Journal of Economic Literature
Vol. XXI (June 1983), pp. 481–517

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The Rhetoric of Economics

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The length of the acknowledgments here testifies to an unexplored feature of the rhetoric of economics, the role of the audience: like oratory, scholarship depends for its virtues on the virtues of its audience. I have been fortunate in mine. I must apologize for my amateurish understanding of what is happening in philosophy, mathematics, literary criticism, rhetorical studies, and other places beyond my competence, and ask that practitioners in these fields assist in my further education. For their early attempts I thank Evan Fales, Paul Hernadi, John Lyne, Michael McGee, Allan Megill, John Nelson, and Jay Semel of the Colloquium on Applied Rhetoric at The University of Iowa; Wayne Booth, Ira Katznelson, and others at the University of Chicago in the program in Politics, Rhetoric and Law, before which the earliest version was delivered; Robert Boynton, Bernard Cohn, John Comaroff, Otis Dudley Duncan, James O. Freedman, Clifford Geertz, William Kruskal, Donald Levine, Laura McCloskey, Richard Rorty, Renato Rosaldo; and the Humanities Society at the University of Iowa. That the economists on whom I have inflicted the argument have reacted with such intelligent skepticism and generous encouragement suggests, as the paper does, that we are better scholars than our methodology would allow. I thank my colleagues in economics at Iowa, especially the Sanctuary Seminar in Economic Argument; Seminars at the World Bank and the National Science Foundation; my colleagues at the Institute of Advanced Studies and the Faculty of Economics at the Australian National University; seminars at the universities of Adelaide, Auckland, Melbourne, New South Wales, Tasmania and Western Australia; at Monash, and Iowa State universities; Victoria University of Wellington; and an assemblage of economists elsewhere: William Breit, Ronald Coase, Arthur Diamond, Stanley Engerman, J. M. Finger, Milton Friedman, Allan Gibbard, Robert Goodin, Gary Hawke, Robert Higgs, Albert Hirschman, Eric Jones, Arjo Klamer, Harvey Leibenstein, David Levy, Peter Lindert, Neil de Marchi, Michael McPherson, Amartya Sen, Robert Solow, Larry Westphal, Gordon Winston, and Gavin Wright. Thomas Mayer's encouragement at an early stage and his detailed comments as referee for this Journal *at a later stage were exceptionally heartening and useful.*

Economists do not follow the laws of enquiry their methodologies lay down. A good thing, too. If they did they would stand silent on human capital, the law of demand, random walks down Wall Street, the elasticity of demand for gasoline, and most other matters about which they commonly speak. In view of the volubility of economists the many official methodologies are apparently not the grounds for their scientific conviction.

Economists in fact argue on wider grounds, and should. Their genuine, workaday rhetoric, the way they argue inside their heads or their seminar rooms, diverges from the official rhetoric. Economists should become more self-conscious about their rhetoric, because they will then better know why they agree or disagree, and will find it less easy to dismiss contrary arguments on merely methodological grounds. Philosophy as a set of narrowing rules of evidence should be set aside in scientific argument, as even many philosophers have been saying now for fifty years.

Economics will not change much in substance, of course, when economists recognize that the economic emperor has positively no clothes. He is the same fellow whether philosophically naked or clothed, in reasonably good health aside from his sartorial delusion. But the temper of argument among economists would improve if they recognized on what grounds they were arguing. They claim to be arguing on grounds of certain limited matters of statistical inference, on grounds of positive economics, operationalism, behaviorism, and other positivistic enthusiasms of the 1930s and 1940s. They believe that these are the only grounds for science. But in their actual scientific work they argue about the aptness of economic metaphors, the relevance of historical precedents, the persuasiveness of introspections, the power of authority, the charm of symmetry, the claims of morality. Crude positivism labels such issues "meaningless" or "nonscientific" or "just matters of opinion." Yet even positivists actually behave as though the matters are discussable. In fact, most discussion in most sciences, and especially in economics, arises from them. Nothing is gained from clinging to the Scientific Method, or to any methodology except honesty, clarity, and tolerance. Nothing is gained because the methodology does not describe the sciences it was once thought to describe, such as physics or mathematics; and because physics and mathematics are not good models for economics anyway; and because the methodology is now seen by many philosophers themselves to be uncompelling; and because economic science would stop progressing if the methodology were in fact used; and, most important, because economics, like any field, should get its standards of argument from itself, not from the legislation of philosopher kings. The real arguments would then be joined.

I. *Rhetoric Is Disciplined Conversation*

These points, elaborated below, amount to an appeal to examine the rhetoric of economics. By "rhetoric" is not meant a verbal shell game, as in "empty rhetoric" or "mere rhetoric" (although form is not trivial, either: disdain for the form of words is evidence of a mind closed to the varieties of argument). In *Modern Dogma and the Rhetoric of Assent* Wayne Booth gives many useful definitions. Rhetoric is "the art of probing what men believe they ought to believe, rather than proving what is true according to abstract methods"; it is "the art of discovering good reasons, finding what really warrants assent, because any reasonable person ought to be persuaded"; it is "careful weighing of more-or-less good reasons to arrive at more-or-less probable or plausible conclusions—none too secure but better than would be arrived at by chance or unthinking impulse"; it is the "art of discovering warrantable beliefs and improving those beliefs in shared discourse"; its purpose must not be "to talk someone else into a preconceived view; rather, it must be to engage in mutual inquiry" (Booth, 1974, pp. xiii, xiv, 59, 137). It is what economists, like other dealers in ideas, do anyway: as Booth says elsewhere, "We believe in mutual persuasion as a way of life; we live from conference to conference" (Booth, 1967, p. 13). Rhetoric is exploring thought by conversation.

The word "rhetoric" is doubtless an obstacle to understanding the point, so debased has it become in common parlance. If "pragmatism" and "anarchism" had not already suffered as much, unable to keep clear of irrelevant associations with the bottom line or the bomb, the title might better have been "Pragmatism's Conception of Truth in Economics" or "Outline of an Anarchistic Theory of Knowledge in Economics" (William James, 1907; Paul Feyerabend, 1975). But the enemies of sophisticated pragmatism and gentle anarchism, as of honest rhetoric, have used the weapons at hand. The results discourage onlookers from satisfying the curiosity they might have had about alternatives to coercion in philosophy, politics, or method. A title such as "How Economists Explain" (Mark Blaug, 1980; but see below) or "Why Methodology Is a Bad" would perhaps have been meeker and more persuasive.[1] Still, "rhetoric" like the others is a fine and ancient word, whose proper use ought to be more widely known among economists and calculators.

The rhetoric here is that of Aristotle, Cicero, and Quintilian among the ancients, reincarnated in the Renaissance, crucified by the Cartesian dogma that only the indubitable is true; which in the third century after Descartes rose from the dead. The faith built on these miracles is known in literary studies as the New Rhetoric, new in the 1930s and 1940s from the hands of I. A. Richards in Britain and Kenneth Burke in America (Richards, 1936; Burke, 1950). In philosophy John Dewey and Ludwig Wittgenstein had already begun to criticize Descartes' program of erecting belief on a foundation of skepticism. More recently Karl Popper, Thomas Kuhn, and Imre Lakatos among others have undermined the positivist supposition that scientific progress does in fact follow Descartes' doubting rules of method. The literary, epistemological, and methodological strands have not yet wound into one cord, but they belong together. On the eve of the Cartesian revolution the French philosopher and educational reformer, Peter Ramus (*fl.* 1550), brought to completion a medieval tendency to relegate rhetoric to mere eloquence, leaving logic in charge of reason. In the textbooks that Descartes himself read as a boy probable argument was made thus for the first time wholly subservient to indubitable argument. Hostile to classical rhetoric, such a reorganization of the liberal arts was well suited for the Cartesian program extending over the next three centuries to put knowledge on foundations built by philosophy and mathematics. The program failed, and in the meantime probable argument languished. In Richard Rorty's words, following Dewey, the search for the foundations of knowledge by Descartes, Locke, Hume, Kant, Russell, and Carnap was "the triumph of the quest for certainty over the quest for wisdom" (Rorty, 1979, p. 61; *cf.* John Dewey, 1929, pp. 33, 227). To reinstate rhetoric properly understood is to reinstate wider and wiser reasoning.

The reaction to the narrowing of argument by the Cartesian program is by now broad. Its leading figures range from professional philosophers (Stephen Toulmin,

[1] After recognizing the intent, Colin Forster of Australian National University suggested the title "The Last Paper on Methodology." But ambition must have limits.

Paul Feyerabend, Richard Rorty) to a miscellany of practitioners-turned-philosophers in chemistry (Michael Polanyi), law (Chaim Perelman), and literary criticism (Wayne Booth). The reach of the idea nowadays that argument is more than syllogism is illustrated well by the lucid treatment of it in what would seem an unlikely place, by Glenn Webster, Ada Jacox, and Beverly Baldwin in "Nursing Theory and the Ghost of the Received View" (1981, pp. 25–35). The reach, however, has not extended to economics. Austrian, institutionalist, and Marxist economists, to be sure, have for a century been attacking certain parts of positivism as the basis for economic knowledge. But they have seized on other parts with redoubled fervor, and have so expressed their remaining doubts as to make them unintelligible to anyone but themselves. In their own way they have been as narrowing as thoroughgoing positivists—the rejection of econometrics, for instance, would be reasonable only if its more naive claims were taken seriously. For the rest, economists have let philosophical scribblers of a few years back supply their official thinking about what a good argument is.

II. *The Official Methodology of Economics Is Modernist*

Economists have two attitudes towards discourse, the official and unofficial, the explicit and the implicit. The official rhetoric, to which they subscribe in the abstract and in methodological ruminations, declares them to be scientists in the modern mode. The credo of Scientific Method, known mockingly among its many critics as the Received View, is an amalgam of logical positivism, behaviorism, operationalism, and the hypothetico-deductive model of science. Its leading idea is that all sure knowledge is modeled on the early 20th century's understanding of certain pieces of 19th century physics. To emphasize its pervasiveness in modern thinking well beyond scholarship it is best labeled simply "modernism," that is, the notion (as Booth puts it) that we know only what we cannot doubt and cannot really know what we can merely assent to.

Among the precepts of modernism are:

(1) Prediction (and control) is the goal of science.
(2) Only the observable implications (or predictions) of a theory matter to its truth.
(3) Observability entails objective, reproducible experiments.
(4) If (and only if) an experimental implication of a theory proves false is the theory proved false.
(5) Objectivity is to be treasured; subjective "observation" (introspection) is not scientific knowledge.
(6) Kelvin's Dictum: "When you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind."[2]
(7) Introspection, metaphysical belief, aesthetics, and the like may well figure in the discovery of an hypothesis but cannot figure in its justification.
(8) It is the business of methodology to demarcate scientific reasoning from non-scientific, positive from normative.
(9) A scientific explanation of an event brings the event under a covering law.
(10) Scientists, for instance economic scientists, have nothing to say as scientists about values, whether of morality or art.

[2] From Sir William Thomson (Lord Kelvin), *Popular Addresses*, edition of 1888–1889, quoted in Kuhn, 1977, p. 178n. An approximation to this version is inscribed on the front of the Social Science Research Building at the University of Chicago. Frank Knight, the famous University of Iowa economist, is said to have remarked on it one day: "Yes, and when you can express it in numbers your knowledge is of a meagre and unsatisfactory kind."

(11) Hume's Fork: "When we run over libraries, persuaded of these principles, what havoc must we make? If we take in our hand any volume—of divinity or school metaphysics, for instance—let us ask, *Does it contain any abstract reasoning concerning quantity or number?* No. *Does it contain any experimental reasoning concerning matter of fact and existence?* No. Commit it then to the flames, for it can contain nothing but sophistry and illusion" (italics his [1748], 1955, p. 173).

Few in philosophy now believe as many as half of these propositions. A substantial, respectable, and growing minority believes none of them. But a large majority in economics believes them all.

For instance, the leading methodologists in economics do. It is odd but true that modernism in economic methodology is associated with the Chicago School.[3] The main texts of economic modernism, such as Milton Friedman's "The Methodology of Positive Economics" (1953) or Gary Becker and George Stigler's *"De Gustibus Non Est Disputandum"* (1977), bear a Chicago postmark; and the more extreme interpretations of the texts flourish among economists bearing a Chicago degree. What is odd about it is that a group so annoying to other economists in most of its activities should have their assent in the matter of official method: Oddly, a watered down version of Friedman's essay is part of the intellectual equipment of most economists, and its arguments come readily to their lips.

Premeditated writings on method, not excluding Chicago's own, are more careful than the remark in the course of other business that reveals modernism in its rawer form. In precept one can be vague enough to earn the assent of everyone; in practice one must make enemies. Kalman Cohen and Richard Cyert, to take one among many examples of first-chapter methodology in economics texts, present in their book an outline of modernism, which they assert is the method "used in all scientific analyses" (1975, p. 17). The "method" they then outline, with a bibliography heavily weighted towards logical positivism and its allies, reduces to an appeal to be honest and thoughtful. Only when such a phrase as "at least in principle testable by experiment and observation" (p. 23) is given content by practice do we know what is at stake. To be sure, vague precepts are not without their uses. When Friedman wrote, for instance, the practice of economics was split into theory without fact and fact without theory. His modernist incantations, supported by choruses of philosophers, were at the time probably good for the souls of all concerned.

Friedman's essay was even then more post-modernist than one might suppose from slight acquaintance with its ideas. He did, for example, mention with approval the aesthetic criteria of simplicity and fruitfulness that an economist might use to select among a multiplicity of theories with the same predictions, though in the next sentence he attempted to reduce them to objective matters of prediction (p. 10). He accepted that questionnaires, forbidden to the modernist in economics, are useful for suggesting hypotheses, though in the next sentence he asserted that they are "almost entirely useless as a means of *testing* the validity of economic hypotheses" (p. 31n). He emphasized the role of the rhetorical community to which the scientist speaks in producing conviction—whether made up of sociologists, say, or of economists—though in the next sentence he returned to an "objective" theory of testing. Like Karl Popper, Friedman appeared to be struggling to escape the grip of positivism and its intellectual traditions, though with only sporadic success. Perhaps that the *locus classicus* of economic modernism contains so much that is anti-modernist indicates that modernism cannot survive intelligent discussion even by its best advocates.

[3] Nothing in this essay is meant to give comfort to the enemies of Chicago. Having long been a victim of their anti-Chicago dogmatism, I am not impressed by the assertion that Chicago economics is peculiarly dogmatic. Chicago is merely a particularly clear and candid version of a dogmatic impulse common to all economics, expressing itself in methodological imperatives. Economists appear to believe that economics is too important to be left to the open-minded, and especially must never be left to anyone lacking faith in some approved formula for achieving knowledge. Chicago is no worse than the rest. *Immo, civis Chicagonus sum, subspecies TP (cf.* Melvin Reder, 1982).

The unpremeditated remark in the heat of economic argument, however, usually has a crudely modernist content, often in Friedman's very words. An article by Richard Roll and Stephen Ross on finance, for instance, asserts that "the theory should be tested by its conclusions, not by its assumptions" and that "similarly, one should not reject the conclusions derived from firm profit maximization on the basis of sample surveys in which managers claim that they trade off profit for social good" (1980, p. 1093 and footnote). The same can be found elsewhere, in nearly identical terms, all dating back to Friedman's essay: William Sharpe (1970, p. 77), for instance, writing on the same matter as Roll and Ross, takes it as a rule of polite scientific behavior that "the realism of the assumptions matters little. If the implications are reasonably consistent with observed phenomena, the theory can be said to 'explain' reality" (1970, p. 77). Repeated often, and exhibiting modernism as well in their devotion to objective evidence, quantifiable tests, positive analysis, and other articles of the faith, such phrases have the ring of incantation. Modernism is influential in economics, but not because its premises have been examined carefully and found good. It is a revealed, not a reasoned, religion.

III. *Modernism Is a Poor Method*

Modernism Is Obsolete in Philosophy

There are a great many things wrong with modernism as a methodology for science or for economic science.[4] Even when philosophically inclined, economists appear to read about as much in professional philosophy as philosophers do in professional economics. It is unsurprising, then, that the news of the decline of modernism has not reached all ears. From a philosopher's point of view the worst flaw in the hostility to the "metaphysics" that modernism sees everywhere is that the hostility is itself metaphysical. If metaphysics is to be cast into the flames, then the methodological declarations of the modernist family from Descartes through Hume and Comte to Russell and Hempel and Popper will be the first to go. For this and other good reasons philosophers agree that strict logical positivism is dead, raising the question whether economists are wise to carry on with their necrophilia.[5]

In the economic case the metaphysical position akin to logical positivism is not well argued, probably because its roots lie more in the philosophizing of physicists from Mach to Bridgeman than in the parallel thinking of professional philosophers. It is at least obscure what might be the appeal of "operationally meaningful statements" (Paul Samuelson, 1947, p. 3 and

[4] The overdiscussed question of whether there can be a value-free social science will not be much discussed here, but it must be accounted one of the chief failings of modernism that it places moral argument outside the pale of rational discussion. In this connection it should be more widely known that Morris Schlick, the founder of the Vienna Circle of logical positivism and a vigorous lecturer on the theme that moral knowledge is no knowledge at all, was murdered in 1936 by one of his students.

[5] See John Passmore, 1967. Karl Popper quotes Passmore with approval for the motto of a chapter of his own entitled "Who Killed Logical Positivism?" (Popper, 1976, pp. 87–90), in which he confesses to the murder.

throughout) or "valid and meaningful predictions about phenomena not yet observed" (Friedman, p. 7) as standards against which all but mathematical assertions are to be judged. Samuelson, Friedman, or their followers do not present reasons for adopting such metaphysical positions, except for confident assertions, at the time correct, that they were the received views of philosophers on the method of science. The trust in philosophy was a tactical error, for the philosophy itself has since changed. Some philosophers now doubt the entire enterprise of epistemology and its claim to provide foundations for knowledge (Richard Rorty, 1982b). A great many doubt the prescriptions of modernist methodology.

Falsification Is Not Cogent

A prescription that economic methodologists have in common, for instance, is an emphasis on the crucial falsifying test, supposedly the hallmark of scientific reasoning. But philosophers have recognized for many decades that falsification runs afoul of a criticism made by the physicist and philosopher Pierre Duhem in 1906, evident at once without philosophical reading to an economist who has tried to use falsification for science. Suppose that the hypothesis H ("British businessmen performed very poorly relative to Americans and Germans in the late 19th century") implies a testing observation O ("Measures of total factor productivity in iron and steel show a large difference between British and foreign steelmaking"); it implies it, that is, not by itself, but only with the addition of ancillary hypotheses H_1, H_2, and so forth that make the measurement possible ("Marginal productivity theory applies to Britain 1870–1913"; "British steel had no hidden inputs offsetting poor business leaderships"; and so forth). Then of course not-O implies not-H—or not-H_1 or not-H_2 or any number of failures of premises irrelevant to the main hypothesis in question. The hypothesis in question is insulated from crucial test by the ancillary hypotheses necessary to bring it to a test. This is no mere possibility but the substance of most scientific disagreement: "Your experiment was not properly controlled"; "You have not solved the identification problem"; "You have used an equilibrium (competitive, single-equation) model when a disequilibrium (monopolistic, 500-equation) model is relevant." And even if the one hypothesis in question could be isolated, the probabilistic nature of hypotheses, most especially in economics, makes crucial experiments non-crucial: chance is the ever present alternative, the H_n that spoils falsificationism.

Prediction Is Impossible in Economics

The common claim that prediction is the defining feature of a real science, and that economics possesses the feature, is equally open to doubt. It is a cliché among philosophers and historians of science, for instance, that one of the most successful of all scientific theories, the theory of evolution, has no predictions in the normal sense, and is therefore unfalsifiable by prediction. It is at least suggestive of something odd in prediction as a criterion for useful economics that Darwin's theory was inspired by classical economics, a system as it happens erroneous in most of the predictions it made. With no apparent awareness of the incongruity, Friedman quoted Alchian's revival of the connection (Armen Alchian, 1950) in the midst of his most famous piece of predictionist metaphysics ("the leaves are positioned as if each leaf deliberately sought to maximize the amount of sunlight it receives").

In any event, predicting the economic future is, as Ludwig von Mises put it, "beyond the power of any mortal man" (1949, p. 867). What puts it beyond his power

is the very economics he uses to make the prediction. When the economist for a big bank predicts lower interest rates after Christmas, and has not before the prediction placed his net worth in margin loans on bonds, properly hedged and insured against variance, he is behaving either irrationally or self-deceivingly. If he knows the expected value of the future, he for some reason chooses not to take the unlimited wealth that such Faustian knowledge can surely bring, and is willing for some reason instead to dissipate the opportunity by the act of telling others about it. If he does not really know, then he faces no such unexploited opportunity. But then he has perhaps no business talking as though he does. Predictionism cannot be rescued by remarking that the big bank economist makes only conditional predictions. Conditional predictions sell for their value in a soft market: if the sea were to disappear, a rock would accelerate in falling from sea level to the sea floor at about 32.17 feet per second per second. But a serious prediction has serious boundary conditions. If it does it must answer again the American Question: If you're so smart why aren't you rich? At the margin (because that is where economics works) and on average (because some people are lucky) the industry of making economic predictions, which includes universities, earns only normal returns.

Modernism Itself Is Impossible, and Is Not Followed

The most damaging, however, of these lesser criticisms of the modernist methodology is that if taken at its word it is narrow to absurdity. Consider again the steps to modernist knowledge, from predictionism through Kelvin's Dictum to Hume's Fork. If economists (or physicists) confined themselves to economic (or physical) propositions that literally conformed to such steps they would have nothing to say. Cartesian or Humean skepticism is too corrosive a standard of belief for a real human. As the chemist and philosopher Michael Polanyi put it, the methodology of modernism sets up "quixotic standards of valid meaning which, if rigorously practiced, would reduce us all to voluntary imbecility" (1962, p. 88). Modernism promises knowledge free from doubt, metaphysics, morals, and personal conviction; what it delivers merely renames as Scientific Method the scientist's and especially the economic scientist's metaphysics, morals, and personal convictions. It cannot, and should not, deliver what it promises. Scientific knowledge is no different from other personal knowledge (Polanyi, 1962). Trying to make it different, instead of simply better, is the death of science.

In other words, the literal application of modernist methodology cannot give a useful economics. The best proofs are historical. In his *Against Method* (1975) Paul Feyerabend uses an interpretation of Galileo's career to attack the claims of prescriptive methodology in physics; the same point can be made about economics. Had the modernist criterion of persuasion been adopted by Galileo's contemporaries, he argues, the Galilean case would have failed. A grant proposal to use the strange premise that terrestrial optics applied also to the celestial sphere, to assert that the tides were the sloshing of water on a mobile earth, and to suppose that the fuzzy views of Jupiter's alleged moons would prove, by a wild analogy, that the planets, too, went around the sun as did the moons around Jupiter would not have survived the first round of peer review in a National Science Foundation of 1632, at any rate if that one (unlike ours) were wedded to modernist ideology. The argument applies widely to the history of physics: observational anomalies in the experiments testing Einstein's theories were ignored for many years, to be revealed as errors of measurement long after the theories had been embraced, embraced on grounds of "the reason of the matter," as Einstein was fond of saying (Feyerabend, 1975, pp. 56–57).

Historians of biology have uncovered one after another case of cooking the statistical results to fit modernist precepts of what counts as evidence, from Pasteur and Mendel down to the present. The measurement of IQ has been a scandal of self-deception and bold fraud in the name of scientific method from its beginning (Stephen Jay Gould, 1981). Perhaps modernism fits poorly the complexities of biology and psychology: straining after evidence of a sort typically available only in the simplest experiments in physics may not suit their frontiers. It suits the frontiers of economics poorly enough. For better or worse the Keynesian revolution in economics would not have happened under the modernist legislation recommended for the method of science. The Keynesian insights were not formulated as statistical propositions until the early 1950s, well after the bulk of younger economists had become persuaded they were true. By the early 1960s liquidity traps and accelerator models of investment, despite failures in their statistical implementations, were taught to first-year students of economics as matters of scientific routine. Modernist methodology would have stopped all this in 1936: where was the evidence of an objective, statistical, controlled kind?

Nor was the monetarist counterrevolution a success for modernist methodology, though so powerful had the methodology become by the 1960s in the minds of economists and especially of monetarist economists that most of the explicit debate took place in its terms. Yet in truth crude experiments and big books won the day, by their very crudeness and bigness. The Kennedy tax cut boosted the Keynesians to their peak of prestige; the inflation of the 1970s brought them down again, leaving the monetarists as temporary kings of the castle. An important blow for monetarism was Friedman and Schwartz' big book, *A Monetary History of the United States, 1867–1960.* It established a correlation, which Keynesians would not deny, between money and money income. The significance of the correlation, however, depended on the assumption that money caused prices and that money was determinable by the monetary authority (in 1929–1933, for example) despite the openness of the American economy to trade in both goods and money itself. Nonetheless, what was telling in the debate was the sheer bulk of the book—the richness and intelligence of its arguments, however irrelevant most of the arguments were to the main point.

A modernist method thoroughly applied, in other words, would probably stop advances in economics. What empirical anomaly in the traditional tale inspired the new labor economics or the new economic history? None: they were merely realizations that the logic of economics had not exhausted its applicability at conventional borders. What observable implications justify the investment of intellect since 1950 in general equilibrium theory? For all the modernist talk common among its theorists, none; but so what? Could applications of economics to legal questions rely entirely on objective evidence? No; but why would one wish to limit the play of understanding? And so forth. There is nothing to be gained and a great deal to be lost by adopting modernism in economic methodology.

The very point is economic. In order for an economic theory to be tested, Ronald Coase points out, some economists must care enough about it to bother. They care only when it is believed by some investigators—they and their allies or some significant group of opponents. Only when many believe is there a demand for tests. Fortunately, "economists, or at any rate enough of them, do not wait to discover

whether a theory's predictions are accurate before making up their minds"; to wait in proper modernist style "would result in the paralysis of scientific activity" (Coase, 1982, p. 14) because no one would have an incentive to choose one out of the infinite number of hypotheses for test. Even quantitative studies, he argues, rely heavily on pre-quantitative arguments founding belief, and he quotes with approval T. S. Kuhn's remark that "the road from scientific law to scientific measurement can rarely be traveled in the reverse direction" (Coase, p. 18, quoting Kuhn, 1977, p. 219). The laws come from a rhetoric of tradition or introspection, and in physics as in economics "quantitative studies . . . are explorations with the aid of a theory" (Coase, p. 17), searches for numbers with which to make specific a theory already believed on other grounds (see Edward Leamer, 1978, and the discussion below). Modernism is impractical.

Any Method Is Arrogant and Pretentious

The objections to modernist method so far, however, are lesser ones. The greater objection is simply that modernism *is* a method. It sets up laws of argument drawn from an ideal science or the underlying history of science or the essence of knowledge. The claim is that the philosopher of science can tell what makes for good, useful, fruitful, progressive science. He knows this so confidently that he can limit arguments that worthy scientists make spontaneously, casting out some as unscientific, or at best placing them firmly in the "context of discovery." The philosopher undertakes to second-guess the scientific community. In economics the claim of methodological legislation is that the legislator is not merely expert in all branches of economic knowledge within sound of his proclamations but expert in all possible future economics, limiting the growth of economics now in order to make it fit a philosopher's idea of the ultimate good.

It is hard to take such claims seriously. Einstein remarked that "Whoever undertakes to set himself up as a judge in the field of Truth and Knowledge is shipwrecked by the laughter of the gods" (Einstein, 1953, p. 38). Modernism sets up a court of the Red Queen ("Normative argument," she says, "off with his head"), and the gods laugh merrily. Any methodology that is law-making and limiting will do so. It will do so with the noblest intentions, but economists are fond of pointing out in like cases that noble intentions can have bad consequences. The methodologist fancies himself the judge of the practitioner. His proper business, though, is an anarchistic one, resisting the rigidity and pretension of rules. I. A. Richards applied the point to the theory of metaphor: "Its business is not to replace practice, or to tell us how to do what we cannot do already; but to protect our natural skill from the interference of unnecessarily crude views about it" (1936, p. 116).

The crudeness of modernist methodology, or of any methodology reducible to rigid precept, is bad: but that it is allowed to interfere with practice is worse. The custom of methodological papers in economics is to scold economists for not allowing it to interfere more. Mark Blaug's useful book summarizing the state of play of economic methodology in 1980, *The Methodology of Economics: Or How Economists Explain*, is a recent case in point. It would be better subtitled "How the Young Karl Popper Explained," since it repeatedly attacks extant arguments in economics for failing to comply with the rules Popper laid down in *Logik der Forschung* in 1934. Blaug's exordium is typical of the best of the methodologists in economics: "Economists have long been aware of the need to defend 'correct' principles of reasoning in their subject; although actual practice may bear little relationship to what is preached, the preaching is worth considering on its own ground" (Blaug, p. xii). Words like these flow easily from a modernist's pen. But why would preaching unrelated to actual practice be worth considering at all? Why do economists have to defend in the abstract their principles of reasoning, and before what tribunal? A case for having a methodology—whether logical positivist or Popperian or Austrian or Marxist—would be expected to give answers to the questions of why, but commonly does not. Recent philosophy of science and ordinary good sense suggest that it cannot. Blaug's peroration is frankly prescriptive, taking economic rhetoric directly from philosophy:

> What methodology can do is to provide criteria for the acceptance and rejection of research programs, setting standards that will help us to discriminate between wheat and chaff. The ultimate question we can and indeed must pose about any research program is the one made familiar by Popper: what events, if they materialize, would lead us to reject that program? A program that cannot meet that question has fallen short of the highest standards that scientific knowledge can attain [1980, p. 264].

It sounds grand, but Einstein's gods are rolling in the aisles. Why should a dubious epistemological principle be any test of practice, much less the ultimate test? And doesn't science take place most of the time well short of the ultimate?

Anyone would commend the vision of science that Popper and his followers have—of science as a self-correcting exploration verging on the dialectic otherwise so foreign to the analytic tradition in philosophy. For an economic scientist to adopt an obdurate refusal to consider objections and to resist offering hostages to evidence, though as common in modernist as in nonmodernist circles, is not merely unscientific; it is cowardly. So much one can take from the idea of falsification by evidence. The problem comes, and the modernist preaching begins, with the word "evidence." Should it all be "objective," "experimental," "positive," "observable"? Can it be? In *The Open Society and Its Enemies* (1945) Popper closes the borders of his society to psychoanalysts and Marxists on the grounds that they do not conform to the modernist notion of evidence prevalent there. He would also have to close it to physicists from Galileo Galilei to particle charmers. An economist bracero, surely, would be deported on the next truck from such an open intellectual society.

Other Sciences Do Not Follow Modernist Methods

For all its claims to the scientific priesthood, then, economics is different from the man-in-the-street's image of Science. Economists should be glad that their subject fits poorly with this image and well with the New Rhetoric, as do studies long foreign to economics such as the study of literature or law or politics. Economics, in other words, is not a Science in the way we came to understand that word in high school.

But neither, really, are other sciences. Other sciences, even the other mathematical sciences, even the Queen herself, are rhetorical. Mathematics appears to an *incognoscento* to be the limiting example of objectivity, explicitness, and demonstrability. Surely here is bedrock for belief. Yet standards of mathematical demonstration change. The last fifty years have been a disappointment to followers of David Hilbert and his program to put mathematics on indubitable foundations. The historian of mathematics, Morris Kline, wrote recently that "it is now apparent that the concept of a universally accepted, infallible body of reasoning—the majestic mathematics of 1800 and the pride of man—is a grand illusion." Or again:

> There is no rigorous definition of rigor. A proof is accepted if it obtains the endorsement of the leading specialists of the time and employs the principles that are fashionable at the moment. But no standard is universally acceptable today [1980, pp. 6, 315].

The recent flap over a computerized proof of the four-color proposition is one example. The more fundamental example is said to be Kurt Gödel's proof fifty years ago that some true and statable propositions in mathematics are unprovable. The point is controversial. John van Heijenoort writes that "the bearing of Gödel's results on epistemological problems remains uncertain. . . . [T]hey should not be rashly called upon to establish the primacy of some act of intuition that would dispense with formalization" (1967, p. 357). To be sure. But one need not dispense with formalization and flee to an unexamined act of intuition to think that formalization has limits.

Kline's opinions are somewhat loosely expressed, and unpopular among mathematicians. Apparently less so are those of Philip J. Davis and Reuben Hersh, whose recent book *The Mathematical Experience* (1981) was described in the *American Mathematical Monthly* as "one of the masterpieces of our age." Davis and Hersh speak of the crisis of confidence in modern mathematical philosophy, however, in terms nearly identical with Kline's. In the work of The Ideal Mathematician "the line between complete and incomplete proof is always somewhat fuzzy, and often controversial" (p. 34; cf. p. 40). They quote a living Ideal Mathematician, Solomon Feferman, who writes "it is also clear that the search for ultimate foundations via formal systems has failed to arrive at any convincing conclusion" (p. 357). Without using the word, Davis and Hersh argue that what is required is a rhetoric of mathematics:

> The dominant style of Anglo-American philosophy . . . tends to perpetuate identification of the philosophy of mathematics with logic and the study of formal systems. From this standpoint, a problem of principal concern to the mathematician becomes totally invisible. This is the problem of giving a philosophical account . . . of preformal mathematics. . . , including an examination of how [it] relates to and is affected by formalization [1981, p. 344].

They assert that "informal mathematics *is* mathematics. Formalization is only an abstract possibility which no one would want or be able actually to carry out" (p. 349). Real proofs "are established by 'consensus of the qualified'" and are "not checkable . . . by any mathematician not privy to the gestalt, the mode of thought in the particular field. . . . It may take generations to detect an error" (p. 354). They conclude:

> The actual experience of all schools—and the actual daily experience of mathematicians—shows that mathematical truth, like other kinds of truth, is fallible and corrigible. . . . It is reasonable to propose a different task for mathematical philosophy, not to seek indubitable truth, but to give an account of mathematical knowledge as it really is—fallible, corrigible, tentative, and evolving, as is every other kind of human knowledge [p. 406].

Not much in this line has been done, though one astounding piece has shown what can be: Imre Lakatos' *Proofs and Refutations: The Logic of Mathematical Discovery* gives an account for a theorem in topology of the rhetoric of mathematics.

It appears, then, that some deep problems facing mathematics are problems of rhetoric, problems in "the art of probing what men believe they ought to believe." Similar points can be made about other sciences, such as paleontology. The sudden proliferation of species at the beginning of the Cambrian period, one of the great puzzles in evolution, was explained by Steven Stanley in 1973 by supposing the sudden arrival of forms of life that fed on other forms of life, single-celled herbivores, as it were, in a grassy sea. Their grazing on the dominant forms allowed new forms to survive the competition from the previously dominant ones, which in turn resulted in new grazers. Stephen Jay Gould remarks of the arguments offered in support of this brilliant and persuasive theory that:

> . . . they do not correspond to the simplistic notions about scientific progress that are taught in most high schools and advanced by most media. Stanley does not invoke proof by new information obtained from rigorous experiment. His second criterion is a methodological presumption, the third a philosophical preference, the fourth an application of prior theory. . . . Science, at its best, interposes human judgment and ingenuity upon all its proceedings. It is, after all (although we sometimes forget it), practiced by human beings [Gould, 1977, p. 125].

One can even say the same of physics, that favorite of outsiders seeking a prescription for real, objective, positive, predictive science. The sequence Carnap-Popper-Lakatos-Kuhn-Feyerabend represents in the history and philosophy of physics a descent, accelerating recently, from the frigid peaks of scientific absolutism to the sweet valleys of anarchic rhetoric (see Popper, 1934, 1976; Lakatos, 1970; Kuhn, 1970; Feyerabend, 1975, 1978). If economics should imitate other sciences, imitate even the majesty of physics and mathematics (there is, to be sure, considerable doubt that it should), then it should officially open itself to a wider range of discourse.

IV. *The Unofficial Rhetoric Is Honorable But Unexamined*

Econometric Rhetoric Is Too Narrow

But unofficially it does. The second attitude towards discourse is that adopted in actual scientific work in economics. It is different from the official, modernist rhetoric. What is alarming about the workaday rhetoric is not its content but that it is unexamined, and that in consequence the official rhetoric pops up in mischievous ways. Economists agree or disagree—their disagreements are exaggerated—but they do not know why. Any economist believes more than his evidence of a suitably modernist and objective sort implies. A recent poll of economists, for example, found that only three percent of those surveyed flatly disagreed with the assertion that "tariffs and import quotas reduce general economic welfare." Only two percent disagreed with the assertion that "a ceiling on rents reduces the quantity and quality of housing available." Only eight percent disagreed with the assertion that "the taxing and spending of government has a significant impact on the income of a partly idle economy" (J. R. Kearl, Clayne Pope, Gordon Whiting, and Larry Wimmer, 1979). You probably fall into the 97, 98, and 92 percent majorities. The evidence for the assertions, however, is obscure. How do economists know these statements are true? Where did they acquire such confidence? The usual answer is that "theory tells us." But great social questions are not answered by looking at a diagram on a blackboard, because it is trivially easy to draw a diagram that yields the opposite answer. The factual experience of the economy, certainly, has little to do with their confidence. No study has shown in ways that would satisfy a consistent modernist, for example, that high tariffs in America during the 19th century, on balance, hurt Americans. Yet it is believed that tariffs hurt then and now.[6] No study has shown that an inadvertent policy of fiscal ease brought unemployment down during the War. Yet it is believed on all sides. Economists have not considered their rhetoric.

Everywhere in the literature of economics one is met with premises that are unargued, tricks of style masquerading as reason ("it is evident that"), forms of evidence that ignore the concerns of the audience, and other symptoms of a lack of self-consciousness in rhetoric. The lack is most evident in quarrels across research paradigms. Some economists (I am one) believe that peasants are rational. The mass of modernist proofs, originating largely from Chicago, that resistance to the "Green Revolution" or persistence in scattering plots of land are rational leave many other economists cold. Some economists (I am one) believe that competition is a robust characterization of the modern American economy. The mass of modernist proofs, originating largely from Chicago, that, for instance, advertising has small effects on profits leaves the others cold. Why? Why do Chicago proofs leave Texas institutionalists or NYU Austrians or Massachusetts Marxists or even Berkeley neoclassicists cold? The non-Chicago economists, of course, believe they have modernist evidence of their own. But part of the problem is that they also believe, without thinking about it much, that they have evidence of a non-modernist sort: stories of peasants and their lumpish character in the flesh; self-awareness of the force of advertising. A good part of the disagreement is over evidence that is not brought openly into the discussion, though it is used.

Even in the most narrowly technical matters of scientific discussion economists have a shared set of convictions about what makes an argument strong, but a set which they have not examined, which they can communicate to graduate students only tacitly, and which contains many elements embarrassing to the official rhetoric. A good example is the typical procedure in econometrics. From economic theory, politics, and the workings of the economist's psyche, all of which are in the rhetorical sense unexamined, come hypotheses about some bit of the economy. The hypotheses are then specified as straight lines, linear models being those most easily manipulated. The straight lines are fitted to someone else's collection of facts. So far the official and workaday rhetoric correspond, and the one might with justice be called a guide to the other. Presently, however, they diverge. If the results of the fitting to the data are reasonable, on grounds that are not themselves subject to examination, the article is sent off to a journal. If the results are unreasonable, the hypothesis is consigned to a do loop: the economic scientist returns to the hypotheses or the specifications, altering them until a publishable article emerges. The product may or may not have value, but it does not acquire its value from its adherence to the official rhetoric. It violates the official rhetoric blatantly.

But why shouldn't it? Even at the level of tests of statistical significance the workaday rhetoric violates the philosopher's law. But so what? It is a cliché of cynicism in economics and related statistical fields to point out that a result significantly different from one that may have been gotten by chance does not have the significance it claims if the hypothesis has been manipulated to fit the data. That only significant results get published has long been a scandal among statistical purists: they fear with some reason that at the five percent level of significance something like five percent of the computer runs will be successful. The scandal is not, however, the failure to achieve modernist standards of scientific purity. The scandal is the failure to articulate reasons why one might want to ignore them.

It would be arrogant to suppose that one knew better than thousands of intelligent and honest economic scholars what the proper form of argument was. The Received View is arrogant in this way, laying down legislation for science on the basis of epistemological convictions held with vehemence inversely proportional to the amount of evidence that they work. Better to look hard at what is in fact done. In an important book that is an exception

[6] Charles Peirce, the founder of pragmatism, related in 1877 how he had been "entreated not to read a certain newspaper lest it might change my opinion upon free-trade" (p. 101).

to the general neglect of rhetorical considerations in econometrics Edward Leamer asks what purpose the workaday procedures in econometrics may be serving (Leamer, 1978, esp. p. 17). Instead of comparing them with a doctrine in the philosophy of science he compares them with reasons that ought to persuade a reasonable person, with what really warrants assent, with, in short, economic rhetoric. As Christopher Sims points out in a review, "there is a myth that there are only two categories of knowledge about the world—'the' model, given to us by 'economic theory,' without uncertainty, and the parameters, about which we know nothing except what the data, via objectively specified econometric methods, tells us. . . . The sooner Leamer's cogent writings can lead us to abandon this myth, to recognize that nearly all applied work is shot through with applications of uncertain, subjective knowledge, and to make the role of such knowledge more explicit and more effective, the better" (Sims, 1979, p. 567). Yes. The very title of Leamer's book is an outline of rhetoric in econometrics: *Specification Searches: Ad Hoc Inference with Nonexperimental Data*.

Examples of the search abound. It is common in a seminar in economics for the speaker to present a statistical result, apparently irrefutable by the rules of positive economics, and to be met by a chorus of "I can't believe it" or "It doesn't make sense." Milton Friedman's own Money Workshop at Chicago in the late 1960s and the early 1970s was a case in point. Put in statistical language, the rhetorical context that creates such skepticism can be called a priori beliefs and can be analyzed in Bayesian terms. It seldom is, but such a step would not be enough even if it were taken. That the rhetorical community in economics might reject "solid" results, for instance that oil prices appear in a regression explaining inflation, and accept "flimsy" ones, for instance that money causes inflation, shows the strength of prior beliefs. (The beliefs can be reversed without changing the example.) To leave the discussion at prior beliefs, however, perhaps formalizing them as prior probability distributions, is to perpetuate the fact-value split of modernism, leaving most of what matters in science to squeals of pleasure or pain. What is required is an examination of the workaday rhetoric that leads to the prior beliefs. It is not enough, as Thomas F. Cooley and Stephen F. LeRoy do in their recent, penetrating paper on "Identification and Estimation of Money Demand" (1981) to merely stand appalled at the infection of econometric conclusions by prior beliefs. If econometric argument does not persuade it is because the field of argument is too narrow, not because the impulse towards thoughtfulness and explicitness which it embodies is wrong. The arguments need to be broadened, not merely dismissed.

The Controversy Over Purchasing Power Parity Is an Example of Unexamined Rhetoric

A good example of how the official rhetoric—in the absence of an examination of the workaday rhetoric—can lead a literature in economics astray, especially in econometric matters, is the debate about purchasing power parity. It is worth examining in detail as a case study in unexamined rhetoric and the need to broaden it (Donald McCloskey and J. Richard Zecher, 1982). The question is: is the international economy more like the economy of the Midwest, in which Iowa City and Madison and Champaign all face given prices for goods; or is it more like the solar system, in which each planet's economy is properly thought of in isolation? If the Iowa City view is correct, then the prices of all goods will move together everywhere, allowing for exchange rates. If the Martian view is correct they will move

differently. If the Iowa City view is correct, then all closed models of economies, whether Keynesian or monetarist or rationally expecting, are wrong; if the Martian view is correct, then economists can (as they do) go on testing macroeconomic faiths against American experience since the War.

The question of whether prices are closely connected internationally, then, is important. The official rhetoric does not leave much doubt as to what is required to answer it: collect facts on prices in, say, the United States and Canada and . . . well . . . test the hypothesis (derived in orthodox fashion from a higher order hypothesis, using objective data, looking only at observable facts, controlling the experiment as much as possible, and so forth, according to the received view). A large number of economists have done this. Half of them conclude that purchasing power parity works; the other half conclude that it fails. A misleading but nonetheless superb paper by Irving Kravis and Robert Lipsey on the subject concludes that it fails, in terms that are worth repeating:

> We think it ***unlikely*** that the ***high*** degree of national and international commodity arbitrage that many versions of the monetarist [sic] theory of the balance of payments contemplate is ***typical*** of the real world. This is not to deny that the price structures of the advanced industrial countries ***are linked*** together, but it is to suggest that the links are ***loose*** rather than ***rigid*** [1978, p. 243, italics added].

Every italicized word involves a comparison against some standard of what constitutes unlikelihood or highness or typicality or being linked or looseness or rigidity. Yet here and elsewhere in the tortured literature of purchasing power parity no standard is proposed.

The narrowest test of purchasing power parity, and the one that springs most readily to a mind trained in the official rhetoric, is to regress the price in the United States (of steel or of goods-in-general, in levels or in differences) against the corresponding price abroad, allowing for the exchange rate. If the slope coefficient is 1.00 the hypothesis of purchasing power parity is said to be confirmed; if not, not. Kravis and Lipsey perform such a test. Being good economists they are evidently made a little uncomfortable by the rhetoric involved. They admit that "Each analyst will have to decide in the light of his purposes whether the purchasing power parity relationships fall close enough to 1.00 to satisfy the theories" (p. 214). Precisely. In the next sentence, however, they lose sight of the need for an explicit standard if their argument is to be cogent: "As a matter of general judgment we express our opinion that the results do not support the notion of a tightly integrated international price structure." They do not say what a "general judgment" is or how one might recognize it. The purpose of an explicit economic rhetoric would be to provide guidance. The guidance Kravis and Lipsey provide for evaluating their general judgment is a footnote (p. 214) reporting the general judgments of Houthakker, Haberler, and Johnson that deviations from parity of anything under 10 to 20 percent are acceptable to the hypothesis. It happens, incidentally, that the bulk of the evidence offered by Kravis and Lipsey passes rather than fails such a test, belying their conclusions. But accepting or rejecting one unargued standard by comparing it with another unargued standard does not much advance the art of argument in economics.

Kravis and Lipsey, to be fair, are unusually sensitive to the case for having some standard, more sensitive than are most economists working the field. They return repeatedly to the question of a standard, though without resolving it. On page 204 they reject in one irrelevant sentence the only standard proposed in the literature so far, the Genberg-Zecher criterion, described below. On pages 204–05 and on 235 and again on 242 they draw a distinction between the statistical and the economic significance of their results. So frequently do they make the point that it must be counted one of the major ones in the paper. On page 205 they remark, for example, that even small differences between domestic and export prices can make a big difference to the incentive to export: "this is a case in which statistical significance [that is, a correlation of the two prices near 1.0, which one might mistakenly suppose to imply that they were insignificantly different] does not necessarily connote economic significance." Yet they do not turn the sword on themselves. No wonder: without a rhetoric of *economic* significance, and in the face of a modernist rhetoric of statistical significance with the prestige of alleged science behind it, they are unaware they are wielding it.

The abuse of the word "significant" in connection with statistical arguments in economics is universal. Statistical significance seems to give a standard by which to judge whether a hypothesis is true or false that is independent of any tiresome consideration of how true a hypothesis must be to be true enough. The point in the present case is that the "failure" of purchasing power parity in a regression of the usual type is not measured against a standard. How close does the slope have to be to the ideal of 1.00 to say that purchasing power parity succeeds? The literature is silent. The standard used is the irrelevant one of statistical significance. A sample size of a million yielding a tight estimate that the slope was .9999, "significantly" different from 1.00000, could be produced as evidence that purchasing power parity had "failed," at least if the logic of the usual method were to be followed consistently. Common sense, presumably, would rescue the scholar from asserting that an estimate of .9999 with a standard error of .0000001 was significantly different from unity in a significant meaning of significance. Such common sense should be applied to findings of slopes of .90 or 1.20. It is not.[7]

The irrelevance of the merely statistical standard of fit does not undermine only that half of the empirical literature that finds purchasing power parity to be wrong. Towards the end of a fine article favorable to purchasing power parity, Paul Krugman writes:

> There are several ways in which we might try to evaluate purchasing power parity as a theory. We can ask how much it explains [that is, R-square]; we can ask how large the deviations from purchasing power parity are in some absolute sense; and we can ask whether the deviations from purchasing power parity are in some sense systematic [1978, p. 405].

The defensive usage "in some absolute sense" and "in some sense" betrays his unease, which is in the event justified. There is no "absolute sense" in which a description is good or bad. The sense must be comparative to a standard, and the standard must be argued.

Similarly, Jacob Frenkel, an enthusiast for purchasing power parity as such things go among economists but momentarily bewitched by the ceremony of regression, says that "if the market is efficient and if the forward exchange rate is an unbiased forecast of the future spot exchange rate, the constant [in a regression of the spot rate today on the future rate for today quoted yesterday] . . . should not differ *significantly* from unity." In a footnote on the next page, speaking of the standard errors of the estimates for such an equation in the 1920s, he argues that "while these results indicate that markets were efficient and that on average forward rates were unbiased forecasts of future spot rates, the 2–8 percent errors were *significant*" (1978, pp. 175–76, italics added). He evidently has forgotten his usage of "significant" in another signification. What he appears to mean is that he judges a 2–8 percent error to be large in some unspecified economic sense, perhaps as offering significant profits for lucky guessers of the correct spot rate. In any event, it is unclear what his results imply about their subject, purchasing power parity, because significance in statistics, however useful it is as an input into economic significance, is not the same thing as economic significance.

The point is not that levels of significance are arbitrary. Of course they are. The point is that it is not known whether the range picked out by the level of significance affirms or denies the hypothesis. Nor is the point that econometric tests are to be disdained. Quite the contrary. The point is that the econometric tests have not followed their own rhetoric of hypothesis testing. Nowhere in the literature of tests of purchasing power parity does there appear a loss function. We do not know how much it will cost in policy wrecked or analysis misapplied or reputation ruined if purchasing power parity is said to be true when by the measure of the slope coefficient it is only, say, 85 percent true. That is, the argument due to Neyman and Pearson that undergirds modern econometrics has been set aside here as elsewhere in favor of a merely statistical standard, and an irrelevant one related to sampling error at that. We are told how improbable it is that a slope coefficient of .90 came from a distribution centered on 1.00 in view of the one kind of error we claim we know about (unbiased sampling error with finite variance), but we are not told whether it matters to the truth of purchasing power parity where such limits of confidence are placed.

Silence on the matter is not confined to the literature of purchasing power parity. Most texts on econometrics do not mention that the goodness or badness of a hypothesis is not ascertainable on merely statistical grounds. Statisticians themselves are more self-conscious, although the transition from principle to practice is sometimes awkward. A practical difficulty in the way of using the Neyman and Pearson theory in pure form, A. F. Mood and F. A. Graybill say, is that

> the loss function is not known at all or else it is not known accurately enough to warrant its use. If the loss function is not known, it seems that a decision function that in some sense minimizes the error probabilities will be a reasonable procedure [1963, p. 278].

The phrase "in some sense" appears to be a marker of unexplored rhetoric in the works of intellectually honest scholars. In any event, the procedure they suggest might be reasonable for a general statistician, who makes no claim to know what is a good or bad approximation to truth in fields outside statistics. It is not reasonable for a specialist in international trade or macroeconomics. If the loss function is not known it should be discovered. And that will entail a study of the question's rhetoric.

One standard of economic significance in questions of parity, for example, might be the degree to which the customary regressions between countries resembled similar regressions within a single country. We agree, for purposes of argument, that the United States is to be treated as a single point in space, as one economy across which distances are said not to matter for the purposes of thinking about inflation or the balance of payments. Having done so we have a standard: is Canada economically speaking just as closely integrated with the United States as is California with

[7] An example is J. D. Richardson's paper, "Some Empirical Evidence on Commodity Arbitrage and the Law of One Price" (1978). He regresses Canadian on American prices multiplied by the exchange rate for a number of industries and concludes: "It is notable that the 'law of one price' fails uniformly. The hypothesis of *perfect* commodity arbitrage is rejected with 95 percent confidence for every commodity group" (p. 347, italics added). The question is, why in an imperfect world would it matter that *perfect* arbitrage is rejected?

Massachusetts? Is the Atlantic Economy as closely integrated as the American Economy? The standard is called the Genberg-Zecher criterion, after its inventors (Hans Genberg, 1976; McCloskey and Zecher, 1976). It is not the only conceivable one. The degree of market integration in some golden age (1880–1913 perhaps; or 1950–1970) might be a standard; the profits from arbitrage above normal profits might be another standard; the degree to which an X percentage deviation from purchasing power parity does or does not disturb some assertion about the causes of inflation might be still another. The point is to have standards of argument, to go beyond the inconclusive rhetoric provided by the pseudo-scientific ceremony of hypothesis-regression-test-publish in most of modern economics.

V. *The Rhetoric of Economics Is a Literary Matter*

Even a Modernist Uses, and Must Use, Literary Devices

The mere recognition that the official rhetoric might be dubious, then, frees the reason to examine how economists really argue. Obscured by the official rhetoric the workaday rhetoric has not received the attention it deserves, and the knowledge of it is therefore contained only in seminar traditions, advice to assistant professors, referee reports (a promising primary source for its study), and jokes. It is significant that George Stigler, a leader of modernism in economics, chose to express his observations about the rhetoric of economics in a brilliantly funny "The Conference Handbook" ("Introductory Remark number E: 'I can be very sympathetic with the author; until 2 years ago I was thinking along similar lines' ") rather than as a serious study in one of the several fields he has mastered, the history of economic thought (1977). The attitude of the Handbook is that rhetoric is mere rhetoric, mere game playing in aid of ego gratification; the serious business of Science will come on that happy day when the information theory of oligopoly or the vulgar Marxist theory of the state is brought to a critical test under the auspices of naive falsificationism.

Economists can do better if they will look soberly at the varieties of their arguments. The varieties examined here can only be crude preliminaries to a fuller study, a study that might dissect samples of economic argument, noting in the manner of a literary or philosophical exegesis exactly how the arguments sought to convince the reader. It is not obvious a priori what the categories might be; in view of the methodological range of modern economics they doubtless would vary much from author to author. A good place to start might be the categories of classical rhetoric, Aristotle's divisions into invention, arrangement, delivery, and style, for instance, with his paired sub-headings of artificial (i.e., argumentative) and inartificial (i.e., factual) proofs, syllogism and example, and the like. A good place to continue would be the procedures of modern literary critics, bright people who make their living thinking about the rhetoric of texts.

The purpose would not be to make the author look foolish or to uncover fallacies for punishment by ridicule—fallacymongering is evidence of a legislative attitude towards method, and it is no surprise that Jeremy Bentham, confident of his ability to legislate for others in matters of method as in education, prisons, and government, had compiled from his notes *The Book of Fallacies* (1824). David Hackett Fischer's book, *Historians' Fallacies* (1970), has this flaw: that it takes as fallacious what may be merely probable and supporting argument.

The purpose of literary scrutiny of economic argument would be to see beyond the received view on its content. Two pages (pp. 122–23) chosen literally at random from that premier text of the received view, and a local maximum in economic scholarship, Samuelson's *Foundations*, will suffice for illustration:

(1) To begin with he gives a general mathematical form from which detailed results in comparative statics can be obtained by reading across a line. The implication of the lack of elaboration of the mathematics is that the details are trivial (leading one to wonder why they are mentioned at all). An "interesting" special case is left "as an exercise to the interested reader," drawing on the rhetorical traditions of applied mathematics to direct the reader's mind in the right directions. The mathematics is presented in an offhand way, with an assumption that we all can read off partitioned matrices at a glance, inconsistent with the level of mathematics in other passages. The air of easy mathematical mastery was important to the influence of the book, by contrast with the embarrassed modesty with which British writers at the time (Hicks most notably) pushed mathematics off into appendices.

Samuelson's skill at mathematics in the eyes of his readers, an impression nurtured at every turn, is itself an important and persuasive argument. He presents himself as an authority, with good reason. That the mathematics is so often pointless, as here, is beside the point. Being able to do such a difficult thing (so it would have seemed to the typical economist reading in 1947) is warrant of expertise. The argument is similar in force to that of a classical education conspicuously displayed. To read Latin like one's mother tongue and Greek like one's aunt's tongue is extremely difficult, requiring application well beyond the ordinary; therefore—or so it seemed to Englishmen around 1900—men who have acquired such a skill should have charge of a great empire. Likewise—or so it seemed to economists around 1983—those who have acquired a skill at partitioned matrices and eigenvalues should have charge of a great economy. The argument is not absurd or a "fallacy" or "mere rhetoric." Virtuosity *is* some evidence of virtue.[8]

(2) There are six instances of appeal to authority (C. E. V. Leser, Keynes, Hicks, Aristotle, Knight, and Samuelson; appeal to authorities is something of a Samuelsonian specialty). Appeal to authority is often reckoned as the worst kind of "mere" rhetoric. Yet it is a common and often legitimate argument, as here. No science would advance without it, because no scientist can redo every previous argument. We stand on the shoulders of giants, and it is a perfectly legitimate and persuasive argument to point this out from time to time.

(3) There are several appeals to relaxation of assumptions. The demand for money is "really interesting . . . when uncertainty . . . is admitted." Again, the implicit assumption in Hicks that money bears no interest is relaxed, unhitching the interest rate from the zero return on money. Relaxation of assumptions is the literature generating function of modern economics. In the absence of quantitative evidence on the importance of the assumption relaxed it is no modernist evidence at all. Samuelson is careful to stick to the subjunctive mood of theory (money "*would* pass out of use"), but no doubt wants his strictures on a theory of the interest rate based merely on liquidity preference (that is, on risk) to be taken seriously as comments on the actual world. They are, surely, but not on the operationalist grounds he articulates when preaching methodology.

(4) There are several appeals to hypo-

[8] The limiting case is spelling. Most college teachers will agree that those who do not know how to spell "consensus" lose some of their authority to speak on it.

thetical toy economies, constrained to one or two sectors, from which practical results are derived. This has since Ricardo been among the commonest forms of economic argument, the Ricardian vice. It is no vice if done reasonably. "It would be quite possible to have an economy in which money did not exist, and in which there was still a substantial rate of interest." Yes, of course.

(5) There is, finally, one explicit appeal to analogy, which is said to be "not . . . superficial." Analogy, as will be shown in detail in a moment, pervades economic thinking, even when it is not openly analogical: transaction "friction," yield "spread," securities "circulating," money "withering away" are inexplicit examples here from one paragraph of live or only half-dead metaphors. Yet analogy and metaphor, like most of the other pieces of Samuelson's rhetoric, have no standing in the official canon.

Most of the Devices Are Only Dimly Recognized

The range of persuasive discourse in economics is wide, ignored in precept while potent in practice. At the broadest level it is worth noting that the practice of economic debate often takes the form of legal reasoning, for, as Booth put it, "the processes developed in the law are codifications of reasonable processes that we follow in every part of our lives, even the scientific" (1974, p. 157). Economists would do well to study jurisprudence, then, with some other aim than subordinating it to economic theory. For instance, economists, like jurists, argue by example, by what Edward Levi calls "the controlling similarity between the present and prior case" (1967, p. 7).

The details of the pleading of cases at economic law have little to do with the official scientific method. Without self-consciousness about workaday rhetoric they are easily misclassified. A common argument in economics, for example, is one from verbal suggestiveness. The proposition that "the economy is basically competitive" may well be simply an invitation to look at it this way, on the assurance that to do so will be illuminating. In the same way a psychologist might say "we are all neurotic"—it does not mean that 95 percent of a randomly selected sample of us will exhibit compulsive handwashing; rather, it is merely a recommendation that we focus attention on the neurotic ingredient "in us all" (Passmore, 1966, p. 438). To misunderstand the expression as a properly modernist hypothesis would be to invite much useless testing. The case is similar to $MV=PT$ understood as an identity. The equation is the same term-for-term as the equation of state of an ideal gas, and has the same status as an irrefutable but useful notion in chemistry as it has in economics. The identity *can* be argued against, but not on grounds of "failing a test." The arguments against it will deny its capacity to illuminate, not its modernist truth.

Another common argument in economics with no status in the official rhetoric is philosophical consistency: "If you assume the firm knows its own cost curve you might as well assume it knows its production function, too: it is no more dubious that it knows one than the other." The argument, usually inexplicit though signalled by such a phrase as "it is natural to assume," is in fact characteristic of philosophical discourse (Passmore, 1970). It is analogous to symmetry as a criterion of plausibility, which appears in many forms and forums. A labor economist tells a seminar about compensating differentials for the risk of unemployment, referring only to the utility functions of the workers. An auditor remarks that the value of unemployment on the demand side (that is, to the firm) is not included.

The remark is felt to be powerful, and a long discussion ensues of how the demand side might alter the conclusions. The argument from the-other-side-is-empty is persuasive in economics, but economists are unaware of how persuasive it is.

Likewise (and here we reach the border of self-consciousness in rhetoric), "ad hoccery" is universally condemned by seminar audiences. An economist will cheerfully accept a poor R^2 and terrible and understated standard errors if only she "has a theory" for the inclusion of such-and-such a variable in the regressions. "Having a theory" is not so open and shut as it might seem, depending for instance on what reasoning is prestigious at the moment. Anyone who threw accumulated past output into an equation explaining productivity change before 1962 would have been accused of ad hoccery. But after Arrow's essay on "The Economics of Learning By Doing" (which as it happened had little connection with maximizing behavior or other higher order hypotheses in economics), there was suddenly a warrant for doing it.

An example of the rhetoric of economics which falls well within the border of self-consciousness is simulation. Economists will commonly make an argument for the importance of this or that variable by showing its potency in a model with back-of-the-envelope estimates of the parameters. Common though it is, few books or articles are devoted to its explication (but, see Richard Zeckhauser and Edith Stokey, 1978). It would be as though students learned econometrics entirely by studying examples of it—no bad way to learn, but not self-conscious in grounding the arguments. What is legitimate simulation? Between A. C. Harberger's modest little triangles of distortion and Jeffrey Williamson's immense multiequation models of the American or Japanese economies since 1870 is a broad range. Economists have no vocabulary for criticizing any part of the range. They can deliver summary grunts of belief or disbelief but find it difficult to articulate their reasons in a disciplined way.

VI. *Economics Is Heavily Metaphorical*

Models Are Metaphors

The most important example of economic rhetoric, however, falls well outside the border of self-consciousness. It is the language economists use, and in particular its metaphors. To say that markets can be represented by supply and demand "curves" is no less a metaphor than to say that the west wind is "the breath of autumn's being." A more obvious example is "game theory," the very name being a metaphor. It is obviously useful to have in one's head the notion that the arms race is a two-person, negative-sum cooperative "game." Its persuasiveness is instantly obvious, as are some of its limitations. Each step in economic reasoning, even the reasoning of the official rhetoric, is metaphor. The world is said to be "like" a complex model, and its measurements are said to be like the easily measured proxy variable to hand. The complex model is said to be like a simpler model for actual thinking, which is in turn like an even simpler model for calculation. For purposes of persuading doubters the model is said to be like a toy model that can be manipulated quickly inside the doubter's head while listening to the seminar. John Gardner wrote:

> There is a game—in the 1950s it used to be played by the members of the Iowa Writers' Workshop—called 'Smoke.' The player who is 'it' [thinks of] some famous person . . . and then each of the other players in turn asks one question . . . such as 'What kind of weather are you?' . . . Marlon Brando, if weather, would be sultry and uncertain. . . . To understand that Marlon Brando is a certain kind of weather is to discover something (though something neither useful nor demonstrable) and in

> the same instant to communicate something [Gardner, 1978, pp. 118–19].

On the contrary, in economics the comparable discovery is useful and by recourse to rhetorical standards demonstrable.

Metaphors in Economics Are Not Ornamental

Metaphor, though, is commonly viewed as mere ornament. From Aristotle until the 1930s even literary critics viewed it this way, as an amusing comparison able to affect the emotions but inessential for thought. "Men are beasts": if we cared to be flat-footed about it, the notion was, we could say in what literal way we thought them beastly, removing the ornament to reveal the core of plain meaning underneath. The notion was in 1958 common in philosophy, too:

> With the decline of metaphysics, philosophers have grown less and less concerned about Godliness and more and more obsessed with cleanliness, aspiring to ever higher levels of linguistic hygiene. In consequence, there has been a tendency for metaphors to fall into disfavour, the common opinion being that they are a frequent source of infection [H. J. N. Horsburgh, 1958, p. 231].

Such suspicion toward metaphor is widely recognized by now to be unnecessary, even harmful. That the very idea of "removing" an "ornament" to "reveal" a "plain" meaning is itself a metaphor suggests why. Perhaps thinking is metaphorical. Perhaps to remove metaphor is to remove thought. The operation on the metaphoric growth would in this case be worse than the disease.

The question is whether economic thought is metaphorical in some nonornamental sense. The more obvious metaphors in economics are those used to convey novel thoughts, one sort of novelty being to compare economic with noneconomic matters. "Elasticity" was once a mind-stretching fancy; "depression" was depressing; "equilibrium" compared an economy to an apple in a bowl, a settling idea; "competition" once induced thoughts of horseraces; money's "velocity" thoughts of swirling bits of paper. Much of the vocabulary of economics consists of dead metaphors taken from noneconomic spheres.

Comparing noneconomic with economic matters is another sort of novelty, apparent in the imperialism of the new economics of law, history, politics, crime, and the rest, and most apparent in the work of that Kipling of the economic empire, Gary Becker. Among the least bizarre of his many metaphors, for instance, is that children are durable goods. The philosopher Max Black pointed out that "a memorable metaphor has the power to bring two separate domains into cognitive and emotional relation by using language directly appropriate to the one as a lens for seeing the other" (1962, p. 236). So here: the subject (a child) is viewed through the lens of the modifier (a durable good). A beginning at literal translation would say, "A child is costly to acquire initially, lasts for a long time, gives flows of pleasure during that time, is expensive to maintain and repair, has an imperfect second-hand market. . . . Likewise, a durable good, such as a refrigerator. . . ." That the list of similarities could be extended further and further, gradually revealing the differences as well—"children, like durable goods, are not objects of affection and concern"; "children, like durable goods, do not have their own opinions"—is one reason that, as Black says, "metaphorical thought is a distinctive mode of achieving insight, not to be construed as an ornamental substitute for plain thought" (p. 237). The literal translation of an important metaphor is never finished. In this respect and in others an important metaphor in economics has the quality admired in a successful scientific

theory, a capacity to astonish us with implications yet unseen.[9]

But it is not merely the pregnant quality of economic metaphors that makes them important for economic thinking. The literary critic I. A. Richards was among the first to make the point, in 1936, that metaphor is "two thoughts of different things *active together, . . . whose meaning is a resultant of their interaction*" (Richards, 1936, p. 93, my italics; Black, 1962, p. 46; Owen Barfield, 1947, p. 54). A metaphor is not merely a verbal trick, Richards continues, but "a borrowing between and intercourse of *thoughts*, a transaction between contexts" (p. 94, his italics). Economists will have no trouble seeing the point of his economic metaphor, one of mutually advantageous exchange. The opposite notion, that ideas and their words are invariant lumps unaltered by combination, like bricks (Richards, p. 97), is analogous to believing that an economy is a mere aggregation of Robinson Crusoes. But the point of economics since Smith has been that an island-full of Crusoes trading is different from and often better off than the mere aggregation.

Another of Becker's favorite metaphors, "human capital," illustrates how two sets of ideas, in this case both drawn from inside economics, can thus mutually illuminate each other by exchanging connotations. In the phrase "human capital" the field in economics treating human skills was at a stroke unified with the field treating investment in machines. Thought in both fields was improved, labor economics by recognizing that skills, for all their intangibility, arise from abstention from consumption; capital theory by recognizing that skills, for all their lack of capitalization, compete with other investments for a claim to abstention. Notice by contrast that because economists are experts only in durable goods and have few (or at any rate conventional) thoughts about children, the metaphor that children are durable goods has so to speak only one direction of flow. The gains from the trade were earned mostly by the theory of children (fertility, nuptiality, inheritance), gaining from the theory of durable goods, not the other way around.

Economic Metaphors Constitute a Poetics of Economics

What is successful in economic metaphor is what is successful in poetry, and is analyzable in similar terms. Concerning the best metaphors in the best poetry, comparing thee to a summer's day or comparing *A* to *B*, argued Owen Barfield,

> We feel that *B*, which is actually said, ought to be necessary, even inevitable in some way. It ought to be in some sense the best, if not the only way, of expressing *A* satisfactorily. The mind should dwell on it as well as on *A* and thus the two should be somehow inevitably fused together into one simple meaning [Barfield, 1947, p. 54].

If the modifier *B* (a summer's day, a refrigerator, a piece of capital) were trite—in these cases it is not, although in the poem Shakespeare was more self-critical of his simile than economists usually are of theirs—it would become as it were detached from *A*, a mechanical and unilluminating correspondence. If essential, it fuses with *A*, to become a master metaphor of the science, the idea of "human capital," the idea of "equilibrium," the idea of "entry and exit," the idea of "competition." The metaphor, quoth the poet, is the "consummation of identity."

[9] A good metaphor depends on the ability of its audience to suppress incongruities, or to wish to. Booth gives the example of:

> 'All the world's a stage' . . . [The reader must make a choice only if the incongruities—failures of fit—come too soon]. Usually they arrive late and without much strength. . . . [W]e have no difficulty ruling from our attention, in the life-stage metaphor, the selling of tickets, fire insurance laws, the necessity for footlights [1961, p. 22f].

The appreciation of "human capital" requires the same suspension of disbelief.

Explicitness	Extent: Short	Extent: Long
Explicit	simile (The firm behaves as if it were one mind maximizing its discounted value.)	tiresome caution (repeat the simile)
Middling	metaphor (human capital)	allegory (economics of education using human capital)
Implicit	symbol (income) (demand curve)	a symbol system: a mathematics; a theory (Keynesian theory of income determination) (supply and demand analysis)

Figure 1. Analogical Thinking Has Two Dimensions (And Economic Cases in Point)

Few would deny, then, that economists frequently use figurative language. Much of the pitiful humor available in a science devoted to calculations of profit and loss comes from talking about "islands" in the labor market or "putty-clay" in the capital market or "lemons" in the commodity market. The more austere the subject the more fanciful the language. We have "turnpikes" and "golden rules" in growth theory, for instance, and long disquisitions on what to do with the "auctioneer" in general equilibrium theory. A literary man with advanced training in mathematics and statistics stumbling into *Econometrica* would be astonished at the metaphors surrounding him, lost in a land of allegory.

Allegory is merely long-winded metaphor, and all such figures are analogies. Analogies can be arrayed in terms of explicitness, with simile ("as if") the most explicit and symbol ("the demand curve") the least explicit; and they can be arrayed by extent.

Economists, especially theorists, are forever spinning "parables" or telling "stories." The word "story" has in fact come to have a technical meaning in mathematical economics, though usually spoken in seminars rather than written in papers. It means an extended example of the economic reasoning underlying the mathematics, often a simplified version of the situation in the real world that the mathematics is meant to characterize. It is an allegory, shading into extended symbolism. The literary theories of narrative could make economists self-conscious about what use the story serves. Here the story is the modifier, the mathematics the subject. A tale of market days, traders with bins of shmoos, and customers with costs of travel between bins illuminates a fixed point theorem.

Even Mathematical Reasoning Is Metaphorical

The critical question is whether the opposite trick, modifying human behavior with mathematics, is also metaphorical. If it were not, one might acknowledge the metaphorical element in verbal economics about the "entrepreneur," for instance, or more plainly of the "invisible hand," yet argue that the linguistic hygiene of mathematics leaves behind such fancies. This indeed was the belief of the advanced thinkers of the 1920s and 1930s who inspired the now-received view in economic method. Most economists subscribe to the belief without doubt or comment or thought. When engaging in verbal economics we are more or less loose, it is said, taking literary license with our "story"; but when we do mathematics we put away childish things.

But mathematical theorizing in economics is metaphorical, and literary. Consider, for example, a relatively simple case, the theory of production functions. Its vocabulary is intrinsically metaphorical. "Aggregate capital" involves an analogy of "capital" (itself analogical) with

something—sand, bricks, shmoos—that can be "added" in a meaningful way; so does "aggregate labor," with the additional peculiarity that the thing added is no thing, but hours of conscientious attentiveness; the very idea of a "production function" involves the astonishing analogy of the subject, the fabrication of things, about which it is appropriate to think in terms of ingenuity, discipline, and planning, with the modifier, a mathematical function, about which it is appropriate to think in terms of height, shape, and single valuedness.

The metaphorical content of these ideas was alive to its inventors in the 19th century. It is largely dead to 20th-century economists, but deadness does not eliminate the metaphorical element. The metaphor got out of its coffin in an alarming fashion in the Debate of the Two Cambridges in the 1960s. The debate is testament, which could be multiplied, to the importance of metaphorical questions to economics. The very violence of the combat suggests that it was about something beyond mathematics or fact. The combatants hurled mathematical reasoning and institutional facts at each other, but the important questions were those one would ask of a metaphor—is it illuminating, is it satisfying, is it apt? How do you know? How does it compare with other economic poetry? After some tactical retreats by Cambridge, Massachusetts on points of ultimate metaphysics irrelevant to these important questions, mutual exhaustion set in, without decision. The reason there was no decision was that the important questions were literary, not mathematical or statistical. The continued vitality of the idea of an aggregate production function in the face of mathematical proofs of its impossibility and the equal vitality of the idea of aggregate economics as practiced in parts of Cambridge, England in the face of statistical proofs of its impracticality would otherwise be a great mystery.

Even when the metaphors of one's economics appear to stay well and truly dead there is no escape from literary questions. The literary man C. S. Lewis pointed out in 1939 that any talk beyond the level of the-cow-standing-here-is-in-fact-purple, any talk of "causes, relations, of mental states or acts . . . [is] incurably metaphorical" (1962, p. 47). For such talk he enunciated what may be called Screwtape's Theorem on Metaphor, the first corollary of which is that the escape from verbal into mathematical metaphor is not an escape:

> when a man claims to think independently of the buried metaphor in one of his words, his claim may . . . [be] allowed only in so far as he could really supply the place of that buried metaphor. . . . [T]his new apprehension will usually turn out to be itself metaphorical [p. 46].

If economists forget and then stoutly deny that the production function is a metaphor, yet continue talking about it, the result is mere verbiage. The word "production function" will be used in ways satisfying grammatical rules, but will not signify anything. The charge of meaninglessness applied so freely by modernists to forms of argument they do not understand or like sticks in this way to themselves. Lewis' second corollary is that "the meaning in any given composition is in inverse ratio to the author's belief in his own literalness" (p. 27). An economist speaking "literally" about the demand curve, the national income, or the stability of the economy is engaging in mere syntax. Lewis cuts close to the bone here, though sparing himself from the carnage:

> The percentage of mere syntax masquerading as meaning may vary from something like 100 percent in political writers, journalists, psychologists, and economists, to something like forty percent in the writers of children's stories. . . . The mathematician, who seldom forgets that his symbols are symbolic, may often rise for short stretches to ninety percent of meaning and ten of verbiage [p. 49].

If economists are not comparing a social fact to a one-to-one mapping, thus bringing two separate domains into cognitive and emotional relation, they are not thinking:

> I've never slapped a curved demand;
> I never hope to slap one.
> But this thing I can tell you now:
> I'd rather slap than map one.

Literary Thinking Reunifies the Two Cultures

Metaphor, then, is essential to economic thinking, even to economic thinking of the most formal kind. One may still doubt, though, whether the fact matters. For it is possible for rhetoricians as well as unreconstructed modernists to commit the Philosophizing Sin, to bring high-brow considerations of the ultimate into discussions about how to fix a flat tire. Pushkin's poetry may be ultimately untranslatable, in view of the difference in language, to be sure, but also the difference in situation between a Pushkin in Russia in the early 19th century and a bilingual translator in New York in the late 20th century. Because he was a different man speaking to a different audience even Nabokov's brilliant translation, in the words of the economist and litterateur, Alexander Gerschenkron, "can and indeed should be studied but . . . cannot be read" (in Steiner, 1975, p. 315). Our intrinsic loneliness will make some nuance dark. Yet crude translation, even by machine, is useful for the workaday purposes of informing the Central Intelligence Agency (for instance, "out of sight, out of mind" = "blind madman"). Likewise, the intrinsic metaphors of language may make it ultimately impossible to communicate plain meaning without flourishes—the flourishes *are* the meaning. But the economist may be able to get along without full awareness of his meaning for the workaday purposes of advising the Central Intelligence Agency.

So it might be argued. But it should be argued cautiously. Self-consciousness about metaphor in economics would be an improvement on many counts. Most obviously, unexamined metaphor is a substitute for thinking—which is a recommendation to examine the metaphors, not to attempt the impossible by banishing them.[10] Richard Whately, D.D., Archbishop of Dublin, publicist for free trade as for other pieces of classical political economy, and author of the standard work in the 19th century on *The Elements of Rhetoric*, drew attention to the metaphor of a state being like an individual, and therefore benefiting like an individual from free trade. But he devoted some attention, not all of it ironic, to the question of the aptness of the figure:

> To this is it replied, that there is a great difference between a Nation and an Individual. And so there is, in many circumstances . . . [he enumerates them, mentioning for instance the unlimited duration of a Nation] and, moreover, the transactions of each man, as far as he is left free, are regulated by the very person who is to be a gainer or loser by each,—the individual himself; who, though his vigilance is sharpened by interest, and his judgment by exercise in his own department, may chance to be a man of confined education, possessed of no general principles, and not pretending to be versed in philosophical theories; whereas the affairs of a State are regulated by a Congress, Chamber of Deputies, etc., consisting perhaps of men of extensive reading and speculative minds [1894, p. 63].

The case for intervention cannot be put better. And the metaphor is here an occasion for and instrument of thought, not a substitute.

Metaphors, further, evoke attitudes that are better kept in the open and under the control of reasoning. This is plain in the ideological metaphors popular with parties: the invisible hand is so very discrete, so soothing, that we might be inclined to accept its touch without protest;

[10] An example of a naïve attack on economic metaphors, and of a failure to realize that economic theory is itself armed with metaphor, is the first page of McCloskey (1981).

the contradictions of capitalism are so very portentous, so scientifically precise, that we might be inclined to accept their existence without inquiry. But it is true even of metaphors of the middling sort. The metaphors of economics convey the authority of Science, and often convey, too, its claims to ethical neutrality. It is no use complaining that we didn't *mean* to introduce moral premises. We do. "Marginal productivity" is a fine, round phrase, a precise mathematical metaphor that encapsulates a most powerful piece of social description. Yet it brings with it an air of having solved the moral problem of distribution facing a society in which people cooperate to produce things together instead of producing things alone. It is irritating that it carries this message, because it may be far from the purpose of the economist who uses it to show approval for the distribution arising from competition. It is better, though, to admit that metaphors in economics can contain such a political message than to use the jargon innocent of its potential.

A metaphor, finally, selects certain respects in which the subject is to be compared with the modifier; in particular, it leaves out the other respects. Max Black, speaking of the metaphor "men are wolves," notes that "any human traits that can without undue strain be talked about in 'wolf-language' will be rendered prominent, and any that cannot will be pushed into the background" (1962, p. 41). Economists will recognize this as the source of the annoying complaints from non-mathematical economists that mathematics "leaves out" some feature of the truth or from non-economists that economics "leaves out" some feature of the truth. Such complaints are often trite and ill-formed. The usual responses to them, however, are hardly less so. The response that the metaphor leaves out things in order to simplify the story temporarily is disingenuous, occurring as it often does in contexts where the economist is simultaneously fitting 50 other equations. The response that the metaphor will be tested eventually by the facts is a stirring promise, but seldom fulfilled (see again Leamer, throughout). A better response would be that we like the metaphor of, say, the selfishly economic person as calculating machine on grounds of its prominence in earlier economic poetry plainly successful or on grounds of its greater congruence with introspection than alternative metaphors (of people as religious dervishes, say, or as sober citizens). In *The New Rhetoric: A Treatise on Argumentation* (1967), Chaim Perelman and L. Olbrechts-Tyteca note that "acceptance of an analogy . . . is often equivalent to a judgment as to the importance of the characteristics that the analogy brings to the fore" (p. 390). What is remarkable about this unremarkable assertion is that it occurs in a discussion of purely literary matters, yet fits so easily the matters of economic science.

This is in the end the significance of metaphors and of the other rhetorical machinery of argument in economics: economists and other scientists are less separate from the concerns of civilization than many think. Their modes of argument and the sources of their conviction—for instance, their uses of metaphor—are not very different from Cicero's speeches or Hardy's novels. This is a good thing. As Black wrote, discussing "archetypes" as extended metaphors in science: "When the understanding of scientific models and archetypes comes to be regarded as a reputable part of scientific culture, the gap between the sciences and the humanities will have been partly filled" (p. 243).

VII. *Be Not Afraid*

The Alternative to Modernism Is Not Irrationalism

It will be apparent by now that the objectivity of economics is overstated and, what is more important, overrated. Pregnant economic knowledge depends little on, as Michael Polanyi put it, "a scientific rationalism that would permit us to believe only explicit statements based on tangible data and derived from these by a formal inference, open to repeated testing" (1966, p. 62). A rhetoric of economics makes plain what most economists know anyway about the richness and complexity of economic argument but will not state openly and will not examine explicitly.

The invitation to rhetoric, however, is not an invitation to irrationality in argument. Quite the contrary. It is an invitation to leave the irrationality of an artificially narrowed range of arguments and to move to the rationality of arguing like human beings. It brings out into the open the arguing that economists do anyway—in the dark, for they must do it somewhere and the various official rhetorics leave them benighted.

The charge of irrationalism comes easily to the lips of methodological authoritarians. The notion is that reasoning outside the constricted epistemology of modernism is no reasoning at all. Mark Blaug, for instance, charges that Paul Feyerabend's book *Against Method* "amounts to replacing the philosophy of science by the philosophy of flower power" (1980, p. 44). Feyerabend commonly attracts such dismissive remarks by his flamboyance. But Stephen Toulmin and Michael Polanyi are nothing if not sweetly reasonable; Blaug lumps them with Feyerabend and attacks the Feyerabend-flavored whole. On a higher level of philosophical sophistication Imre Lakatos' *Methodology of Scientific Research Programmes* (1978, from articles published from 1963 to 1976) repeatedly tars Polanyi, Kuhn, and Feyerabend with "irrationalism" (e.g., Vol. 1, pp. 9n1, 76n6, 91n1, 130 and 130n3), emphasizing their sometimes aggressively expressed case against rigid rationalism and ignoring their moderately expressed case for wider rationality. The tactic is an old one. Richard Rorty notes that "the charges of 'relativism' and 'irrationalism' once leveled against Dewey [were] merely the mindless defensive reflexes of the philosophical tradition which he attacked" (1979, p. 13; Rorty, 1982a, Ch. 9). The position taken by the opponents of Dewey, Polanyi, Kuhn, and the rest is "if the choice is between Science and irrationality, I'm for Science." But that's not the choice.

The Barbarians Are Not at the Gates

Yet still the doubt remains. If we abandon the notion that econometrics is by itself a method of science in economics, if we admit that our arguments require comparative standards, if we agree that personal knowledge of various sorts plays a part in economic knowledge, if we look at economic argument with a literary eye, will we not be abandoning science to its enemies? Will not scientific questions come to be decided by politics or whim? Is the routine of Scientific Method not a wall against irrational and authoritarian threats to inquiry? Are not the barbarians at the gates?

The fear is a surprisingly old and persistent one. In classical times it was part of the debate between philosophy and rhetoric, evident in the unsympathetic way in which the sophists are portrayed in Plato's dialogues. Cicero viewed himself as bringing the two together, disciplining rhetoric's tendency to become empty advocacy and trope on the one hand and disciplining philosophy's tendency to become useless and inhuman speculation on the other. The classical problem was that rhetoric was a powerful device easily misused for evil ends, the atomic power of the classical world, and like it the subject of worrying about its proliferation. The solution was to insist that the orator be good as well as clever: Cato defined him as *"vir bonus dicendi peritus,"* the good man

skilled at speaking, a Ciceronian ideal as well. Quintilian, a century and a half after Cicero, said that "he who would be an orator must not only *appear* to be a good man, but cannot *be* an orator unless he *is* a good man" (*Institutio* XII, 1, 3). The classical problem looks quaint to moderns, who know well that regressions, radios, computers, experiments, or any of the now canonized methods of persuasion can be and have been used as methods of deceit. There is nothing about anaphora, chiasmus, metonymy, or other pieces of classical rhetoric that make them more subject to evil misuse than the modern methods. One can only note with regret that the Greeks and Romans were more sensitive to the possibility, and less hypnotized by the claims of method to moral neutrality.

The 20th century's attachment to limiting rules of inquiry solves a German problem. In the German Empire and Reich it was of course necessary to propound a split of fact from values in the social sciences if anything was to be accomplished free of political interference. And German speculative philosophy, one hears it said, warranted a logical positivist cure. The German habits, however, have spilled over into a quite different world. It is said that if we are to avoid dread anarchy we cannot trust each scientist to be his own methodologist. We must legislate a uniform though narrowing method to keep scholars from resorting to figurative and literal murder in aid of their ideas. We ourselves could be trusted with methodological freedom, of course, but the others cannot. The argument is a strange and authoritarian one, uncomfortably similar to the argument of, say, the Polish authorities against Solidarity or of the Chilean authorities against free politics. It is odd to hear intellectuals making it. Perhaps their low opinion of the free play of ideas comes from experiences in the faculty senate: the results of academic democracy, it must be admitted, are not so bad an argument for authoritarianism, at least until one looks more closely at the results of authoritarianism. Surely, though, the alternative to blindered rules of modernism is not an irrational mob but a body of enlightened scholars, perhaps more enlightened when freed to make arguments that actually bear on the questions at issue.

There Is No Good Reason to Wish to Make "Scientific" As Against Plausible Statements

The other main objection to an openly rhetorical economics is not so pessimistic. It is the sunny view that scientific knowledge of a modernist sort may be hard to achieve, even impossible, but all will be well on earth and in heaven if we strive in our poor way to reach it. We should have a standard of Truth beyond persuasive rhetoric to which to aspire. In Figure 2 all possible propositions about the world are divided into objective and subjective, positive and normative, scientific and humanistic, hard and soft. The modernist supposes that the world comes divided nicely along such lines.

scientific	humanistic
fact	opinion
objective	subjective
positive	normative
vigorous	sloppy
precise	vague
things	words
cognition	intuition
hard	soft

Figure 2. The Task of Science Is to Move the Line

According to the modernist methodologist the scientist's job is not to decide whether propositions are useful for understanding and changing the world but to classify them into one or the other half, scientific or nonscientific, and to bring as many as possible into the scientific portion. But why? Whole teams of philosophical surveyors have sweated long over the placing of the demarcation line between scientific and other propositions, worrying for instance about whether astrology can be demarcated from astronomy; it was the chief activity of the positivist movement for a century. It is not clear why anyone troubled to do so. People are persuaded of things in many ways, as has been shown for economic persuasion. It is not clear why they should labor at drawing lines on mental maps between one way and another.

The modernists have long dealt with the embarrassment that metaphor, case study, upbringing, authority, introspection, simplicity, symmetry, fashion, theology, and politics serve to convince scientists as they do other folk by labeling these the "context of discovery." The way scientists discover hypotheses has been held to be distinct from the "context of justification," namely, proofs of a modernist sort. Thomas Kuhn's autobiographical reflections on the matter can stand for puzzlement in recent years about this ploy:

> Having been weaned intellectually on these distinctions and others like them, I could scarcely be more aware of their import and force. For many years I took them to be about the nature of knowledge, and . . . yet my attempts to apply them, even *grosso modo*, to the actual situations in which knowledge is gained, accepted, and assimilated have made them seem extraordinarily problematic [Kuhn, 1970, p. 9].

The methodologist's claim is that "ultimately" all knowledge in science can be brought into the hard and objective side of Figure 2. Consequently, in certifying propositions as really scientific there is great emphasis placed on *"conceivable* falsification" and *"some future* test." The apparent standard is the Cartesian one that we can find plausible only the things we cannot possibly doubt. But even this curious standard is not in fact applied: a conceivable but practically impossible test takes over the prestige of the real test, but free of its labor. Such a step needs to be challenged. It is identical to the one involved in equating as morally similar the actual compensation of those hurt during a Pareto optimal move with a hypothetical compensation not actually paid, as in the Hicks-Kaldor test; and it is identically dubious. A properly identified econometric measurement of the out-of-sample properties of macroeconomic policy is "operational," that is, conceivable, but for all the scientific prestige the conceivability lends to talk about it, there are grave doubts whether it is practically possible. While economists are waiting for the ultimate they might better seek wisdom in the humanism of historical evidence on régime changes or of introspection about how investors might react to announcements of new monetary policies. And of course they do.

The point is that one cannot tell whether an assertion is persuasive by knowing at which portion of the scientific/humanistic circle it came from. One can tell whether it is persuasive only by thinking about it. Not all regression analyses are more persuasive than all moral arguments; not all controlled experiments are more persuasive than all introspections. Economic intellectuals should not discriminate against propositions on the basis of race, creed, or epistemological origin. There are some subjective, soft, vague propositions that are more persuasive than some objective, hard, precise propositions.

Take, for instance, the law of demand. The economist is persuaded that he will buy less oil when its price doubles better than he or anyone else is persuaded of the age of the universe. He may reasonably be persuaded of it better than he is that the earth goes around the sun, because not being an astronomer with direct knowledge of the experiments involved

he has the astronomical facts only from the testimony of people he trusts, a reliable though not of course infallible source of knowledge.[11] The economic fact he has mostly from looking into himself and seeing it sitting there. The ceremony of the official rhetoric to the contrary, it is not because the law of demand has predicted well or has passed some statistical test that it is believed—although such further tests are not to be scorned. The "scientific" character of the tests is irrelevant. It may be claimed in reply that people can agree on precisely what a regression coefficient means but cannot agree precisely on the character of their introspection. Even if true (it is not) this is a poor argument for ignoring introspection if the introspection is persuasive and the regression coefficient, infected with identification problems and errors in variables, is not. Precision means low variance of estimation; but if the estimate is greatly biased it will tell precisely nothing.

An extreme case unnecessary for the argument here will make the point clear. You are persuaded that it is wrong to murder better than you are persuaded that inflation is always and everywhere a monetary phenomenon. This is not to say that similar techniques of persuasion will be applicable to both propositions. It says merely that each within its field, and each therefore subject to the methods of honest persuasion appropriate to the field, the one achieves a greater certainty than the other. To deny the comparison is to deny that reason and the partial certitude it can bring applies to nonscientific subjects, a common but unreasonable position. There is no reason why specifically scientific persuasiveness ("at the .05 level the coefficient on M in a regression of prices in 30 countries over 30 years is insignificantly different from 1.0") should take over the whole of persuasiveness, leaving moral persuasiveness incomparably inferior to it. Arguments such as that "murder violates the reasonable moral premise that we should not force other people to be means to our ends" or that "from behind a prenatal veil of ignorance of which side of the murderer's revolver we would be after birth we would enact laws against murder" are persuasive in comparable units. Not always, but sometimes, they are, indeed, more persuasive, better, more probable (Toulmin, 1958, p. 34). We believe and act on what persuades us—not what persuades a majority of a badly chosen jury, but what persuades well educated participants in our civilization and justly influential people in our field. To attempt to go beyond persuasive reasoning is to let epistemology limit reasonable persuasion.

[11] The astronomical "fact" that the earth goes around the sun, of course, is not even a properly modernist fact, though it is commonly treated as one in such discussions. Which goes around which is a matter of the point of view one chooses. It is the aesthetics of the simpler theory, not the "facts," that leads to heliocentrism.

VIII. *The Good of Rhetoric*

Better Writing

Well, what of it? What is to be gained by taking the rhetoric of economics seriously? The question can be answered by noting the burdens imposed by an unexamined rhetoric.

First of all, economics is badly written, written by a formula for scientific prose. The situation is not so bad as it is in, say, psychology, where papers that do not conform to the formula (introduction, survey of literature, experiment, discussion, and so forth) are in some journals not accepted. But economists are stumbling towards conventions of prose that are bad for clarity and honesty. The study of rhetoric, it must be said, does not guarantee the student a good English style. But at least it makes him blush at the disdain for the reader that some economics exhibits (Walter Salant, 1969).

The economist's English contains a message, usually that "I am a Scientist: give way." Occasionally the message is more genial: Zvi Griliches' irony says "Do not make a fetish out of these methods I am expounding: they are mere human artifices." Milton Friedman's style, so careful and clear, has to an exceptional degree the character of the Inquirer. We will not raise up a race of Dennis Robertsons, Robert Solows, George Stiglers, or Robert Lucases by becoming more sensitive to the real messages in scientific procedure and prose, but maybe we will stunt the growth of the other kind.

Better Teaching

A second burden is that economics is badly taught, not because its teachers are boring or stupid, but because they often do not recognize the tacitness of economic knowledge, and therefore teach by axiom and proof instead of by problem-solving and practice. To quote Polanyi yet again:

> . . . the transmission of knowledge from one generation to the other must be predominately tacit. . . . The pupil must assume that a teaching which appears meaningless to start with has in fact a meaning that can be discovered by hitting on the same kind of indwelling [a favorite Polanyi expression] as the teacher is practicing [1966, p. 61].

It is frustrating for students to be told that economics is not primarily a matter of memorizing formulas, but a matter of feeling the applicability of arguments, of seeing analogies between one application and a superficially different one, of knowing when to reason verbally and when mathematically, and of what implicit characterization of the world is most useful for correct economics. Life is hard. As a blind man uses his stick as an extension of his body, so whenever we use a theory "we incorporate it in our body—or extend our body to include it—so that we come to dwell in it." Problem-solving in economics is the tacit knowledge of the sort Polanyi describes.[12] We know the economics, but cannot say it, in the same way a musician knows the note he plays without consciously recalling the technique for executing it. A singer is a prime example, for there is no set of mechanical instructions one can give to a singer on how to hit a high C. Al Harberger often speaks of so-and-so being able to make an economic argument "sing." Like the directions to Carnegie Hall, the answer to the question "how do you get to the Council of Economic Advisors?" is "practice, practice."

Better Foreign Relations

A third burden placed on economics by its modernist methodology is that economics is misunderstood and, when regarded at all, disliked by both humanists and scientists. The humanists dislike it for its baggage of antihumanist methodology. The scientists dislike it because it does not in reality attain the rigor that its methodology claims to achieve. The bad foreign relations have many costs. For instance, as was noted above, economics has recently become imperialistic. There is now an economics of history, of sociology, of law, of anthropology, of politics, of political philosophy, of ethics. The flabby methodology of modernist economics simply makes this colonization more difficult, raising irrelevant methodological doubts in the minds of the colonized folk.

Better Science

A fourth burden is that economists pointlessly limit themselves to "objective" facts, admitting the capabilities of one's own or others' minds as merely sources of hypotheses to be tested, not as themselves arguments for assenting to hypotheses. The modernist notion is that common sense is nonsense, that knowledge must somehow be objective, not *verstehen* or introspection. But, to repeat, we have much information immediately at our disposal about our own behavior as economic molecules, if we would only examine the grounds of our beliefs. The idea that observational proofs of the law of demand, such as the Rotterdam School's multi-equation approach, are more compelling than introspection is especially odd. Even the econometrics itself would be better, as Christopher Sims has recently argued:

> If we think carefully about what we are doing, we will emerge, I think, both more confident that much of applied econometrics is useful, despite its differences from physical science, and more ready to adapt our language and methods to reflect what we are actually doing. The result will be econometrics which is more scientific [by which he means "good"] if less superficially similar to statistical methods used in experimental sciences [Sims, 1982, p. 25].

The curious status of survey research in modern economics is a case in point. Unlike other social scientists, economists are extremely hostile towards questionnaires and other self-descriptions. Secondhand knowledge of a famous debate among economists in the late 1930s is part of an economist's formal education. The debate concerned the case of asking businessmen if they equalized marginal cost to marginal revenue. It is revealing that the failure of such a study—never mind whether that was indeed the study—is supposed to convince economists to abandon all self-testimony. One can literally get an audience of economists to laugh out loud by proposing ironically to send out a questionnaire on some disputed economic point. Economists are so impressed by the confusions that might possibly result from questionnaires that they abandon them entirely, in favor of the confusions resulting from external observation. They are unthinkingly committed to the notion that only the externally observable behavior of economic actors is admissable evidence in arguments concerning economics. But self-testimony is not useless, even for the purpose of resolving the marginal cost-average cost debate of the 1930s. One could have asked "Has your profit margin always been the same?" "What do you think when you find sales lagging?" (Lower profit margin? Wait it out?) Foolish inquiries into motives and foolish use of human informants will produce nonsense. But this is also true of foolish use of the evidence more commonly admitted into the economist's study.

Better Dispositions

A fifth and final burden is that scientific debates in economics are long-lasting and ill-tempered. Journals in geology are not filled with articles impugning the character of other geologists. They are not filled with bitter controversies that drone on from one century to the next. No wonder. Economists do not have an official rhetoric that persuasively describes what economists find persuasive. The mathematical and statistical tools that gave promise in the bright dawn of the 1930s and 1940s of ending economic dispute have not succeeded, because too much has been asked of them. Believing mistakenly that operationalism is enough to end all dispute, the economist assumes his opponent is dishonest when he does not concede the point, that he is motivated by some ideological passion or by self-interest, or that he is simply stupid. It fits the naive fact-value split of modernism to attribute all disagreements to political differences, since facts are alleged to be, unlike values, impossible to dispute. The extent of disagreement among economists, as was mentioned, is in fact exaggerated. The amount of their agreement, however, makes all the more puzzling the venom they bring to relatively minor disputes. The assaults on Milton Friedman or on John Kenneth Galbraith, for example, have a bitterness that is quite unreasonable. If one cannot reason about values, and if most of what

[12] On this score, and some others, I can heartily recommend McCloskey, 1982.

matters is placed in the value half of the fact-value split, then it follows that one will embrace unreason when talking about things that matter. The claims of an overblown methodology of Science merely end conversation.[13]

A rhetorical cure for such disabilities would reject philosophy as a guide to science, or would reject at least a philosophy that pretended to legislate the knowable. The cure would not throw away the illuminating regression, the crucial experiment, the unexpected implication unexpectedly falsified. These too persuade reasonable scholars. Non-argument is the necessary alternative to narrow argument only if one accepts the dichotomies of modernism. The cure would merely recognize the good health of economics, disguised now under the neurotic inhibitions of an artificial methodology of Science.

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[13] Listen to Harry Johnson: "The methodology of positive economics was an ideal methodology for justifying work that produced apparently surprising results without feeling obliged to explain why they occurred" (Johnson, 1971, p. 13). I do not need to think about your evidence for widespread monopolistic competition because my methodology tells me the evidence is irrelevant.

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