of "deep causes" that go behind observable surface phenomena is a major agenda item for Lawson. The search for deep causes would not be necessary in closed systems where reality stays put–no learning and human choices which change the connections between things. Lawson notes that even in the natural sciences there are few connections of the type of "if X, then Y in general." The relationship holds only for the experimental conditions arranged by the scientist. Experiments act to close the system. This is sufficient to tease out many laws and to guide practical engineering. Experiments form the essence of natural science methodology. Realism, constant conjunctions among things, and experiments go together. Lawson argues that there are reasons to believe that constant conjunctions are few in social affairs, and economics is limited in experimentation.

Social systems are not closed. Unlike a system of fixed gears, humans are capable of changing the conjunctions among themselves and between themselves and objects (valuations). Institutions describe the relationship among people circumscribed by formal rights and cultural habits. The existing rules condition the perception and distribution of opportunities. And, at the same time, our perception of the connection leads us to try to change these institutions.

How have economists tried to fit a methodology appropriate for closed systems to what is mostly an open system? Central to field theory in physics is the conservation principle. This principle allows for the identity of the system to be maintained even if transformed (Mirowski, p. 272). How is the system closed in consumption theory? One way is to postulate path independence. Another way is to rule out the phenomenon of regret. But experience and empirical evidence suggest otherwise.

Motivation is a deep cause of human action, but essentially unobservable. Economists have always been deeply suspicious of what people say. We suspect they rationalize and state their ideals rather than what really controls actions. But what are our choices as scientists? We can either assume a motivation (narrow self interest) or try to make what we can out of what people say about themselves. Herbert Simon urges us to get out of our armchairs and talk to business people. He critiques Becker's explanation of the increase in female participation in the work force. Becker's theory can only consider that it must have been caused by a change in relative prices. Simon imagines that it could have been caused by a change in ideology and preference. Shall we accept the dictum of theory or shall we ask people? Where will the greatest mistakes occur?

Just as motivation and regret gives meaning to surface events, so do institutions. Boylan and O'Gorman (p.99) make the point as follows:

> *The action of agents "includes an indispensable reference to some conceptual scheme or other. For instance, the same behaviour, such as handing over a signed cheque, can constitute different actions: in one instant the agent is paying off a debt and in another he or she is*

defaulting on a payment. Without the concepts of bank accounts, legal contracts, purchasing in advance, etc. these actions cannot be properly identified."

"Lawsonian economic realism embraces this essential interpretative dimension of the actions of economic agents. Thus Lawsonian realist economists clearly acknowledge human agency as a causal power."

"If, for instance, the action of paying a debt by cheque could not exist without the concept of a bank account, neither can it exist without a banking system. Social systems or structures as well as individual agents have causal powers. Individual agency presupposes social structures and vice versa; neither can be reduced to the other."

Deep causes whether motivational or institutional can't be seen in a production function. Some have argued that it is not worth studying institutions since economic growth can be explained by a physical production function. But this surely begs many questions of what must be the case in human relationships for the physical factors to be present at the right time and place. We have learned to carefully specify production inputs. Hybrid corn is not the same as open-pollinated. We have learned to carefully specify the labor variable in a production function. An hour of labor of a person with technical training is different from an hour from an illiterate. We are learning that we can't take the person hour applied to work as a given. It depends on motivation and monitoring. The MVP of labor may even depend on the price it is paid (Stiglitz). We need not add technology or institutions to the physical production function, but any useful analysis of development will want to know what factors condition the presence of technology and the presence of motivated labor.

Prices and quantities are surface phenomena which neoclassical economics has been content to work with. But it is not just money or exchange that make goods commensurate. It is the institutional choice of rules of property rights and their distribution. Change the rules and your change the money measure. What a thing costs is a function of whose opportunities have to be taken into account by a decision maker. (Samuels and Schmid)

Even more fundamentally, price can affect demand and not just quantity demanded. In social capital research we are interested in whether reducing the cost of volunteering, via tax deduction for example, might increase the demand for volunteering (the supplying of labor). Our standard theory holds the preferences for goods constant, but in fact reducing the cost of volunteering may change the value of the activity for the volunteer. The existence of paid blood providers can reduce the value of unpaid donors' blood by providing an alternative source for those in need (Titmuss). Without a search for deep causes we could never understand this relationship.

Realism in Social Capital and Evolutionary Economics

Recent work on social capital and evolutionary economics provides an example of how being more conscious of methodological issues might affect the path of research..

Both involve a more realistic reinterpretation of game theory. In standard prisoner's dilemma games and models of contribution toward the production of high exclusion cost goods it is common, following Olson, to assume an isolated individual following a decision rule of optimizing expected utility. So no matter what the other person does, it is rational to defect and not contribute (be a free rider). In fact, empirical studies show that free riding is common in large number situations. But they also show considerable exceptions. Models shape research agendas. If we find a significant correlation between high exclusion cost situations and free ridership, inquiry may stop there since it seems consistent with theory. But if you start with a more realistic observation that relationships matter, it is more likely that we will inquire into the conditions where people appear to use self-regarding optimization decision rules, and how they differ from conditions where people appear to do something else.

Grant and Thompson integrated producer decision making into an ecological model of a common range (Hardin's tragedy of the commons), where the Nash equilibrium for each economic agent causes overgrazing, leading to ecosystem collapse. Two decision strategies were compared. One presumes that decision-makers having perfect knowledge optimize expected utility. The alternative decision rule mimics Axelrod's tit-for-tat (1984): start with a bias for cooperation, then do what your neighbor did last time. Not surprisingly, the rule-following "do what your neighbor did last time" allows the ecosystem and its economic decision makers to continue in a common use pattern indefinitely, while optimization quickly drives the ecosystem into irrecoverable disturbances and resource loss. Placing the prisoner's dilemma into a more realistic ecological context suggests at least two important lines for future research in economics.

First, as Axelrod noted, we can see the emergence of institutions as an evolutionary phenomenon. However a nascent institution like "do what your neighbor did last time" is initiated, groups that possess this institution will survive in some ecological environments whereas groups that tend toward individual optimization will disperse or die. This suggests that some norms are products of economic evolution. Their 'rationality' consists not in their consistency with individual optimization behavior, but in their capacity to produce collective behavior that is both ecologically and economically sustainable over time. Second, the example makes it clear that however they arise, norms like "do what your neighbor did last time" must be reproduced from year to year, from generation to generation, in order to be effective. Why do some groups adopt non-optimizing norms of cooperation, while others do not?

The most plausible answers to this question point toward social capital. Knack and Keefer observe that countries with higher levels of trust have higher levels of economic development. The regression coefficient indicates that a ten-percentage-point rise in the trust variable is associated with an increase in growth of four-fifths of a percentage point. But what underlies trust? Trust is an act of exposing ones assets to the opportunistic behavior of others. Knack and Keefer have no measure of the frequency and quality of such behavior. They only have a reported agreement with the statement, "Generally speaking, would you say that most people can be trusted." In any case, why do people trust each other? It might be because of the build up of experience other time. It may be behavior following a rule such as enter into cooperative agreements with kin. Or it might be because there is a genuine affinity of one person for another and you do not take advantage of people you like. The correlation between trust and development only scratches the surface. It does not get at what experiences might be created to increase the amount of trust. It does not illuminate how one source of trust complements and substitutes for another and thus the basic ingredients of economizing to produce a result. One may not need to understand where trust comes from to predict economic growth, but I surely do if I want to encourage growth by building trust.

Much of the research that must be done on social capital presents a methodological challenge. The design of a questionnaire to discover motivation for a goods movement illustrates the inescapable role of priors. The movement may be motivated by a combination of selfish advantage, sympathy or rule following. Economists are used to thinking in terms of a budget constraint, fixed preferences, and calculation. So why not ask people to distribute 100 points amongst the several motives noted above? This will elicit the importance of the various motives. But the frame of the question assumes that people are calculating utility maximization. What if the mind does not always work like that? What if it just jumps from an image of a situation to a fitting action? Psychologists like to use Likert scales and frame their questions in terms of the extent of agreement with a statement. This does not impose a "budget constraint" but it nevertheless also presumes calculation and conscious consideration of whatever list of alternatives is presented. In the face of difficulties in investigating deep causes, some economists are content to just assume them. But why should putting words in people's mouths be preferable to trying to interpret what they say, even if shaped by what is asked?

The Role of Economics in Policy

Prediction has two different meanings. One refers to predicting some future state such as the size of GNP in 2000. Another is an "if, then" proposition. It predicts the size (maybe direction) of GNP or some other more limited state if policy X is implemented. Or it predicts that the rate of change in GNP will be associated with X policy. These are likely to be conditional on the other variables being present or absent.

Analysts who make policy suggestions are predicting in the second sense. They are not predicting that policy X will in fact be implemented, but if it were they would think they were successful in their analysis if the predicted state generally occurred for a particular time and place. They might think that they were not altogether unsuccessful even if the state did not occur, if the policy could be shown to have moved things in the desired direction and the actual state would have been further from the mark if the policy had not been adopted. What policy gets adopted is a matter of the outcome of power struggles among contending interests. And, its impact depends on the interactive choices of many people with different perceptions and interests.

Positivism and the models that it has generated have served this model of policy analysis well, for models define quantitative "if-then" relationships that allow precise predictions to be specified. But positivist policy analysts have not been curious about whether and how these predictions themselves might alter the policy process, how they might bias policy making toward norms of utility maximization, rather than altruistic or cooperative norms like "do what your neighbor did last time."

Turning toward deep causes is emancipatory (Lawson's word) in that it expands the policy agenda. Attention to deep cause shifts our focus from merely changing events within structures to changing event possibilities by transforming institutions. It is the difference between just changing prices to changing preferences; the difference between reducing transaction costs and changing who is buyer and who is seller. It also provokes inquiry into the reflexivity of economic policy analysis itself. In the world of positive economics, analysts observe and model a closed system from the outside. In the real world, policy analysts mold preferences, provoke learning, and reinforce certain interests while weakening others. Surely this process of changing the very phenomena economic policy analysis has been developed to describe deserves comment and criticism.

But can economics be more than social criticism? We can live with modesty in prescribing institutions that will serve a particular interest. We are quite aware of Boulding's law of unintended consequences. Things will happen that we did not predict. We are comfortable with Lawson's observation that emerging reality can be out of phase with our experience of it and that can be out of phase with our intended choices of institutions. We are comfortable with a great deal of randomness. Richard Rorty gets our attention when he argues that there is no place to stand outside of culture in order to evaluate it, that all inquiry is situated and a matter of perspective, and that science makes truth rather than simply discovers it. Still, Lawson speaks of demi-regs–connections that hold over some time and space. These are enough to make us get up in the morning and inquire into deep causes and then into causes behind that the next morning. As Lawson (p. 288) writes, the aim of economics can be the "formulation of effective responses, such as alternative institutional structures, including relationships." Given this pragmatic orientation to the discipline, a shining beacon is not needed to guide us in the contest with others over whose interests are to be reflected in prevailing institutions and to judge the weight of the evidence that a given policy would serve a particular interest (Solo). One can be satisfied with some even cloudy sense of direction obtained by constantly emerging understanding of transient patterns. We agree with Lawson (p. 289) that there is reason to "preserve the intuition that human social history is both explicable and yet actively made." It is our moral judgment that the world would be better off believing so even if we can't ever know if it is true.

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Endnotes

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2. Lawson argues that the dominant economic methodology adopts a deductivist approach which ultimately gets its support from positivist philosophy in which reality consists of atomistic events given in experience. In such a world the only possible form of scientific generalities are correlations in actual phenomena. For predictable action to follow, correlations are assumed to be invariant. Lawson argues, however, that underlying (often directly unobservable) structures such as gravity or social structures are equally real, and that it is at this deeper level that any invariance or short term durability lies. Social reality is conceptualized as emerging from deep causes, human volition, and is dynamic and holistic. All knowledge is fallible, partial, contested and transient. There is no ultimate timeless, spaceless essence. And, even if there were, reality is subject to contradictory perceptions (Samuels, 1992, p.114).

Appendix

Philosophy of Science and Positivism: A Short History

by Paul B. Thompson

Philosophy of science emerged from general epistemology between 1880 and 1920. It inherited a robust distinction between deductive and empirical knowledge. Deductive knowledge consisted in logical and mathematical inferences and in the analysis of concepts or definitions. Empirical knowledge involved the observation of phenomena, the formulation of factual claims, and the validation of generalizations. Throughout the early history of epistemology rationalist philosophers such as Descartes argued that only deduction could produce authentic knowledge. Empiricist philosophers such as Locke argued that even definitions, concepts and inferential patterns were learned through experience. Empiricists also argued that since deductive implications are by their nature already contained in the premises from which they were deduced, deduction did not represent an advance in knowledge.

By the late 19th century, this debate had cooled considerably. Philosophers were deeply engaged in two projects. One was an exploration of the logical underpinnings of mathematics. The other was an attempt to resolve the problem of induction, or the establishment of lawlike generalizations from isolated generalizations. The idea that a prime source of error came from an uncritical reliance on beliefs and claims that were actually meaninglessness error influenced both projects. Propositions or theories could be meaningless on logical grounds if they implied contradictory or paradoxical claims. They were thought meaningless on empirical grounds if they implied the existence of an entity that did not exist.

The logical positivists were a group of philosophers who argued that every claim admissible as "true" or scientific" must be capable of verification through a process of logical test and experiential observations. Claims that could not pass this test were claimed to lack meaning. This philosophy placed science squarely in the tradition of epistemology. Scientists must be content to make logically consistent claims about their data. Since data are derived from human sensory experience, scientists are never in a position to make claims about the world as it might exist beyond human experience. Thus the logical positivists arrived at the most influential tenet of their position: the prohibition of "metaphysical" language (i.e. any language referring to things beyond human experience).

Almost before it got started, logical positivism was abandoned. Philosophers who had been intrigued with verificationism realized that the principle of verification was itself unverifiable. It was itself a metaphysical claim and no theory that included it could consistently prohibit metaphysical language. Philosophers of science attempted many solutions to this problem from 1930 to 1970, not without some progress. Yet logical positivism's emphasis on procedures of verification and prohibition of metaphysical language remain enormously influential among working scientists, while few of the more sophisticated theoretical approaches appear to have many followers outside the philosophy of science.

The main exception to this generalization is Sir Karl Popper's falsificationism. Popper was arguing for this idea at the height of logical positivism, but it did not become widely influential until Carl Hempel synthesized what was most appealing about the positivist program with Popper's ideas on falification. Hempel argued that every scientific explanation has a deductive structure. Factual observation statements and theoretical generalizations form the *explanans* (e.g.

premises) of an explanation. Together, these must deductively entail the *explanadum—a* description of the phenomenon to be explained.

Applied to a standard economic problem, we would say that the *explanans* contains both the quantitative model and any data that had been collected to specify parameters or constants within the model. Together these elements would entail certain factual allegations, such as "Inflation increases," or "Hog prices collapse." What makes this approach scientific, according to Popper and Hempel, is that if the factual allegation turns out to be false, the researcher has a basis for revising the model. The logical rigor of the structure guarantees that the explanadum is true when the *explanans* are true. So if the explanadum is false, at least one of the premises is false. If one has been diligent in collecting data, the false premise must be somewhere in the covering theoretical structure; in this case the econometric model.

Hempel's achievement is referred to as the "covering law" model of scientific explanation or "deductive-nomathetic" generalization. On one hand, Hempel was simply working out ideas that were implicit in positivist philosophy. On the other hand, his approach was an enormous shift in the way that scientific inference was construed. Formerly, philosophers and scientists had understood the "hypothesis" —by all accounts the key element of empirical science—as the conclusion of inquiry, resting on factual observations. The "problem of induction" is that the logical link between factual observations and the kind of lawlike generalizations was obscure, and subject to error. Hempel reoriented this approach so that the hypothesis appears in the theoretical generalizations of the premises (or *explanans*), and a particular factual circumstance appears in the *explanadum*.

From this point, it is a relatively straightforward move to see science as a form of inquiry that is involved in hypothesis testing. If lawlike generalizations plus observed data logically entail a claim that is known to be false, there must be a false statement somewhere in the *explanans*. Since the data have been observed to be true, the most insecure part of the lawlike generalizations—the "hypothesis"—is the most likely candidate for being false. The Hempel model also introduced logical rigor into the idea of prediction. To say that the *explanans* predict the *explanadum* is simply to say that the explanadum is a logical consequence of the *explanans*. Experimental scientists used the covering law method by creating an experimental apparatus that, if the theory were true, would produce a certain result. It was back to the drawing board when the expected result did not occur. Historical data could also be used to see whether the result that did, in fact, occur at some point in the past was the one that was logically entailed by a theory or model. This also provides a basis for falsification and revision. Thus, prediction does not involve future states of affairs in any fundamental way. Science, Popper claimed, proceeds by constructing experiments that will subject hypotheses to rigorous tests of falsification. The hypothesis combined with factual statements about empirical conditions "predicts" a certain state of affairs. If that state of affairs fails to obtain, the hypothesis is falsified. Scientists go back to the drawing board.

Though nothing in philosophy is uncontested, falsificationism continues to be influential. Most mainstream philosophers of science would defend the basic principles of covering law models and falsification. However, there are important respects in which this approach does not satisfy all the desiderata for a philosophy of science. First, though we can show that a given hypothesis is false, and can make enormous progress through eliminating faulty competitors, we cannot show definitively that any hypothesis is true. Second, the theory says nothing at all about the process scientists use to propose hypotheses. It is thus a poor guide to distinguishing between work done on the leading edge and routine industrial practices. A related third problem is that scientists regard some elements of theory as very well established. Established theory is never subjected to falsification. The "hypothesis" being tested is thus a small part of the lawlike generalizations in the explanans. We know that there are some lawlike generalizations are false when the explanans are falsified. Yet we have no test to ensure that the false claims are in the hypothesis, rather than generalizations that make up the body of established theory. So the practice of science requires judgment on the part of scientists and a collective sense of which elements in scientific theory are "established" and which are not.

Popper was clearly aware of these results, and did not regard them as defects. He was effectively arguing that certainty was an illicit goal for science. The period after Popper's contribution took up these problems, while generally accepting the covering law model and falsification as an adequate account of prediction, explanation and experimental practice. From roughly 1960 to 1990, scholars tried to develop an account of how scientists distinguish established science from hypothesis. This was widely recognized to be a *normative* (though not politically driven) distinction, hence by the 1960's philosophers of science had given up the idea that science could be "value free". W.V.O. Quine developed an approach that stressed parsimony and comprehensiveness. Thomas Kuhn and Michael Polanyi stressed cultural factors operating within the world of science itself. Imre Lakatos has provided what many philosophers would regard as the best defense of the integrity of scientific explanation and of the progressive development of knowledge. Post-Popperian philosophy of science was eventually pushed to an extreme position by cultural relativists and postmodernists who argued that the social authority of science resided in its capacity to reinforce a given power distribution, rather than any epistemological guarantee of certainty.

All these views from Descartes to Kuhn philosophy of science presuppose an epistemological approach. Scientific theorizing and research are presumed to differ from ordinary, non-scientific forms of learning and knowledge acquisition in virtue of the way that they employ methods of data collection, logical and mathematical inference, and the rejection of hypotheses through falsification. Scientific theories are, on this view, constructions of the mind. None of the leading figures in 20th century philosophy of science argued that scientific theories are descriptions of reality. For a realist the key point is whether the mechanisms and forces that are posited in theory do, in fact, control the course of events in the real world. And if they do exert control, they must exist. A realist, in other words, moves immediately to questions about whether putative mechanisms, be they divine, evolutionary, or equilibria, actually exist. For a positivist, what counts is whether postulating such mechanisms is consistent with certain logical tests.

On the realist view, science differs from ordinary, non-scientific knowledge in being the account that seems most likely to be true. This is decided through a process of offering competing accounts and then subjecting them to a process of "fair causal comparison" that notes the relative strengths and weaknesses of each, regarding unrealistic constructs as particularly problematic. It is not clear how recent realism escapes the charge dogmatism, the circumstance that led to positivist philosophy in the first place. The factors that make a given theoretical account plausible or preferable would presumably have as much to do with the human mind as the external world. And as postmodernists have said, appealing to 'reality' can be just another way of asserting power.

The pragmatist philosophy of Charles Sanders Peirce, formulated a century ago, has reemerged as a third way. For Peirce, "truth" is a hypothetical end point that would be reached if the process of comparative analysis could be pursued without cost or limit. Of course, actually existing science has costs and must reach closure. But scientists who regard their activity as guided by the norm of convergence on reality will follow a set of rules for conducting and reporting their research, as well as for critiquing the work of others. These rules will differ dramatically from the kind of strategic debates that would be generated by scientists trying only to maintain positions of wealth or power. Pragmatists would distinguish science from non-science by appeal to its procedures of discourse and debate. Scientific discourse would not involve claims or arguments strategically calculated to advance financial, ideological or personal interests. It would take all competing explanations seriously. It would regard the competition between theories as resolvable in the fullness of time, and would rely explicitly on value judgements to justify temporary closure of debate in situations calling for immediate action.

Before concluding this whirlwind history, it is worth noting that since 1970 mainstream philosophy of science has turned away from the problems in physics and astronomy that were the focus of its early practitioners. Biologists had long expressed dissatisfaction with approaches to scientific explanation that analyzed cause and effect in terms of the "if-then" connection of formal logic. Evolutionary biology in particular did not take the simple causality of events to be particularly interesting. As a discipline, biology was interested in the emergence of a pattern out of a host of separate events that were, in themselves, causally unproblematic. However, biologists wanted a non-teleological account of species and genetic evolution, one that did not depend upon a plan or design imputed to nature. As principles of evolution and biological equilibria have become clearer, philosophers have increasingly recognized that the basic pattern of explanation in biology differs fundamentally from that of physics.

Where does economics fit in all this? Generally speaking, 20th century philosophy of science has been a debate about the philosophy of physics. Physics was thought to provide the best example of scientific theory and experimental practice. Until recently, economics was thought to be a deductive science: a discovery of the logical structure implicit within a limited set of formally specified assumptions. John Stuart Mill had proposed this view of economics in 1836. Mill's starting assumption—a person guided only by self-interested maximization—was modified over time to include assumptions about the logical structure of preferences, zero information costs and the like. Yet even today many economists would insist that their discipline has little empirical content.

Mill believed that economics is applied in the same way that mathematics is applied. If you are ordering carpet, it is handy to know some geometry. If you are running a business, it is handy to know some economics. Neither geometry nor economics is *explaining* anything in either case. In Mill's view, the application of economic theory is an art requiring judgment to assess whether and in what respects the formal assumptions of the theory are relevant to a practical case at hand. This view shifted over time, especially in Lionel Robbins' influential essay *On the Nature and Significance of Economic Science* (1935). Robbins moved economics away from realism by arguing that human psychology was irrelevant to economic science. What mattered was whether observable economic behavior is consistent with the behavior entailed by assumptions specifying the conduct of a rational economic agent. Although Robbins wrote before Popper and Hempel became established names in philosophy of science, the model of economics that he described was entirely consistent with the covering law and falsification approaches to scientific progress.

Post-Robbins, the view was that the rational actor model explains most human behavior, so much so that it is possible to deduce unobservable preferences from economic choices (something that Robbins himself would never have endorsed). This shift in the philosophy of economics precipitated two contradictory developments. On the one hand, economics took on a decidedly utilitarian look when the result of free and informed exchanges among individuals was taken to represent the social optimum. On the other hand, the theory was said to be a purely objective, value-free tool for predicting economic behavior. Milton Friedman linked the latter set of claims with the word "positivism" in a famous article form 1953.

It seems unlikely that economists can have their philosophy of science cake and eat it, too. If the theory is value-free, economists should just shut-up about efficiency and losses. But Robbins and Friedman style positivism has encountered deeper problems, as well. If "positive economics" is interpreted in light of the covering law model of explanation, the basic assumptions of economic theory would have to be combined with factual observations in order to generate logically rigorous predictions. When predicted results turn out wrong, the theory should be rejected and revised. But human behavior cannot be subjected to the controls needed to fix background conditions and to eliminate extraneous influences. The theory could not, in practice, be falsified because circumstances that were, from the point of the theory, imperfections—information costs, inconsistency, ethical constraints—could not be eliminated from the *explanans*.

This resulted in a dilemma that continues to haunt economists. On the one hand, if one stresses the behavioral orientation of the theory it can be understood as a program of falsification. But the unrealistic elements of the assumptions limit both its testability and its applicability. On the other hand, if one stresses the deductive validity of the formal theory, one can apply the theory simply by arguing that human affairs should be restructured to more closely resemble the conditions of individual optimization. This yields robust recommendations for practice, but shields the substantive assumptions of the theory from criticism.

Philosophy of science thus ends with what many might regard as disappointing results. many practicing applied economists may with some justification find this convoluted history, even abridged and summarized as it is, to be largely irrelevant to their research and teaching. Although the situation might have been different a century ago, many scientists do not need to have any idea what they are is actually doing in order to be successful. Science is now a socially established institution, with socially reinforced rules and procedures. Perhaps these rules and procedures no longer need a public rationale that would distinguish scientific claims from those of religion or simple prejudice. While we believe that is a questionable assumption, it is not a topic that can be pursued here.

Today, the methods and procedures in which scientists are trained need not inspire any more critical reflection than the catechisms of an earlier epoch. Understandably, most working scientists do not want to be troubled by those who would question the underpinnings of a discipline in which huge numbers of people and organizations participate in the use of established methods for research and training of the next generation. One doesn't get tenure for defending science, only for doing it. As this set of papers attests, economists may be more willing to participate in debate over foundations than many physical and biological scientists, but for the most part one would expect the bench-economist to have about as much interest in philosophy and methods as the average bench-scientist. That is, very little at all.

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Robbins, L. 1935. *An Essay on the Nature and Significance of Economic Science*, 2nd Ed., London, Macmillan.