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Author(s): Deborah A. Redman

Source: *Journal of Economic Issues*, Vol. 28, No. 1 (Mar., 1994), pp. 67-99

Published by: Taylor & Francis, Ltd.

Stable URL: <http://www.jstor.org/stable/4226788>

Accessed: 27-06-2016 07:12 UTC

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Karl Popper's Theory of Science and Econometrics: The Rise and Decline of Social Engineering

Deborah A. Redman

Karl Popper is, in general, best known for his *Logik der Forschung*, translated into English in 1959 as *The Logic of Scientific Discovery* [1972b], an antipositivist work that, ironically, first appeared in 1934 as one of the Vienna Circle *Schriften*. It is there that he first developed his falsification thesis—that component of Popper's theory of science that has held sway over economists and social scientists in the second half of the twentieth century. Although Terence Hutchison first introduced economists to Karl Popper's ideas in 1938 in his *Significance and Basic Postulates of Economic Theory*, Popper seems to have been far more influential in econometrics than in any other field in economics. Popper gave the new field its animus; but, unlike developments in other fields, his ideas were integrated into economic theory *without* reference to him or his works. Consequently, Popper's influence on econometrics has gone virtually unnoticed. A similar fate has befallen the early econometricians' dreams (inspired by Popper's philosophy of science) of developing a tool of social engineering for the betterment of society.

In this essay, I will develop the thesis that Popper's theory of falsification has had a major impact on the early development of

The author is at the Fakultät für Wirtschaftswissenschaften, Tübingen University, Germany. This paper has been adapted from chapters four and eight of her "Economics and the Philosophy of Science" [1991] and is reproduced here in greatly revised form with the kind permission of Oxford University Press, Inc. An earlier version of this paper was delivered at St. Andrews University in 1992.

macroeconomics, that the long-term impact has, unfortunately, not been particularly productive, and that tracing this development helps to explain much of the current rumblings in both econometrics and macroeconomics. Because the economic literature has spawned a myriad of myths about Popper's theory of science, I will first review Popper's theories of natural and social science and explain why falsification fails in both branches of science before providing an account of the econometrics connection.

*Popper's Philosophy of Natural Science*¹

Popper's philosophy of science—his critical rationalism—is intertwined with an attempt to build a purely deductive approach to science, with his views on theory appraisal and the growth of knowledge, and with falsification and the demarcation criterion. At a time when Marx's philosophy of history, Freud's psychology, and Alfred Adler's individual psychology were in vogue, Popper was searching for the answer to such questions as: How can we decide if a theory is correct? How can we distinguish between scientific and unscientific theories? What gives scientific theories their validity? In addition, Popper was fighting against the totalitarianism and intellectual relativism of the war era. He was and remains firmly convinced that the critical method can make the use of violence obsolete [1976, 292].

Perhaps the most radical aspect of Popper's philosophy of science is his complete rejection of induction. "As for induction (or inductive logic, or inductive behavior, or learning by induction or by repetition or by 'instruction')," Popper tells us in his autobiography [1974a, 29], "I assert that there is no such thing."² Popper's is a purely deductive philosophy, resting on the belief that the prior probability of any law must equal zero. The intuitive proof of this runs as follows. Popper argued that no matter how often one encounters white swans and only white swans, the universal statement "All swans are white" can never be confirmed as true (as the logical positivists had believed), for the future could yield a black swan. Hence, according to Popper, the prior probability of "All swans are white," as well as all other generalizations, must be zero. Still, his position on induction must be regarded as an extreme one—surpassing even the views of Hume—that is not accepted by most philosophers [cf. Newton-Smith 1981, 49-52].

For Popper, the critical method is the method of trial and error: "The method of proposing bold hypotheses, and exposing them to the severest criticism, in order to detect where we have erred" [Popper 1974a, 68]. Because there is no induction, science starts with problems: "We never argue from facts to theories, unless by way of refutation or 'falsification'" [1974a, 68]. This method of trial-and-error elimination, the *modus tollens* to use the jargon of formal logic, takes on a dialectic form that in Popper's view embodies the process of science:

$$P_1 \rightarrow TT \rightarrow EE \rightarrow P_2.$$

According to Popper, "All scientific discussions start with a problem (P_1), to which we offer some sort of tentative solution—a *tentative theory* (TT); this theory is then criticized, in an attempt at *error elimination* (EE); and as in the case of dialectic, this process renews itself: the theory and its critical revision give rise to new *problems* (P_2)" [1974a, 105-6]. Popper later worried about this representation because he knew that all problems arise in a theoretical context. He thus acknowledged that science could start at any place, with TT or EE as well, although it normally starts with a "practical problem," even if the problem may just be "felt" [1974a, 105-6].

Whereas a universal statement cannot be confirmed, Popper recognized that it can be deduced as false. Consider, for instance, the observation "A black swan was seen at place x in Germany at time t." From this we can easily deduce that the universal statement "All swans are white" is false. Hence Popper's message in a nutshell was: "falsify theories, don't confirm them." Falsifiability required that an observation statement be so formulated that it could contradict a hypothesis, thus falsifying it.³ "Either the sun will or will not shine on August 15" is not falsifiable; neither are definitions and tautologies. Popper insists scientists should in fact propose bold theories, that is, theories that run a great risk of being false. "All swans are white" is thus preferred to "All swans are either white or grey."

It is a simple step to Popper's theories of the growth of knowledge and objectivity. An objective theory is one that "is arguable, which can be exposed to rational criticism, preferably a theory which can be tested; not one which merely appeals to our subjective intuitions" [1974a, 110]. The critical method also be-

comes the "instrument of growth" since "our knowledge grows through trial and error elimination" [1974a, 91]. If a theory withstands severe tests, it is said to have been "corroborated" or confirmed, not to be confused with the positivists' usage of confirmation meaning established as true.

Why Falsification Fails

There are numerous convincing reasons for falsification's failure. First, theories, unlike the single universal statement "All swans are white," are complex webs of assumptions, laws, and various restrictive conditions. Since the unit of appraisal is in practice not a simple statement, scientists cannot know which assumption of the theory is causing the problem; they can only conclude that at least one of the many assumptions is false. Hence, the theory can never be falsified conclusively. This difficulty is known to philosophers of science as the Duhem problem and was taken up in greater detail by Lakatos, one of Popper's most famous students [Lakatos 1978; Harding 1976].

Second, Popper fails to develop a fully noninductive schema; induction inevitably resurfaces. Popper does not believe we can ever know the truth; the goal of science is to obtain not the truth, but increasing verisimilitude, or increasing "truth" content. So how does one know whether one theory is better than another? Theory comparison depends on the "degree of corroboration" or how well a theory has stood up to severe tests [Popper 1974a, 82]. But if theory A has passed 100 severe tests, we infer from this it will pass more and is therefore reliable: induction. Popper realizes induction reappears, admitting "there may be a 'whiff' of inductivism here" [1974b, 1193], but is unwilling to modify his extreme position. Philosopher of science Newton-Smith [1981, 68] hits the mark with his analysis of Popper's reply: "On one meaning of the word 'whiff' a whiff is 'a kind of flatfish'," comments Newton-Smith, "and certainly this argument is kind of fishy. On another construal 'whiff' is a puff of air. But it is just false to say that there is a whiff of inductivism here—there is a full-blown storm."

Popper's treatment of ad hoc theories and the growth of science, then, also comes up against insurmountable difficulties. For Popper, a theory is ad hoc if it cannot be "independently tested"; he treats ad hoc modifications as a mere deflection of criticism. If a theory modification is independently testable, the theory's truth

content should increase because of the modification. But insistence on increasing truth content fails in a purely deductive system. Popper's method also rests on being able to establish that progress can be made—something that he cannot do without induction.

The third way in which falsification is inadequate is that it lacks a historical basis: the history of science indicates that the best theories would have been rejected if scientists had adhered to the principles of falsification. History also shows that the practice of science has not been one of rejecting theories when observation conflicts with theory.

A related matter and fourth point is that Popper overestimates scientists' willingness to attack their theories. As Lakatos [in Newton-Smith 1981, 52] once remarked, "You know a scientist who wants to falsify his theory?" Nonetheless, Popper did appreciate the fact that theories can be "immunized" from criticism; that is the point of his theory of ad hoc modification.

A fifth obstacle that thwarts falsification is the fallibility of observation statements (what Popper calls "basic statements"). Since it is to observation statements that we turn to falsify a theory conclusively, Popper's empirical basis for science must be absolute.⁴ But this is not the case, as Popper well knows. Popper [1972b, 11] discusses this problem with the use of a metaphor in a much cited passage of his *Logic of Scientific Discovery*:

The empirical basis of objective science has thus nothing "absolute" about it. Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above the swamp, but not down to any natural or "given" base; and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, at least for the time being.

His message, stated directly: observation statements are also unreliable. Problems with observation later became the theme of much of N. R. Hanson's work.⁵

In view of this complication, how, then, do scientists know when to reject a theory? Popper argued in *The Logic of Scientific Discovery* [1972b, sec. 30] that acceptance and rejection of basic statements ultimately rest upon a decision reached through a process resembling a trial by jury. Some philosophers of science

(for instance, Newton-Smith [1981, 64]) argue that with this admission Popper relegates science to the world of irrationalism. Although such an assertion is typical of contemporary philosophers of science, it rests upon a very narrow definition of rationality.⁶

Popper's Philosophy of Social Science

Unlike many philosophers of science who concern themselves exclusively with physics, Popper has aimed at influencing the social sciences. Whereas in the English-speaking world Popper is most often associated with his *Logic of Scientific Discovery*, in the German-speaking world Popper is regarded as one of the world's foremost philosophers of social science.

It is unfortunate that there seems to be no full treatment of Popper's philosophy of the social sciences or even of economics, his favorite social science.⁷ If we want to assess whether and to what extent economists have used and benefited from Popper's ideas, we must inevitably first understand Popper's philosophy. This rests on interpretation that is made especially difficult by the evolution and transformation of Popper's philosophy since the 1930s, which left behind confusing and conflicting streams of thought. Moreover, Popper never wrote a systematic treatise on the methodology of social science. Most of his works deal almost exclusively with natural science, where the focus is physics.

His philosophy of social science can only be understood within a framework of his philosophy of natural science. And there are clear ties between the two. For instance, Popper advocated a unity of method among all disciplines and a naturalistic view that rejects any telling differences between the social and natural sciences. Moreover, much of Popper's philosophy is prohibitive, one example being that the goal of science is error elimination, while the goal of society is the diminishment of suffering, Popper's "negative utilitarianism." But whereas the scientific method of the natural sciences is "revolutionary" because criticism induces major changes, social scientists are advised to recommend small adjustments to social policy, his "piecemeal engineering."

In the *Poverty of Historicism*, Popper [1960, 2] advanced the proposition that the social sciences had somehow fallen behind the natural sciences and therefore should be characterized as "the less successful sciences," concluding that "the social sciences do not as yet seem to have found their Galileo" [1960, 1].⁸ Elsewhere Popper

[1970, 57-8] attacked sociology and psychology as "riddled with fashions, and with uncontrolled dogmas." Yet to Popper [1960, 60, n. 1], the one exception among the social sciences was economics, for "it must be admitted, however, that the success of mathematical economics shows that one social science at least has gone through its Newtonian revolution."

Why was Popper so content with economics? He adopted a pronaturalistic stance in *The Poverty of Historicism*, which meant he "favour[ed] the application of the methods of physics to the social sciences" [1960, 2].⁹ At that time, Popper probably believed that economics was fulfilling this prescription; this may be explained by his familiarity with the ideas of Marschak, who, as we shall see later, was very optimistic about using statistics in economics. Moreover, numerous historians of economic thought have noted that neoclassical economics has been built upon nineteenth-century physics.¹⁰ But Popper later reversed his views on naturalism in his *Postscript to The Logic of Scientific Discovery* [1983, 7], where he wrote:

I dislike the attempt, made in fields outside the physical sciences, to ape the physical sciences, by practising their alleged "methods"—measurement and "induction from observation." The doctrine that there is as much science in a subject as there is mathematics in it, or as much as there is measurement or "precision" in it, rests upon a complete misunderstanding. On the contrary, the following maxim holds for all sciences: *Never aim at more precision than is required by the problem at hand* [emphasis added].¹¹

Still, Popper has never revised his views on economics.¹² In the *Poverty of Historicism*, he had insisted economics, too, should have laws, e.g., "You cannot introduce agricultural tariffs and at the same time reduce the cost of living" [1960, 62]. But Popper was realistic enough to perceive that difficulties were greater in the social sciences: "Trends exist, or more precisely, the assumption of trends is a useful statistical device. *But trends are not laws*. A statement asserting the existence of a trend is existential, not universal" [1960, 115]. Popper thus properly assumed that reliable (for Popper, scientific) predictions are based on laws and cannot be based on the existence of trends. He went on to say that the confusion of laws with trends inspired the false doctrine of historicism. In spite of this, he footnoted the view that "it is [still] possible to formulate a law corresponding to the trend" [1960,

160], thus hopelessly muddling an important methodological distinction.

Besides the problem with trends, Popper [1960, 142-43] aptly described a second drawback in the social sciences:

But it cannot be doubted that there are some fundamental difficulties here. In physics, for example, the parameters of our equations can, in principle, be reduced to a small number of natural constants—a reduction which has been successfully carried out in many important cases. This is not so in economics; here the parameters are themselves in the most important cases quickly changing variables. *This clearly reduces the significance, interpretability, and testability of our measurements* [emphasis added].

Popper nevertheless admitted to having no knowledge of the social sciences when he wrote *The Logic of Scientific Discovery* in the early 1930s and *The Poverty of Historicism* in the mid-1930s.¹³ Given his confessed ignorance of economics and the youthful state of the social sciences, his misgivings about extending the method of the natural sciences to the social sciences should, perhaps, have been taken more seriously by economists.

In both natural and social science, Popper urged scientists to formulate "practical technological rules stating *what we cannot do*" [1972a, 343]—in other words, hypotheses should be formulated in order to be falsifiable. Thus, Popper also anchored his philosophy of social sciences in falsification and reduced social theories to universal statements. This, he thought, would enable him to attack Marxism, psychologism, and other theories that he believed to be of dubious scientific validity.

His philosophy of social science also includes a polemic against historicism, already touched upon, and his theory of situational logic. According to Popper, historicism, a false method, is the reason for the lack of progress among the other social sciences. Historicism [Popper 1960, 3] is defined as "an approach to the social sciences which assumes that *historical* prediction is their principle aim, and which assumes that this aim is attainable by discovering the 'rhythms' or the 'patterns', the 'laws' or the 'trends' that underlie the evolution of history." It is a *Kampfbegriff* befitting a polemic [Lee and Beck 1953-54, 575] and a term close to the negative meaning of the German term *Historismus*, indicating the abandonment of theory, particularly in economics and law [Iggers 1973, 457]. As with critical rationalism, Popper conflates too many

views: historicism, for example, is equated with holism, relativism, and large-scale prophesizing, all of which he attacked and tried to expose as dangerous components of spurious philosophies such as Marxism, Freudianism, and other "sciences" then fashionable.¹⁴ As Passmore [1974, 47] has observed, "Popper's work is an object lesson in the way in which the use of a label can obfuscate discussion." It did in fact tend to support a message that was not Popper's: that there is no place for history in any science.

The theory of situational logic was also first developed in the *Poverty of Historicism*. Another concept representing too many things to be clear, situational logic is a statistical generalization, an ideal law, a mathematical method, a rational reconstruction, and the assumption that humans act rationally. It is false but objective and not psychological. The most recent discussion of it appears in his autobiography [1974a, 93-94], where Popper underscores the importance of building models that can serve as testable hypotheses:

The method of situational analysis . . . was developed from what I had previously called the "zero method." The main point here was an attempt to *generalise the method of economic theory (marginal utility theory) so as to become applicable to other theoretical social sciences*. In my later formulations, this method consists of constructing *a model of the situation*, including especially the institutional situation, in which an agent is acting, in such a manner as to explain the rationality (the zero character) of his action. Such models, then, are the testable hypotheses of the social science; and those models are "singular," more especially, are the (in principle testable) singular hypotheses of history.

By zero method, Popper says that he means "constructing a model on the assumption of complete rationality (and perhaps on the assumption of the possession of complete information)" [1960, 141] and refers the reader to Jacob Marschak's work.

Much of what Popper said about situational logic, although not particularly unambiguous, seems to boil down to supporting the use of statistics, in particular, the testing of a statistical hypothesis against an alternative hypothesis, with the parallel to his falsification thesis being the test and possible rejection of the null hypothesis. Upon closer examination, the "Newtonian revolu-

tion" that Popper had claimed for mathematical economics is merely the adoption of mathematical and statistical techniques.

Popper's theory of situational analysis is only developed sketchily. We can say, nonetheless, that it allies itself with rational behavior and provides economists with the following advice: "Try to explain all actions and beliefs in terms of situational analysis and the Rationality Principle. If a given action or belief appears to be irrational always blame your model of the agent's situation, *not* the Rationality Principle" [Koertge 1975, 457].¹⁵ But Popper did not leave it at that: he stirred great controversy by tying the rationality principle to the curious view that the social world is less complex than the natural world [1960, 140-41]. The belief that the social scientist starts out at an *advantage* over the natural scientist because of the element of rationality in human behavior is actually a typical Austrian view not peculiar to Popper, although similar excess claims for economics were also made by several British classical economists after Adam Smith,¹⁶ who, incidentally, upheld the converse. This position led the Frankfurt School (of neo-Marxists) to attack Popper for being a positivist.¹⁷

In brief, Popper's philosophy of social science is incomplete, inconsistent, and somewhat disjointed. Nonetheless, we can conclude that Popper's situational logic and anti-historicism were meant to work together in an attempt to put economics on sound foundations. In so doing, however, Popper made assertions that are too demanding. Not only did Popper argue that the social world is less complex than the natural, he also implied that laws essentially fall into two groups, black and white in nature: reliable laws like Newton's and unreliable, even dangerous, ones like historical or psychological laws. Finally, he grounded economics in a "purely objective method," that is, in a "logic" and in the testability of hypotheses. This once again puts the falsification thesis at the heart of objectivity and theory appraisal.

Falsification and Economics

Most economists have heard of Popper's theory of falsification, although they have no personal familiarity with his works, and most do believe that economic theories can be falsified in the sense of being refuted.¹⁸ Boland [1977, 104] even relates the anecdote that Popper is taken so seriously that "theory papers [are] being rejected by editors because they [the authors] did not show that

they contributed to an *increase* in the testability of standard demand theory (since that supposedly is the primary criterion of progress—increased testability)."

We have seen that falsification fails in the natural sciences—for reasons equally compelling in the world of economics. To the list of problems already mentioned we can add Boland [1977, 93] and Papandreou's [1958] argument that the refutation of a specific model of a theory does not necessarily refute the theory represented by the model. Caldwell [1984] offers several more reasons for falsification's failure in economics: problematic assumptions, initial conditions that are not testable, a thornier empirical basis than in physics, and other restrictive conditions. Hausman's discussion [1988] includes additional reasons. In spite of these drawbacks, we still find good economists insisting on falsification in economics. Even an economic luminary such as Mark Blaug [1980], who surveys the philosophy of science in the first part of his book *The Methodology of Economics* and finds that falsification in the natural sciences is impossible, continues to insist on falsification in economics in a later section. And this is by no means an isolated case.

Why does such contradiction occur? Besides the fact that Popper's discussion of falsification has been inconsistent and confusing, economists, like many philosophers of science, are unwilling to draw Popper's philosophy to its logical consequences, sometimes because they share his political outlook. Another reason is economists' tendency to hang on the belief that empirical evidence is the *absolute* arbiter of truth in economics—a view they think is anchored in Popper's falsification thesis. This brings us to the econometrics connection.

*The Impact on Econometrics*¹⁹

Lagging, but paralleling, philosophers' awareness that Popper's theory of falsification had floundered, a "certain disenchantment with econometrics" [Stewart 1979, 209] became noticeable, gaining momentum in the 1980s. The cause for the disenchantment was a growing awareness of the flimsy statistical basis of econometrics. The movement was initiated with Hendry's article "Econometrics—Alchemy or Science?"²⁰ in 1980 in which Hendry showed that, when attempting to explain inflation in the United Kingdom, he got a good fit by using "cumulative rainfall in

the UK" as the explaining variable. Hendry [p. 395] concludes bluntly: "It is meaningless to talk about 'confirming' theories when spurious results are so easily obtained."²¹ Two years later, in what was to become a highly controversial article, Leamer [1983, 36] implored economists to "take the con out of econometrics." His point is that economists have inherited a false sense of objectivity from natural scientists. Philosopher of science Clark Glymour [1985, 290] reviewed Leamer's article, finding much objectionable from a philosopher's point of view, but confessing that the substance of the paper is very important. And the substance, put briefly, is that "statistical tests don't inform us as to whether or not a model is *approximately* true. They don't permit us to compare false models to determine which is closer to the truth" [1985, 293]. In other words, statistical tests cannot play a definitive role in theory appraisal. The controversy continues to rage on the pages of economics journals.²²

A careful reading of Popper's theory of science shows that one *compelling* falsifying instance refutes a theory.²³ In contrast with today's dogma, pioneer econometricians knew that this is clearly not the way that econometrics works or can ever be expected to work: most economists of all eras would agree that observations can be found to contradict all theories and economic "truths." In its formative years, while econometrics was still struggling to gain acceptance, the pioneers of econometrics were motivated by the conviction that false hypotheses could nonetheless be refuted decisively; this underscored the purpose of econometrics as a reliable tool for forecasting. Trygve Haavelmo's (1911-) approach, which formulated theories as probabilistic statements, offered a solution that would become most influential: once hypotheses were tested against data, true theories would be accepted and false theories rejected most of the time—an approximation to Popper's falsification.²⁴ That this was a view quite compatible with that of philosophers of science such as Popper was no accident, for his ideas influenced the early formation of econometrics and vice versa: Popper knew Jacob Marschak (1898-1977) and discussed his work and the development of econometrics with him. According to Popper [1992], Marschak was fully acquainted with his work and interested in "a development of econometrics which would lead to formulating falsifiable predictions that could perhaps in their turn lead further to falsifiable general theories."²⁵

Marschak is indeed a key figure, for it is due to his efforts that Popper's views on falsification became crystallized in the "Cowles Commission method." Thus, one chapter of the history of falsification in economics is the history of the Cowles Commission of the 1940s, whose original staff developed the theoretical core of econometrics.²⁶ Assuming Haavelmo's probability approach, the Cowles Commission staff built on the work of Jan Tinbergen (1903-), who in the 1930s had developed the first econometric model of the entire economy with two basic goals in mind: to show how a macroeconomic model could be constructed and used for simulation and policy and to test statistically theories of the business cycle.²⁷ Once the economy was conceived as a system of equations that could be altered "so as to drive the endogenous variables along any desired path" [Epstein 1987, 62], the idea of controlling the economy became a mere matter of course. The Cowles Commission staff was certain that once Tinbergen's approach was reformulated to avoid simultaneity bias and the identification problem, the control of business cycles would be firmly within grasp. The political environment favored this development since, by the late 1940s, most Western governments had committed themselves to intervention in the economy to prevent high unemployment or instability. This, combined with the U.S. Full Employment Act of 1946 and the establishment of an American presidential "Council of Economic Advisors," suddenly resulted in econometricians finding themselves in great demand.

Not only Marschak absorbed Popper's view that objective, politically neutral research in natural science consists in falsifying hypotheses. Marschak's efforts were equalled by those of his good friend and life associate, Tjalling Koopmans, who had first met Marschak at Oxford in 1938 and followed him to the Cowles Commission in July 1944.²⁸ Without mentioning Popper's name or works, Marschak and Koopmans imbued econometric theory with the ideas of *The Logic of Scientific Discovery* and the *Poverty of Historicism*, making them the credo and guiding force of the new field. Koopmans's 1941 article, "The Logic of Econometric Business-Cycle Research," echoes the title of Popper's magnum opus and defends the benefits of statistical induction for economics.²⁹ At the same time, Marschak [1941, 448] was writing: "I hope we can become 'social engineers'. . . I don't believe we are much good as prophets," rephrasing Popper in his *Poverty* without the nicety of formal acknowledgement of the source of inspiration.³⁰ Amaz-

ing though it may seem, the conspicuous Popperian tenor of Koopmans's work must be attributable solely to his close collaboration with Marschak, since he was ignorant of Popper's philosophy of science at the time.³¹ The influence of both men in the making of econometrics should not be underestimated: "Marschak and Koopmans became the proselytizers for the econometrics 'movement' in journal articles and professional meetings" [Epstein 1987, 65].

By "social engineering," Marschak had in mind consultation to firms and government on a grand scale. But, as Epstein [1987, 61-62] notes, the phrase "social engineering" was soon changed to "economic policy" to avoid all associations with central planning. Marschak conceived of the policy problem in two stages: first, the estimation of equations and, then, adjustment in light of the social welfare function to obtain the social optimum. The original research staff at the Cowles Commission believed that their method would make social engineering possible and would yield concrete solutions to economic and social problems.

The Cowles Commission method entailed the specification of a model as a set of identified structural equations with an assumed stochastic distribution of the error term [see Epstein 1987, 60ff.]. The Cowles group understood econometrics as the statistical study of the interaction of rational decision makers. Behavioral laws were determined that could be represented by structural equations. The equations of the model were designed to describe the plausible behavior of economic agents by making full use of *a priori* knowledge. The outcome of the interactions became known as the "reduced form." For Marschak, simultaneous equations estimation was "the 'rational empirical' approach: the only possible way of using past experience for current rational action (policy as distinct from passive prediction)" [Epstein 1987, 69].

The Cowles Commission method was grounded in the conviction that the critical testing of economic hypotheses would be the foundation on which economics as a science would be built. Structural estimation was adopted for the very purpose of providing a decisive means of testing multiple or competing hypotheses. Marschak always assumed that only one—or perhaps even none—of the hypotheses to be tested was really true. Economic theories (or models or "laws") that passed statistical tests devised by econometricians would, then, form a class of potentially "true" theories; failures would be catalogued before being dismissed.

Pioneer econometricians believed this process would benefit them as experiments do natural scientists, a view that soon became an unassailable component of the more general econometrics ethos. Consider also the main objective of the Econometric Society, founded in 1930 by Tinbergen, Ragnar Frisch (1895-1973), and Irving Fisher (1867-1947): "to promote studies that aim at a unification of the theoretical-quantitative approach and the empirical-quantitative approach to economic problems and that are penetrated by constructive and rigorous thinking similar to that which has come to dominate in the natural sciences" [Constitution of the Econometric Society 1933, 106].³²

The idea (running from the logical positivists to Popper) that theory assessment occurs when a theory is confronted with data was completely absorbed into the econometric program. In fact, the pioneers of econometrics were hell-bent on finding a method that would allow theories to be classified as consistent or inconsistent with the data. This would, as Lawrence Klein [1947, 111] makes clear in the passage below, not only enable their work to be precise, but also to rule out partisan influences.

It is desirable to provide tools of analysis suited for public economic policy that are, as much as possible, independent of the personal judgments of a particular investigator. Econometric models are put forth in this scientific spirit, because these models, if fully developed and properly used, eventually should lead all investigators to the same conclusions, independent of their personal whims.

The key figures shaping econometrics—Jacob Marschak, Joseph Schumpeter (1883-1950), and Tjalling Koopmans, among others—erected the new field with the spirit that science would guarantee a fair, objective, democratic world. "For the immigrants who lived through the interwar period in Europe—and some, like Marschak, had fled first Lenin and then Hitler," explains Leijonhufvud [Craver and Leijonhufvud 1987, 181], "this hope of building a *wertfrei* social science, immune to propaganda of every kind, gave motivating force to the econometric movement."

We see, then, that a chief distinguishing feature of the fledgling macroeconometrics movement, especially as espoused by the Cowles Commission, was its optimistic ambition. "One has to be impressed by the enormous pioneer confidence displayed by the Cowles researchers in these early years," writes Epstein [1987, 70], who admits that he was moved by the "idealism and aspira-

tions, as well as the naiveté, of the early workers in this field" [1987, 2-3]. Marschak's confidence went so far that he sometimes referred to his research as the "gospel" [Epstein 1987, 61]. It was an optimism "based on an extraordinary faith in quantitative techniques and the belief that econometrics bore the hallmarks of a genuinely scientific form of applied economics" [Morgan 1990, 1], an optimism rooted in the belief that statistical methods were the counterpart to the experimental method of the natural sciences.

Indeed they were zealous but not Pollyannish; they possessed ambition but also humility. The Cowles Commission method met with strong opposition, even from some members of the Econometrics Society and the editorial board of *Econometrica*,³³ the NBER (using Mitchell and Kuznet's approach), and numerous statisticians.³⁴ That Keynes had balked at Tinbergen's first macro-econometric models, labelling them "statistical alchemy," is general knowledge [Keynes 1940, 156].³⁵ But two of the most damaging criticisms came from Milton Friedman (1912-), who attended many of the Cowles Commission seminars from 1946-1948. Both dealt with the problem of multiple hypotheses, now known as the problem of model selection. Friedman argued that the problem of competing hypotheses made a structural estimation approach unsuitable for empirical research. He expressed deep reservations about using estimation procedure to discriminate between competing theories or hypotheses that relied on the same data rather than on new observations as does experimental procedure in physics. The problem, recognized by the Cowles staff, was that the data were supporting too many structures that were equally plausible a priori. In other words, false models would receive some support from the data—or more disturbing, perhaps all models were "true."³⁶

Worse, Friedman [1953, 12, n. 11] never tired of pointing out that hypothesis selection "may suit the psychological needs of particular investigators," i.e., estimation results simply reflect the prejudices of the investigators, the second objection.³⁷ Marschak kept hoping that more information would reveal the true hypothesis. But it is easy to see how effectively Friedman's criticisms cut to the methodological quick of the Cowles Commission method.³⁸

Not surprisingly, the Cowles Commission method did not meet expectations in the long run because the problem of weeding out competing hypotheses was never solved.³⁹ Indeed, "the model

selection problem is as pressing now as two generations ago but has tended to be suppressed in published reports of empirical investigations" [Epstein 1987, 4]. As the 1940s came to an end, the Cowles staff had already faced the fact that their attempts to control business cycles had not yielded the desired results.⁴⁰ As they recognized this, their interests slowly turned from econometrics to mathematical economics. After 1947, and with Koopmans directing research, the Cowles Commission did little statistical inference and almost no hypothesis testing [Epstein 1987, 113]. But rather than abandon their high-flying ideals, they channeled their energy into activity analysis with the hope that it might steer clear of the problems with multiple hypotheses and aggregation. Not unexpected, econometrics in the 1950s and 1960s downplayed the critical testing of theories.

Epstein [1987, 127ff.] relates that by the 1960s, the Nobel prize laureates had also reassessed the promise of the new field, emerging on balance with a pessimistic outlook. In 1961, Ragnar Frisch, disappointed with its failure to capture institutional and political constraints on policy, no longer mentioned econometrics as a forecasting method. Haavelmo had already voiced his reservations about the value of econometric models for policy purposes. By the time Tinbergen was awarded the Nobel prize in 1969, his interests had shifted to problems of development, and he had reached the conclusion that the chief policy problem in economics was to devise an appropriate *institutional arrangement* for an economy⁴¹—a task unrelated to econometrics and a full about-face in the direction of the American institutionalists and the German *Ordnungspolitik* tradition [Epstein 1987, 155].⁴² In his Nobel lecture, Tinbergen warned that model building had become a fashion that today's economist sometimes overdoes. By 1987, Tinbergen [p. 136], one of the few remaining major pioneer econometricians alive, expressed a concern that "so much refinement of methods of testing is perhaps not necessary. . . . I've a vague feeling that I would have liked somewhat more applications and somewhat less pure theory."

These concerns have had little effect on mainstream econometrics, although work in structural estimation waned (to be taken up again in the 1970s). The early postwar period provided a fertile environment in the United States for the development of other strains of econometrics. As Klein [in Pesaran 1987, 13] once remarked, "The Keynesian theory was simply 'asking' to be cast in

an empirical mold." Progress in computer technology, a U.S. government with the Democrats at the helm, and Klein's first macroeconomic model in the tradition of the Cowles Commission were all significant catalysts in the postwar era; the Brookings model, the first monetarist model at the St. Louis Federal Reserve Bank, and the proliferation of econometrics journals, to mention a few developments, followed—more grist for the mill.

Whereas the pioneer econometricians had accepted and conceded their failures, and, being guided by scientific honesty, gave up on structural estimation as a means of providing an absolute, objective testing apparatus, the second generation ignored most of the negative results reached by the pioneers.⁴³

Many of these later workers did not share the Cowles Commission's emphasis on exposing models to critical statistical tests to the greatest possible extent. As a result, they tended to foster an unfortunate illusion of empirical knowledge, the extent of which was never fully determined when the large macro models were jolted by the events of the 1970s. Even where the best statistical practices have been followed, however, it is argued that the present state of the science would still support only very modest claims for the stock of empirical results they have so far produced [Epstein 1987, 3-4].

Consequently, the falsification legacy was not allowed to run its course. The U.S. economy was relatively stable in the 1950s and 1960s: the macroeconomic models worked, and that was what counted.⁴⁴ Econometrics suddenly found itself in its heyday. Little of that unflagging quest to find an objective, completely reliable method directed the work of the second generation. Instead, as econometricians no longer felt the need to justify their work, the overly bold and zealous element of the pioneer program hardened into dogma; external criticism dissipated and with it the critical attitude that had so distinguished the Cowles Commission of the Marschak-Koopmans era.⁴⁵ What was once youthful, naive enthusiasm about what econometrics could achieve had, by the 1970s, turned into an inhibiting hubris. The path was laid for the reservations raised by the first generation, and subsequently neglected, to return to haunt us.

That catapults us into contemporary debates in macroeconomics and econometrics. The crisis became acute when, in the aftermath of the supply shock and currency instabilities of the

1970s, macroeconometric models continued to fail to predict even after repeated respecification. The imbroglio also revolves around the fact that investigators have not, to paraphrase Klein, been led to the same conclusions, a particularly grave problem for macroeconomics. All this forced a return to a focus on model selection/evaluation and on testing.

By the 1970s and 1980s, the legacy of falsification in economics had come full circle. In an article pointedly titled "The Poverty of Economics," one economist [Kuttner 1985, 78] sums up the current state of econometrics and its methods with the following message:

By manipulating time lags the determined econometrician can "prove" almost anything. Moreover, though many economists argue that the fair way to test a theory is to specify the hypothesis, and run the regression equations once, it is common practice to keep fiddling with the equations, manipulating lag times, lead times, and other variables, until the equations more or less confirm the hypothesis.⁴⁶

This was equalled by the discernment of Erich Streissler [1970, 53], a Viennese econometrician, who criticizes modern econometric methods by citing Popper's warnings against historicism (which, of course, were never meant to be turned upon economics, the discipline that had, in Popper's words, undergone a Newtonian revolution):

It is a commonplace of long standing that exact forecasts are impossible in economics. This has been most forcefully stressed by Sir Karl Popper. He once said: "Long-term prophecies can be derived from scientific conditional predictions only if they apply to systems which can be described as well-isolated, stationary and recurrent. These systems are very rare in nature; and modern society is surely not one of them." Strictly speaking [Streissler continues] the same impossibility theorem is true for short-term economic forecasts as well. It may sometimes be appropriate to refer to Popper's stern measuring rod and denounce the machinations of naive—or even dishonest—forecasters as an intellectual sham.⁴⁷

The attempt to capture objectivity and scientific creativity in rules—the naive quality of both Popper's early philosophy and

some strains of econometrics up to the present—was originally motivated by an oppressive political climate and an unswaying will to find an objective basis for practicing science.⁴⁸ The present confusion in economics reflects a transition phase;⁴⁹ change is being hampered by a fear that an admission of falsification's failure will jeopardize theories' objectivity and scientific validity. But this holding onto the overly confident components of Popper's theory and econometric testing doctrines amounts to nothing more than wishful thinking and self-deception, as the German economist-philosopher Hans Albert, a friend of Popper's, recognizes: "All epistemological certainty is self-fabricated and hence worthless for grasping reality" [1980, 30].⁵⁰

In short, understanding Popper and his legacy entails grasping the fact that he had one foot in contemporary analytic philosophy and another in the positivist tradition that was anchored in epistemological certainty and an overly zealous faith in the new logic and mathematics as a means of ensuring this infallibility. There is, after all, no tried and sure rule for determining whether a theory is good or bad, no methodology that guarantees success. Despite the contradictions in his work, Popper knows this. In the 1920s, J. M. Keynes (1883-1946) wrote: "The Theory of Economics does not furnish a body of settled conclusions immediately applicable to policy" [Hicks 1983, 375].⁵¹ In a similar way, methodology does not furnish a body of settled rules for the successful appraisal of theories. No philosophy of science can alter this because economists' professionalism, credibility, and objectivity rest upon exercising sound judgments, not in adhering to prespecified rules. As disappointingly prosaic as this lesson from the philosophy of science may be, there are no shortcuts to knowledge and no rules that can guarantee objectivity or creativity.

Whither Popperian Philosophy and Econometric Testing?

In spite of the negative results reached here, the failure of Popper's falsification theory and of the early Cowles Commission testing goals is no reason to reject his whole philosophy or, for that matter, modern econometrics. Whereas today we know the absolute quality of test results associated with falsification, and the Cowles Commission method with it, is deceptive, the emphasis on testing and the striving to be nonpartisan have certainly had a beneficial influence. Furthermore, Popper conflated falsification

with many other elements of his philosophy, some of which have not succumbed to criticism. For instance, Popper always insisted that, in all sciences, objectivity can be obtained by mutual criticism. So long as promoting criticism is an attitude and is not equated with logical contradiction, Popper's critical rationalism does not reduce to falsification and rests on safe ground. Critical rationalism so formulated was developed and refined by Popper's former student William Bartley.⁵² Seen from this perspective, theories evolve by being subjected to criticism, revision, further criticism, etc. We have mentioned that with the second generation of econometricians this critical stance lost priority. Equally important, Popper developed falsification to enhance clarity: clearly, if a hypothesis, idea, or theory is stated unambiguously, it is easier to criticize.

Much also speaks for taking more seriously Popper's emphasis on learning from failure: econometricians and theoreticians almost never publish plausible attempts that have failed. Perhaps in the future, journal editors will give more consideration to unexpected failures—hardly a novel request, for in response to the Cowles Commission method, Milton Friedman had urged for the publication of more information on methods and models that were deemed *unsatisfactory* [Epstein 1987, 107]. Koopmans is also on record for having bemoaned a lack of documentation of econometric failures [Epstein 1987, 54]. Doubtless, reasons why a model is rejected or accepted, why certain variables are included in the model, and how the researcher comes to these conclusions are important because they are an act of judgment.⁵³ It seems what Friedman and others were getting at was a plea for a catalogue or written history of testing results—a kind of running track record on each theory or hypothesis. This project subsequently fell into oblivion, perhaps explaining the desultory nature of contemporary theory testing.

I think several morals can be gleaned from the demise of falsification and its influence on economics. First, economists would do well to treat philosophers' theories of science with skepticism before applying them to economics. Second, the neglect of weighty methodological problems has a boomerang effect; it is only a question of time until they reappear. Finally, the growth of science can be fostered not only by pushing out the frontiers of knowledge, but also by making formal acknowledgement of the limitations of our knowledge, by placing a greater stress on methodological modesty,

and by coming to terms with the fact that a decisive apparatus for theory testing in science does not and cannot exist. For, as Lawrence Klein admitted in 1947 [p. 138], "It is, of course, important to know what we cannot do in order that we do not fool ourselves." Once economists abandon absolute standards, the focus on developing a sense of good judgment and producing fallible but, to the best of our knowledge, reliable results can once again rise to prominence.⁵⁴

Notes

1. By virtue of space limitations, the sketches of Popper's theory of natural and social science are very simplified, providing only a skeleton of his views. For those who would appreciate a more solid frame, I suggest they consult Redman [1991], Blaug [1980], Caldwell [1982], Hausman [1988; 1992], and, of course, the primary sources.
2. Most economists and social scientists have difficulties swallowing this; nonetheless, a rejection of induction is *the* distinguishing feature of the Popper school of philosophy.
3. I refer to Popper's views on falsification in the past tense because he has abandoned work in this area.
4. Both Popper and the logical positivists assumed theory assessment occurs when a theory or hypothesis is confronted with data.
5. Hanson developed the notion of the theory-ladenness of observation. See Hanson [1958].
6. As noted, Popper has abandoned work on falsification. Some of his most recent work deals with evolutionary approaches to science and with Darwinism, e.g., Popper [1987], a startling change of direction since until recently Popper condemned Darwin on grounds that the theory of natural selection is metaphysical due to its tautological nature, which in Popper's system renders it incapable of being falsified. Popper [1987, 144] now admits he was in error. It is noteworthy that what he once called the critical or Socratic method he is now calling the "method of critical selection" [1987, 146].

7. Popper [1974a, 96] tells us in his autobiography: "In fact, the only theoretical social science which appealed to me was economics." In *The Poverty of Historicism* alone, Popper refers to five economists, the first two pioneers of econometrics: Jacob Marschak (1898-1977), Ragnar Frisch (1895-1973), F. A. von Hayek (1899-1992), Carl Menger (1840-1921), and a lesser known British applied economist who took an interdisciplinary approach to economics, Philip Sargent Florence (1890-1982).
8. In 1846, Pierre Proudhon wrote a treatise called *The Philosophy of Poverty*. Karl Marx responded with *The Poverty of Philosophy* a year later, which Popper in turn targeted with his *The Poverty of Historicism*.
9. This view that other sciences should use the methods of the natural sciences (especially of physics) has been criticized for being scientism by Hayek.
10. The most extreme advocate of this position is Mirowski [1984, 377], who argues that "neoclassical theory is bowdlerised nineteenth century physics."
11. No doubt we get two such contrary views from Popper because he was reacting to the trends in the science first of the 1920-1930s and then of the 1970-1980s.
12. Popper [1974a, 91] did, however, later deny any great significance to *The Poverty of Historicism* in his autobiography.
13. Popper [1960, 137-38] explains: "I have every reason to believe that my interpretation of the methods of science was not influenced by any knowledge of the methods of the social sciences; for when I developed it first, I had only the natural sciences in mind, and I knew next to nothing about the social sciences."
14. It seems to me that the flavor of Popper's philosophy has parallels in cowboy movies that pit the good guys in white against the bad guys in black. The chief difference lies in appearance: Popper's intellectual costumery is so impressive that even the most perceptive reader may sometimes fail to catch what is going on behind the scenes.
15. A whole range of rationality concepts are used and being developed in economics [see Tisdell 1975];

- never have economists been able to agree on the exact role rationality should play in economics.
16. I have in mind John Elliot Cairnes (1825-1875) and W. Nassau Senior (1790-1864).
 17. This debate culminated in a conference/confrontation that yielded the (somewhat strangely titled) book *The Positivist Dispute in German Sociology* by Theodor Adorno et al. [1976].
 18. The literature on economics and falsification is copious. See Redman [1989], where 41 sources are collected in Part 2, Section 3.1.
 19. This section owes much to Epstein's gem, *A History of Econometrics* [1987; originally a 1984 Yale University dissertation], in which he draws heavily on unpublished materials (unpublished research, internal memoranda, correspondence, and minutes from conferences between 1933 and 1954) from the Cowles Commission Archives at Yale University.
 20. The title is an obvious allusion to Keynes's quip likening econometrics to alchemy [Keynes 1940, 156].
 21. Many economists associate a failure to reject a theory with theory confirmation (proving a theory to be true) without realizing that both theory confirmation and falsification (proving a theory to be wrong) are impossible. I imagine that by "confirmed," Hendry simply means "accepted for the time being as plausible and reliable." The words "verification," "confirmation," "falsification," and "induction" all have such burdened histories that they have become hopelessly ambiguous, signalling that a reversion back to plain language is overdue.
 22. Consider, for starters, Hendry, Leamer, and Poirier [1990], McAleer et al. [1985], Pagan [1987], Summers [1991], and the contributions to the new journal *Econometric Theory*. Modern controversy in econometrics is, of course, not limited to issues of model selection, the focus of this paper.
 23. But recall that the fallibility of basic statements causes theory acceptance/rejection to fall back on a process similar to a trial by jury.

24. Obviously, a dilemma arises if several theories end up being compatible with the same data set—a problem that would soon arise and plague them.
25. Today Popper [1992] claims he "was always a little sceptical" about Marschak's project, that is, about whether falsification would work in economics. Apparently, at that time, Marschak was the only major pioneer figure in econometrics to have known Popper personally and read his work.
26. The original econometrics research staff at the Cowles Commission in 1944/45 under Marschak included Tjalling Koopmans (1910-1984), Leonid Hurwicz (1917-), Herman Rubin (1926-), Lawrence Klein (1920-), and T. W. Anderson (1918-).
27. The reference is to Tinbergen [1939]. Although in a much cited passage, Tinbergen [1939, 132] argues that while "no statistical test can prove a theory to be correct . . . [it] can, indeed, prove that theory to be incorrect," he claims to have had no contact with Popper or knowledge of his works, having, in general, little interest in or inclination for things philosophical [Tinbergen 1993].
28. Jacob Marschak, whose role in shaping econometrics was unsurpassed, had been Director of the Oxford Institute of Statistics before Alfred Cowles persuaded him in 1943 to direct the Cowles Commission (first founded in 1932 in Colorado and later moving to Chicago and finally to Yale). Koopmans acted as director of research at the Cowles Commission from 1948-1954 and again from 1961-1967.
29. Writes Epstein [1987, 53]: "The essay initially defends econometrics as a tool to falsify hypotheses and the argument seems quite similar to the one elaborated by Popper for natural sciences."
30. Compare, for example, Popper [1960, 12-13]: "I do *not* believe that *historical prophecy* is one of the tasks of the social sciences," as well as this passage in which Popper [p. 43] delineates the difference between prophecy and social engineering: "In the one case we are told about an event which we can do nothing to prevent. I shall call such a prediction a '*prophecy*'. Its

practical value lies in our being warned of the predicted event, so that we can side-step it or meet it prepared. . . . Opposed to these are predictions of the second kind which we can describe as *technological* predictions since predictions of this kind form a basis of *engineering*. They are, so to speak, constructive, intimating the steps open to us *if* we want to achieve certain results. The greater part of physics . . . makes predictions of such a form."

31. Epstein [1993] recalls asking Koopmans in 1984 whether he was familiar with Popper's work in the thirties and forties and receiving an answer to the negative. According to Epstein, at that point in time Koopmans was not even aware of Popper's influence on the Cowles Commission in the era.
32. Tinbergen, Frisch, and Koopmans had all originally been trained in physics (Tinbergen's Ph.D. was in physics, Frisch and Koopmans's in mathematical statistics) and were aware of the analogies between their methods and those of physics and thermodynamics. No doubt possessing a common background made Popper's ideas more conducive to the pioneer economists and enhanced the two-way flow of ideas between economics and the philosophy of science.
33. This was internal criticism since *Econometrica*, the journal of the Econometric Society, was run in the 1930s-1940s from the Cowles Commission. (Recall that Frisch was the editor of *Econometrica* from 1933-1954.)
34. The early econometricians and mathematical economists came up against strong resistance in general. Epstein [1987, 18], for example, recounts the story that the founder of American econometrics, Henry Moore (1869-1958), who did work in microeconomic econometrics (a field far less controversial than macroeconomics), was confronted with such antagonism from his contemporaries (most notably Marshall, Edgeworth, and Taussig) that he eventually refrained from attending professional meetings. In the

- end, he completely dissociated himself from his colleagues, save J. B. Clark.
35. Koopmans's 1941 essay is a rebuttal of Keynes's critique. But it was Trygve Haavelmo who met Keynes's criticisms head on by arguing for an explicit probability approach to structural estimation.
 36. As Milton Friedman [1953, 12, n. 11] noted, "If one hypothesis is consistent with available evidence, an infinite number are."
 37. This apparently caused an exasperated Koopmans to once retort: "But what if the investigator is *honest*?" [Epstein 1987, 107].
 38. These two objections make it hard to swallow Friedman's claim that he belongs to Popper's school of philosophy of science.
 39. "The profusion of competing models, of course, has continued to be the bane of empirical macroeconomics" [Epstein 1987, 106].
 40. Epstein [1987, 128] remarks that "the Cowles Commission found it nearly impossible to demonstrate a realistic capacity for their methods to guide the kinds of 'social engineering' projects which Marschak had discussed in 1941" and, further: "Hopes for discovering structure and designing effective policies of structural change began to fade nearly continuously from the onset of empirical work" [p. 8].
 41. In 1987, Tinbergen [pp. 132-33] elaborated on this theme: "I must say that I am also rather sceptical about forecasting. I think, I see much more in indicating how a certain evil can be abolished—that is, to say what the optimal policy is at a certain moment—than to forecast what will happen. That is much more difficult and you have to know many more things. I think the real task for econometricians is what I would call the policy part, that is to indicate what sort of policy has to be followed."
 42. He was not the only one to echo institutionalist doctrines. Keynes and Friedman both seemed to think that a realistic, useful model of the business cycle would have to incorporate a large number of variables in its equations to reflect adequately the

highly intricate historical and institutional structure of the economy.

43. Lawrence Klein continued building structural equation systems and is, I suspect, the only pioneer member who also belongs to the second generation.
44. Compare with Pesaran [1987, 13]: "The relatively stable economic environment of the 1950s and 1960s was an important factor in the initial success enjoyed by macroeconomic models. Whether the use of macroeconomic models in policy formulation contributed towards the economic stability over this period is, of course, a different matter."
45. In 1970, Ragnar Frisch [p. 153] of the first generation was counselling that "the econometric army has now grown to such proportions that it cannot be beaten by the silly arguments that were used against us previously. This imposes on us a *social and scientific responsibility* of high order in the world of today."
46. A technical discussion of data mining, sometimes jokingly referred to as "economagic," can be found in Lovell [1983].
47. For a fascinating characterization of the recent state of econometrics from a University of Chicago perspective, see Heckman [1992, 882-84].
48. Popper's critical rationalism does not always call for rules; his philosophy is frustratingly inconsistent. Like most of Popper's other concepts, falsification conflated too many ideas—testability, empirical refutation, intellectual honesty, logic, and the Socratic method, inter alia—some of which are incompatible with rules. (See also note 11.)
49. In my view, analytic philosophy is in a similar state for similar reasons.
50. The German passage that I have taken the liberty of translating reads: "Alle Sicherheiten in der Erkenntnis sind selbstfabriziert und damit für die Erfassung der Wirklichkeit wertlos."
51. This was part of a preface written by Keynes for a series of Cambridge Economic Handbooks; in later editions the preface was omitted, so the quotation has remained somewhat obscure.

52. See especially Bartley's [1984]. Popper, too, came to embrace this interpretation of critical rationalism.
53. Leamer advocates, in addition, the publication of failures of data sets: "It is obviously news worthy of publication," he asserts [1992, 92], "that a particular data set, which might have been useful, in fact could not shed light on some important empirical issue."
54. At least one econometrician is singing my song: "Econometric models are important tools of forecasting and policy analysis, and it is unlikely that they will be discarded in the future. The challenge is to recognize their limitations and to work towards turning them into more reliable tools. There would seem to be no viable alternatives" [Pesaran 1987, 19]. Consider also Walters's [1986] much less sanguine conclusions about modern econometrics.

References

- Adorno, Theodor, et al. *The Positivist Dispute in German Sociology*. New York: Harper and Row, 1976.
- Albert, Hans. *Traktat über kritische Vernunft*. 4th rev. ed. Tübingen: J.B.C. Mohr, 1980.
- Bartley, William. *The Retreat to Commitment*. 2d. rev. ed. La Salle, Ill.: Open Court, 1984.
- Blaug, Mark. *The Methodology of Economics or How Economists Explain*. Cambridge: Cambridge University Press, 1980.
- Boland, Lawrence A. "Testability in Economic Science." *South African Journal of Economics* 45 (March 1977): 93-105.
- Caldwell, Bruce. *Beyond Positivism*. London: Allen and Unwin, 1982.
- _____. "Some Problems with Falsification in Economics." *Philosophy of the Social Sciences* 14 (1984): 489-95.
- "Constitution of the Econometric Society." *Econometrica* 1 (1933): 106-108.
- Craver, Earlene, and Axel Leijonhufvud. "Economics in America: The Continental Influence." *History of Political Economy* 19, no. 2 (1987): 173-83.
- Epstein, Roy J. *A History of Econometrics*. Amsterdam, New York, Oxford, Tokyo: North-Holland, 1987.
- _____. Letter to author of 15 July 1993.

- Friedman, Milton. "The Methodology of Positive Economics." In *Essays in Positive Economics*, by Milton Friedman, 3-43. Chicago: University of Chicago Press, 1953.
- Frisch, Ragnar. "Econometrics in the World of Today." In *Induction, Growth and Trade: Essays in Honour of Sir Roy Harrod*, edited by W. A. Eltis, M. F. G. Scott, and J. N. Wolfe, 152-66. London: Clarendon Press, 1970.
- Glymour, Clark. "Interpreting Leamer." *Economics and Philosophy* 1 (1985): 290-94.
- Hanson, Norwood Russell. *Patterns of Discovery*. Cambridge: Cambridge University Press, 1958.
- Harding, Sandra G., ed. *Can Theories Be Refuted? Essays of the Duhem-Quine Thesis*. Dordrecht: D. Reidel, 1976.
- Hausman, Daniel. "An Appraisal of Popperian Methodology." In *The Popperian Legacy in Economics*, edited by Neil de Marchi, 65-85. Cambridge and New York: Cambridge University Press, 1988.
- _____, ed. *The Inexact and Separate Science of Economics*. Cambridge and New York: Cambridge University Press, 1992.
- Heckman, James J. "Haavelmo and the Birth of Modern Econometrics: A Review of *The History of Econometric Ideas* by Mary Morgan." *Journal of Economic Literature* 30, no. 2 (June 1992): 876-86.
- Hendry, David F. "Econometrics: Alchemy or Science?" *Economica* 47 (November 1980): 387-406.
- Hendry, David H., Edward E. Leamer, and Dale J. Poirier. "The ET Dialogue: A Conversation on Econometric Methodology." *Econometric Theory* 6 (1990): 171-261.
- Hicks, Sir John. "A Discipline Not a Science." In *Classics and Moderns*, vol. 3, by Sir John Hicks, 365-75. *Collected Essays on Economic Theory*. Oxford: Basil Blackwell, 1983.
- Iggers, Georg G. "Historicism." *Dictionary of the History of Ideas*, edited by Philip P. Wiener. New York: Charles Scribner's Sons, 1973 ed.
- Keynes, John M. "Comment." *Economic Journal* 50 (March 1940): 154-56.
- Klein, Lawrence R. "The Use of Econometric Models as a Guide to Economic Policy." *Econometrica* 15 (April 1947): 111-51.
- Koertge, Noretta. "Popper's Metaphysical Research Program for the Human Sciences." *Inquiry* 18 (1975): 437-62.

- Koopmans, Tjalling. "The Logic of Econometric Business-Cycle Research." *Journal of Political Economy* 49, no. 2 (April 1941): 157-81.
- Kuttner, Robert. "The Poverty of Economics." *Atlantic Monthly* 255, no. 2 (February 1985): 74-84.
- Lakatos, Imre. *Philosophical Papers*, edited by John Worrall and Gregory Currie. 2 vols. Cambridge: Cambridge University Press, 1978.
- Leamer, Edward E. "Let's Take the Con out of Econometrics." *American Economic Review* 73, no. 1 (March 1983): 31-43.
- . "Taste, Economics, and Econometrics." In *Educating Economists*, edited by David Colander and Reuven Brenner, 91-94. Ann Arbor: University of Michigan Press, 1992.
- Lee, Dwight E., and Robert N. Beck. "The Meaning of 'Historicism'." *American Historical Review* 59 (1953-54): 568-77.
- Lovell, Michael C. "Data Mining." *Review of Economics and Statistics* 65, no. 1 (February 1983): 1-12.
- Marschak, Jacob. "A Discussion of Methods in Economics." *Journal of Political Economy* 49, no. 3 (June 1941): 441-48.
- McAleer, M., A. R. Pagan, and P. A. Volcker. "What Will Take the Con Out of Econometrics?" *American Economic Review* 75, no. 3 (June 1985): 293-307.
- Mirowski, Philip. "Physics and the 'Marginalist Revolution'." *Cambridge Journal of Economics* 8 (1984): 361-79.
- Morgan, Mary S. *The History of Econometric Ideas*. Cambridge and New York: Cambridge University Press, 1990.
- Newton-Smith, W. H. *The Rationality of Science*. London: Routledge & Kegan Paul, 1981.
- Pagan, Adrian. "Three Econometrics Methodologies: A Critical Appraisal." *Journal of Economic Surveys* 1, no. 1 (1987): 3-24.
- Papandreou, Andreas George. *Economics as a Science*. Philadelphia: Lippincott, 1958.
- Passmore, John. "The Poverty of Historicism Revisted." *History and Theory*, Beiheft 14: Essays on Historicism 14, no. 4 (1974): 30-47.
- Pesaran, M. Hashem. "Econometrics." In *The New Palgrave Dictionary of Economics*, vol. 2, edited by John Eatwell, Murray Milgate, and Peter Newman. London: Macmillan; New York: Stockton Press; and Tokyo: Maruzen Press, 1987.

- Popper, Sir Karl. *The Poverty of Historicism*. 2d ed. 1957. Reprint. London: Routledge & Kegan Paul, 1960.
- _____. "Normal Science and Its Dangers." In *Criticism and the Growth of Knowledge*, edited by Imre Lakatos and Alan Musgrave, 51-58. London: Cambridge University Press, 1970.
- _____. *Conjectures and Refutations*. 4th ed. 1963. Reprint. London: Routledge and Kegan Paul, 1972a.
- _____. *The Logic of Scientific Discovery*. 6th ed. 1959. Reprint. London: Hutchinson, 1972b.
- _____. "Autobiography of Karl Popper." In *The Philosophy of Karl Popper*, edited by Paul Arthur Schilpp, vol. 1, 3-181. La Salle, Ill.: Open Court, 1974a.
- _____. "Karl Popper: Replies to My Critics." In *The Philosophy of Karl Popper*, edited by Paul Arthur Schilpp, vol. 2, 963-1197. La Salle, Ill.: Open Court, 1974b.
- _____. "Reason or Revolution." In *The Positivist Dispute in German Sociology*, by Theodor W. Adorno, 288-300. New York: Harper & Row, 1976.
- _____. *Realism and the Aim of Science*. Vol. 1, *Postscript to the Logic of Scientific Discovery*. Edited by W. W. Bartley III. London: Hutchison, 1983.
- _____. "Natural Selection and the Emergence of the Mind." In *Evolutionary Epistemology, Rationality, and the Sociology of Knowledge*, edited by Gerard Radnitzky and William Bartley, 139-53. La Salle, Ill.: Open Court, 1987.
- _____. Letter to author of 19 June 1992.
- Redman, Deborah. *Economic Methodology: A Bibliography with References to Works in the Philosophy of Science, 1860-1988*. New York, Westport, and London: Greenwood Press, 1989.
- _____. *Economics and the Philosophy of Science*. New York: Oxford University Press, 1991.
- Stewart, I. M. T. *Reasoning and Method in Economics*. Toronto: McGraw-Hill, 1979.
- Streissler, Erich W. *Pitfalls in Econometric Forecasting*. London: Institute of Economic Affairs, 1970.
- Summers, Lawrence H. "The Scientific Illusion in Empirical Macroeconomics." *Scandinavian Journal of Economics* 93, no. 2 (1991): 129-48.
- Tinbergen, Jan. *Statistical Testing of Business-Cycle Theories*. Vol. 1: *A Method and Its Application to Investment Activity*.

Geneva: League of Nations Economic Intelligence Service, 1939.

_____. "The ET Interview: Professor J. Tinbergen." Interviewed by Jan R. Morgan and Mary S. Morgan. *Econometric Theory* 3 (1987): 117-42.

_____. Personal correspondence to author of 24 April and 12 July 1993.

Tisdell, Clem. "Concepts of Rationality in Economics." *Philosophy of the Social Sciences* 5 (1975): 259-72.

Walters, Sir Alan. "The Rise and Fall of Econometrics." In *The Unfinished Agenda*, edited by Martin J. Anderson, 116-24. London: Institute of Economic Affairs, 1986.