

part I

Exordium

ex OR di um; L. “beginning” . . . The first part of a classical oration. It caught the audience’s interest while introducing the subject.

Richard A Lanham, *A Handlist of Rhetorical Terms*, second edition, Berkeley and Los Angeles: University of California Press (1991), p. 49

chapter 1

A positivist youth

In 1964, when social engineering was in its prime, all the best people were positivists, or so a first-year graduate student in economics would naturally have believed. Among philosophers the doctrines of strict positivism were moribund. Philosophical positivism had had its day in the 1920s. One of the headings of Karl Popper's intellectual autobiography, *Unended Quest* (Popper 1974 [1976], pp. 87f) asks "Who Killed Logical Positivism?" He answers, "I fear that I must admit responsibility." I, said the Popper, / With my little chopper, / I killed logical positivism. His book of 1934, written when he was about thirty and translated into English twenty-five years later as *The Logic of Scientific Discovery* (Popper 1934 [1959]), was the knell. Popper quotes the Australian philosopher John Passmore as writing in 1967 that "Logical positivism, then, is dead, or as dead as a philosophical movement ever becomes" (Passmore 1967, p. 56). Even the broader doctrines of logical empiricism under which positivism sheltered had by 1964 been under attack for a long time. W. V. Quine's "Two Dogmas of Empiricism" had in 1951 undermined the distinction inherited from Kant between analytic and synthetic statements. Hilary Putnam dates the reign of positivism from 1930 to about 1960 (Putnam 1990, p. 105). Over in the philosophy department, then, no one earned prestige by declaring himself to be a positivist; not in 1964.

Over in the economics department, however, there was still prestige to be earned by sneering at the soft little qualitative people. No one in economics in 1964 had heard that positivism was dead. Positivism then was the philosophy of the hard-nosed against the soft-headed. The division of "soft" and "hard" was irresistible to a

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male 22-year-old in 1964. A new graduate student wanted to be hard, of course: that was why one studied economics rather than history or, perish the thought, English. Economists and other academics in the 1960s espoused a positivism notably cruder and more masculinist than the philosophical kind.

The crude version persists. An economist who uses “philosophical” as a cuss word (“That’s rather philosophical, don’t you think?”) and does not regard philosophical argument as relevant to his business will not watch what is going on in the department of philosophy. Even mature economists therefore do not have an occasion to rethink their youthful positivism. Economists young and old still use the positivist way of arguing, against the advice of economists who have paid attention to the history of philosophy since 1955 (Caldwell 1982; Caldwell 1984, 1987; Backhouse 1985, 1988, 1992a). The non-philosophical economists talk about hypotheses, verifiability, observable implications, meaningful statements, science vs. pseudo-science, the unity of sciences, the emulation of physics, the fact/value split, prediction and control, hypothetico-deductive systems, axiomatization, and the formalization of languages. Logical positivism had charmed the young men in philosophy during the 1920s and 1930s. Long dead, it charmed the graduate student in economics of 1964. In zombie form, dead from the neck up, it still charms young men in many fields (the young women find it less attractive).

An article by Milton Friedman in 1953, interpreted as positivism and using the word “positive” throughout, is supposed to be the Torah of economic Method. Daniel Hammond (1990) and Abraham Hirsch and Neil de Marchi (1990) argue persuasively what I from personal acquaintance have long believed – that Friedman is no dogmatic positivist, but a practical arguer, intent on finding scientific agreement. Indeed, Friedman (unlike George Stigler, for example) can be claimed as an early exponent of a pragmatic and rhetorical and (I don’t mind saying so) thoroughly American approach to economic discourse. He said recently that “the role of statistics is not to discover truth. The role of statistics is to resolve disagreements among people” (quoted in Hammond 1990, p. 167). In a letter of 1983 he said flatly that “logical positivism and the kind of positivism I favor are wholly different” (Friedman 1983, p. 3).

And yet positivistic sentences from Friedman’s pen, contradicting Friedman’s practice, provide the philosophy for economics. As the philosopher of economics Daniel Hausman observed, “Friedman’s advice has rarely been followed, and to implement it would require

that microeconomics be radically transformed. Yet, since Friedman presents his views in defense of theoretical ‘business-as-usual’ against critics of standard economics, one finds economists espousing Friedman’s methodology, who would never dream of seriously acting on it” (Hausman 1992, p. 275). And the Methodologists – economists and philosophers who say they are interested in how economists explain – go over and over the Friedman text, Mishnah-style, seldom getting beyond the terms defined at the height of positivism’s prestige. The history and appeal of positivism continues now to be hot news in economics, and was hotter in 1964.

The graduate student at the Harvard of 1964 was typical. I do not want to laugh too harshly at him: people forget that from Olympus we all look pretty funny. And I want to emphasize again that I do not regard positivism as a useless or silly movement. In its day it did good. In 1938 Terence Hutchison argued against the a priorism of the 1920s and 1930s; in 1953 Friedman argued against the refusal to examine facts of the 1940s and 1950s. But its day has passed; its values need scrutiny; it has become an oppressive rather than a liberating force in economics and has become (positively) Stalinist in econo-wannabe fields such as academic accounting and political science.

A young non-philosopher who declared himself to be a positivist in 1964 must be seen as declaring an allegiance vaguely understood. The young are good at vague allegiances (something to be borne in mind when teaching them) but are not so good at the details of doctrine. The young man was beginning in 1964 to stop thinking of himself as a socialist, yet even during his socialist phase had not read much of *Capital* (vol. I), or much else of the doctrine. On the positivist front he seems to have owned a copy of A. J. Ayer’s, ed. *Logical Positivism* (1959), but internal evidence suggests that he didn’t read it until later, and then never more than a couple of essays. (At the head of the essay by Otto Neurath the young man wrote in pencil: “This paper reeks of metaphysics,” which is either a complaint by a naïve positivist against what he imagined was backsliding or an observation by a sophisticated anti-positivist that logical positivism requires metaphysics to live; probably the former.) Like everyone else, he had been charmed by Friedman’s article. He thrilled especially to the part about leaves on trees not having to know that they “want” to face towards the sun, and wondered at Hendrick Houthaker’s diffident lecture on the matter to the first-year students of price theory at Harvard. A year or so into graduate school, following the economist John R. Meyer, his mentor and a leading young scholar in

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econometrics and transportation economics, he read some of R. B. Braithwaite's book (1953), and fancied himself advanced about hypothetico-deductive systems in science.

At about the same time, having decided to study economic history, he read Carl Hempel's "The Function of General Laws in History" (1942) and concluded that storytelling could be reduced to model testing. Believing that hypothetico-deductive testing of models covered what was of value in human thought, he tried to force his work on British economic history into the plan. In the margin next to Braithwaite's positivist sneer at "a policy of deep breathing followed by free association" (p. 272) he wrote "*verstehen*," which he understood without inquiry to be the method over in the department of history.

His grasp of the doctrines of the new religion, then, was weak in book learning. Yet one did not need book learning in the mid 1960s to be a thoroughgoing positivist. The intellectual world then was positivist. To take back the earlier point about philosophy being utterly dead in the departments of philosophy, even in philosophy a version of it reigned: "much of the philosophy that succeeded it in the Anglo-American world, though it would disclaim the name, has been and remains thoroughly Positivist in spirit" (Barrett 1979, p. 49). A sense in which the wider world remained positivist was soon to be demonstrated in the Vietnam War: here were social engineers, committed to the observable and the verifiable, armed with falsifiable if loony hypotheses deduced from higher-order propositions and a wide protective belt of rationalization when the dominos did not fall, unencumbered by the value half of the fact/value split, seeking body counts from the gunboats on the Mekong. Positivistic thinking, a popular version of philosophical positivism, pervaded intellectual life from economics to art (see again Klammer 1991).

Amateur positivism fitted the trend of Western philosophy, or at any rate the trend as discerned by the logical positivists themselves, who were the best of the philosophical crop between 1920 and 1940 and were the writers of the books that young men bought and admired. The young man had when in high school in the 1950s browsed on the non-technical works of Bertrand Russell. He picked up Russell's scornful attitude towards the intellectual past, a convenient scorn for a young person to have. Logical positivism was seen by Russell and his fans as a culmination: glorious if muddled Greek beginnings; Christian fall back; then the ascent to Descartes, Hume, Kant, and Bertrand Russell.

Positivism therefore appealed in 1964 to a young man's desire to

be up to date. And it was Scientific. A touching faith in what science could do seemed justified: Scientism. Science seemed then, as it still seems to people who do not know economic history, to have been the main engine of economic progress since 1700 (Rosenberg and Birdzell 1986 and Mokyr 1990 decisively overturn the notion). The history of science had not yet established that the rational reconstructions of which philosophers talked had nothing to do with how science worked (Farley and Geison 1974; Shapin and Schaffer 1985). The philosophers of science had not begun to use Thomas Kuhn (*The Structure of Scientific Revolutions* [1962], of course, but more particularly the case studies in his articles collected in 1977) against Popper (Feyerabend 1975 [1978]; Laudan 1977). A sociology of science that scrutinized laboratory life was still a decade away (Latour and Woolgar 1979; Mulkey 1979). Someone trying to become an economic Scientist around 1964 was going to latch on to a theory of how to be scientific. How do I know what Scientific Economics is? Positivism tells what, in this little book.

Being Scientific means in English being different from the rest of society. Demarcating Science from other thinking was the main project of the positivist movement. The project of demarcation, when you come to think about it, is strange. Perhaps the mixing of the positivistic program of demarcation with the English definition of Science explains why positivism of a sort has stuck to the English-speaking world. English-speaking people even now worry a good deal about whether they are scientific or not – witness the ignorant sneers which non-scientists such as mathematicians and journalists in English-speaking countries direct at social “science.” A graduate student in 1964 had a desire to be Scientific, in the English, honorific, lab-coated, hard-nosed, and masculine definition of the word (the desire of students has not changed, although then as now some of them have wise doubts: see Klamer and Colander 1990).

Importantly in 1964, as I have said, the economic scientists whom the graduate student took as models were positivists. I have mentioned the economist John Meyer, whose positivistic work with Alfred Conrad on the economics of slavery and on quantitative economic history had come out as papers a few years before. The graduate student had been a research assistant for Meyer, helping him put the papers with Conrad into *The Economics of Slavery and Other Studies in Econometric History* (Conrad and Meyer 1964), and later he worked on Meyer’s projects on the economics of commuter buses and on a simulation model of Colombian transport, meeting there the first-rate engineers, civil and economic, that Meyer had

gathered. All the economic historians and engineers he met were positivists, though all more sophisticated about it than the student. "Bliss was it in that dawn to be alive / But to be young [and positive] was very heaven!"

The student soon met his next hero, the economic historian Alexander Gerschenkron, who presided from Harvard over a piece of the new economic history. Gerschenkron, a learned and cagey teacher, a polyglot European and a fan of baseball, gave the young man another example of an admired scholar talking positivism (while doing something else; but the point here is the official doctrine, not the scientific behavior). Near the beginning of Gerschenkron's essay "Economic Backwardness in Historical Perspective" (1952 [1962a], p. 6, my italics) he declared that "historical research consists essentially in application to *empirical material* of various *sets of empirically derived hypothetical generalizations* and in *testing the closeness of the resulting fit*, in the hope that in this way certain *uniformities*, certain typical situations, and certain *typical relationships among individual factors* in these situations can be ascertained." *Scientor gloriosus*. The sentence has a whiff of Bacon in it, but could pass for the usual positivism of the chair. And elsewhere Gerschenkron said repeatedly that the concept of relative backwardness is "an operationally usable concept" (Gerschenkron 1962b, p. 354).

Avant-gardism, hero worship, being scientific, and joining in the ceremonies of scientism, then, partly explained the student's youthful positivism. Its univocal certitude was half the rest. The longshoreman and sage Eric Hoffer wrote in *The True Believer* that "The effectiveness of a doctrine does not come from its meaning but from its certitude. No doctrine however profound and sublime will be effective unless it is presented as the embodiment of the one and only truth" (Hoffer 1951 [1963], pp. 83f). William James had made a similar point: "I read in an old letter – from a gifted friend who died too young – these words: 'In everything, in science, art, morals and religion, there must be one system that is right and *every* other wrong.' How characteristic of the enthusiasm of a certain stage of youth!" (James 1907 [1949], p. 240). Our fate seems to be to lurch from one dogma to the next, from one univocal certitude to another, because the youngsters like it.

The remaining charm of the dogma was efficiency. Even to a graduate student with a feeble grasp on scholarship it was clear that positivism saved effort. Positivism was economical in ways attractive to the young and impatient. Here was a Method of being an

economic historian, for example, that required no tiresome involvement with “all the sources” (as students in the department of history put it so irritatingly). No. You needed merely to form an “observable implication” of your “higher-order hypothesis,” then proceed to “test” it. Most of the facts of the matter could be ignored, since most could be construed, if you were dull enough, as not bearing on the hypothesis under test. No tacit knowledge was necessary, no sense of the landscape, no feel for the story.

A young historian of the British steel industry 1870–1914 therefore did not have to learn about what was going on from 1870 to 1914 in the British steel industry, or anywhere else. (He did in fact learn more than was required on positivistic grounds, because he was thrown into a company of historians at the London School of Economics while doing his research, and anyway had a non-positivistic father, also an academic, who from time to time would remark mildly to his technocratic son that one needed to know something in order to write about it.) Nothing could be simpler than the positivistic formula. In fact, nothing was. The proliferation of normal if unpersuasive science in economics has shown how simple it is.

The simplicity of positivism has great appeal to the young. To put the point harshly, positivism is a 3" × 5"-card philosophy of science, which the young can read in a minute and understand in a day. Once it is understood they can apply it to everything. (For their simple-minded devotion to Method the young should be forgiven. They have few enough weapons against the old.)

Positivism from its beginnings tried to narrow the grounds on which scholars could converse down to the observable, to the numerical, to the non-tacit. Positivism commends intellectual narrowness. The word “positivism” was coined by Auguste Comte early in the nineteenth century, but by century’s end the successes of science appeared to warrant another and more rigorous neopositivism. The physicist Ernst Mach, for example, attacked in the 1890s the idea of the electron, calling it a non-observable figment. His slogan was “the observable.” A century later the economic slogans with which economists narrow their arguments are similarly unreasonable: “macroeconomics must always be re-expressed as microeconomics”; “no economics unless constrained maximization”; “ethical discussions are meaningless.” Positivism is one of the great sloganeering movements, substituting intellectual bumper stickers for thought.

It is like that with movements that young intellectuals find

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attractive, as Hoffer said. The German classicist Ulrich von Wilamowitz-Moellendorff wrote thus of his own youthful fascination with the Method of his age: "Philology had [in 1870] the highest opinion of itself, because it taught method, and was the only perfect way of teaching it. Method, *via ac ratio*, was the watchword. It seemed the magic art, which opened all closed doors; it was all important; knowledge was a secondary consideration." He remarked fifty years on, "Gradually the unity of science [*Wissenschaft*, "inquiry" in the German] has dawned on me . . . Let each do what he can, . . . and not despise what he himself cannot do" (Wilamowitz-Moellendorff 1928 [1930], p. 115; cf. 1927 [1982], p. 136; *via ac ratio* is "method and theory," literally "the way and the reason," as for example in Cicero, *De Oratore*, I, 205).

The third-year graduate student's attitude towards *via ac ratio* in 1966 is best illustrated by the Churchillian motto he and his colleagues affixed over the doorway of the Economic History Workshop, in the attic of a building just off Harvard Square: "Give us the data and we will finish the job." It seemed clever at the time. Economists need not be concerned with the mundanities of *collecting* the data. (For this reason the student spurned the chance to take courses from Simon Kuznets, the great historical economist who had just come to Harvard from Johns Hopkins. Graduate students, being graduate students, have defective intellectual taste.) And there was of course nothing beyond the easily quantifiable, observable implications to be known from a phenomenon.

By way of contrast consider the eminent biologist, Barbara McClintock, who approached nature with the idea, as Evelyn Fox Keller puts it in her account of McClintock's career, that

Organisms have a life and an order of their own that scientists can only begin to fathom . . . [McClintock said] "there's no such thing as a central dogma into which everything will fit." . . . The need to "listen to the material" follows from her sense of the order of things . . . [T]he complexity of nature exceeds our own imaginative possibilities . . . Her major criticism of contemporary research is based on what she sees as inadequate humility . . . [The usual] dichotomies of subject-object, mind-matter, feeling-reason, disorder-law . . . are directed towards a cosmic unity typically excluding or devouring one of the other pair.

Keller 1985, pp. 162–163

Perhaps positivism is a male method. The style of empirical inquiry that spends six years on the aberrant pigmentation of a few kernels of corn is rare in economics. Yet no one is surprised to find it

disproportionately among female economists: Margaret Reid of Iowa State and Chicago, for example, or Dorothy Brady of Pennsylvania and of the Women's Bureau at the Department of Labor, or Anna Jacobson Schwartz of New York University and the National Bureau of Economic Research; and latterly Francine Blau of Illinois or Claudia Goldin of Harvard. "The thing is dear to you for a period of time; you really [have] an affection for it," said McClintock (Keller 1985, p. 164). What is dear to male economists, by contrast, is the alleged model and the alleged test. "Testing hypotheses," after all, is easier than thinking; and it is a lot easier than making the phenomenon "dear to you for a period of time."

The men (I choose the word carefully) of the seventeenth century were the patriarchs of positivism (see Bordo 1987). The inventors of rationalism in the seventeenth century – Bacon, Descartes, Hobbes, Spinoza – had a paradoxically low opinion of the power of reasoning in human affairs. Ancient and medieval writers had more faith in the power of speech to move people towards the light. The men of the seventeenth century had seen words kill and sought therefore a way to disarm them. Their refuge was "crushing" proof and "compelling" demonstration, to "put Nature to the rack," as Bacon said so sweetly. They assigned everything else, as for example Hobbes did in his book on rhetoric, to mere ornament, suited only to arousing a feminine passion. We have inherited their low opinion of reasoning.

So the young man in 1967, aged twenty-five, now working on his dissertation, was a positivist. Einstein wrote to his friend Michele Angelo Besso about Mach's positivism: "I do not inveigh against Mach's little horse; but you know what I think about it. It cannot give birth to anything living; it can only exterminate harmful vermin" (May 13, 1917; Bernstein 1989, pp. 86f; later he called it "fairly dried-up petty-foggery" [quoted in Lakatos 1976, p. 93n]). That seems about right. Positivism was a reaction to German idealism. Harmful or not, idealism was exterminated in the English-speaking world for half a century. It is coming back as something more grown-up, as pragmatism or rhetoric or other projects after virtue, finding its reality in social discourse rather than in the transcendental spirit or in the datum seen clearly and distinctly by a lone observer.

In the meantime positivism *per se* did not give birth to anything living, at any rate in economics. Our theories of the economy are more numerous and interesting now than in 1955 or 1964 because more people have thought longer and more mathematically and more empirically about the economy, not because of positivism. They are