"The Ghosts I Called I Can’t Get Rid of Now": The Keynes-Tinbergen-Friedman-Phillips Critique of Keynesian Macroeconometrics

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This essay is part of a wider investigation into the political economy of the Keynesian revolution and the monetarist counterrevolution. I examine the much-publicized dispute between those followers of Keynes who presented econometric evidence in favor of a Phillips curve trade-off, and those monetarists who presented countereconometric evidence. I argue that the collapse of the Keynesian Phillips curve vindicated a critique of macroeconomic practices, which was jointly authored by John Maynard Keynes, Jan Tinbergen, Milton Friedman, and A. W. H. “Bill” Phillips. This analysis is informed by the usual sources plus two documents that have been recently rediscovered (Phillips's private papers and records of the London School of Economics [LSE] Staff Seminar on Methodology, Measurement and Testing—referred to as MPT) and two essays by Keynes that have been overlooked in this context (1938a, 1938b).

Keynes's critique of econometrics can be disaggregated into three distinct categories, namely, technical issues that could be overcome by

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further research, criticisms that were directed at macroeconometrics but not necessarily at microeconometrics, and concerns that econometrics, in the wrong hands, could become a hazard for the economics profession. Econometricians have long been aware that Keynes's detailed technical criticisms were sometimes ill founded (R. A. Gordon 1949, 53, n. 4). Since this first category of Keynes's critique is not germane to my theme, these criticisms can be safely relegated to a footnote.¹

In this article, I marshal evidence in favor of four propositions. The first is that Tinbergen acknowledged the validity of the central thrust of one of Keynes's critiques. Keynes had addressed mechanical aspects of the econometric practices that were developing in the late 1930s—practices that had been unwittingly stimulated by The General Theory (Stone 1978, 62; Tinbergen 1947). The second proposition is that there is a considerable overlap between the views of Keynes and Friedman with respect to econometrics.

The third proposition is that Friedman (who was deputy director of the Statistical Research Group during World War II and who must be regarded as among the most statistically literate economists ever) implicitly predicted that macroeconomic disputation could only end inconclusively. In the late 1950s, there was a changing of the Keynesian guard in Cambridge, Massachusetts. Alvin Hansen, who was skeptical about econometrics, made way for younger Keynesians who apparently did not share these doubts. The year 1958 was a pivotal time in macroeconomic history: Phillips (1958) presented his seminal empirical paper and the senator from Massachusetts began to mobilize his "academic Kennedy gang" at Cambridge (Hauserstam 1972, 157; Leeson 1997b, 1997c), H. S. Houthakker (1958a, 1958b), on study leave at Harvard, chose to engage Friedman (1957) in an econometric dispute over the relative merits of rival consumption functions (which may explain why the anti-Keynesian counterrevolution, when it came, was monetarist and not Austrian).

This article's fourth proposition is that Phillips was an insightful early critic of Keynesian Phillips curve econometrics. This proposition is based, in part, on the recently discovered—and complete—records

¹ The question of how variables were measured, and in what units, left open the possibility of "devastating inconsistencies" (Keynes 1939, 563–66). Second, the assumption of linearity with respect to all economic forces was, he thought, "ridiculous" (Keynes 1940, 155). Third, the arbitrary choice of the first and last year of a series for which a time trend is calculated "looks to be a disastrous procedure" (Klein 1992, 184).
of the M2T Seminar series. This supplements Neil De Marchi's (1988) fascinating discussion, which was based on the incomplete records then available.

Econometrics has had some success stories; it has also had some less impressive episodes. More important, it has become an ambiguous but high-status language, engaging a large share of professional effort. According to George Stigler (1962, I), the statistical evaluation of economic relationships is the only distinctive trait of modern economics; but in 1912, Irving Fisher had been unable to find enough interested people (apart from W. C. Mitchell and a few others) to establish an econometric society. This society was ultimately founded by just sixteen people, in a meeting at the Statler Hotel, Cleveland, Ohio, in December 1930. The first European meeting of the society, in Lausanne in 1931, attracted about twenty people. The first volume of *Econometrica* had a circulation of less than 300 (Cowles 1960; Frisch 1970, 152; Christ 1952, 5; Bjerkholt 1995, 755). But today, econometrics occupies a large proportion of the pages of the professional journals, and according to Adrian Darnell and Lynne Evans (1990, ix), some see econometrics as an "umbrella discipline for economics."

Some econometricians have made an impressive theoretical contribution to statistical analysis; but doubts remain about the value of the "average economic regression." In the pre-econometric age, the average academic economist could aspire to become an authority on some aspect of the economy; now, it seems, many economists pursue professional advancement by applying (or misapplying) estimating techniques to data—in spite of Keynes's warnings. The quality or relevance of the work often remains unexamined.

The econometric pioneers hoped that they were uncovering a "rock" upon which to base reliable policy advice (Frisch 1970). But later econometricians (Pagan 1984, 103) have published scathing criticisms of the research strategy that underpinned the econometric models of the 1960s, which appeared to suggest that inflation would reduce unemployment. Econometricians (Laidler 1985) have also been concerned that the unemployment cost of reducing inflation—the dominant policy derived from "natural rate" econometric models from the 1970s onward—was much greater than anticipated by those models (Leeson 1997a). An investigation into the origins of this unwarranted confidence in macroeconometric models is simultaneously an investigation into the causes of these policy failures.
Despite Joseph Schumpeter's protestations (1933, 5), many econometricians have neglected, if not "belittle[d]," a superbly rich data source—the serial dependent history of their own subject. If the history of econometrics stood in equal status alongside other subdisciplines within economics, this might tend to alleviate some of the rather disturbing problems with regard to both graduate education and professional incentives: "Something is terribly wrong in the economics profession and in the incentives that economists perceive... in economics normal science has run amok. The invisible hand of truth has lost its guiding influence" (Colander 1989, 31, 34–35; see also Colander and Klammer 1987).

Tinbergen and Keynes

In the last decade, econometrics has begun to attract the attention of historians of thought, much of it focused on the Keynes-Tinbergen exchange. Most commentators have adopted one (or sometimes two) attitudes with respect to Keynes's critique. For some, it was a lamentable performance on Keynes's part (Klein 1951, 450–51), traceable to his ill health, technical rustiness, and tactical predilections (Stone 1978, 62–63). For some, Keynes simply misunderstood what Tinbergen was attempting to do (Klant 1985) or "did not really have the necessary technical knowledge to understand what he was criticising" (Samuelson 1946, 197, n. 11). For others, Keynes was, in a qualified way, more sympathetic to econometrics than had hitherto been supposed (Bateman 1990). Keynes's reference to "alchemy" may have been intended as a gesture of encouragement, suggesting that Tinbergen might ultimately succeed in creating the foundations of an econometric science (Rima 1988, 16). Some have even speculated that had he lived longer, Keynes might have become a computer-based modeler at the center of a "high-tech 'circus'" (Bodkin, Klein, and Marxhah 1988, 9, n. 10, 10–11, n. 15). Alternatively, others have argued that Keynes's critique is still relevant to modern econometrics (Patinkin 1976; Hendry 1980).

2. The student passing through an econometrics course would be forgiven for thinking that this subject lacked a systematically analyzed history: "It is a minor scandal that there is no comprehensive history of either the rise of econometrics or the mathematization of economics" (Weintraub 1985, 140). In recent years we have benefited from some excellent research into the history of econometrics. See, for example, Darnell 1984, Christ 1985, Epstein 1987, Morgan 1990, Darnell and Evans 1990, Keuzenkamp 1990, Dharmapala 1993, and the special issue of Oxford Economic Papers (De Marchi and Gilbert 1980).
Too much of the history of macroeconomics has been bedeviled by attempts to label (and sometimes libel) the author of the macroeconomic Old Testament. Surprisingly, while there have been both general and specific discussion of the Keynes-Tinbergen debate, there seems to have been no attempt to dissect Keynes's critique into operational categories. By treating the Keynes critique en bloc, we are in danger of concluding that his suspicions about econometrics were "invalid" (Malinvaud 1991, 636) or "venial and not to be remembered" (Stone 1978, 88, last sentence) or not worthy of mention (Stone 1980, section 3). A disaggregated approach shines a much clearer light on the aspects of Keynes's critique that have relevance to contemporary econometric practices. It also reveals that Tinbergen finally acknowledged the potency of parts of Keynes's critique, as Keynes predicted he would (1939, 568, last paragraph). The words from Goethe's Zauberlehrling that form part of the title of this essay were prophetically cited by Tinbergen on the occasion of his Nobel lecture (Tinbergen 1969, 43; for similar sentiments see Klein 1971b, 416).

Ragnar Frisch, Tinbergen's coreipient of the first Nobel Prize in economics also bemoaned "the cascade of papers of the playometric kind" (1970, 164) and had long been skeptical about the direction of applied econometrics (Arrow 1960, 183). Tinbergen (1967, 272) criticized economists for being averse to the time-consuming factual and statistical research that was required for quality empirical research. He also developed parts of what became known as the Lucas critique (Tinbergen 1956, 149–85; Lucas 1976, 20). Tinbergen assumed that "expectations are 'rational' i.e. are consistent with the economic relationships" (cited in Kenzenkamp 1991, 1247). Half a century after the publication of his Econometric Approach to Business Cycle Problems, M. R. Magnus and Mary Morgan (1987, 136) asked Tinbergen, "How do you feel about the way econometrics has developed over the last twenty years or so? In 1952 you feared that techniques could take over from attention to human needs and problems in the field of economics. Do you feel this fear was justified?" Tinbergen replied, "I'm afraid, yes." Again, a vindication of parts of the Keynes critique.

Keynes and Tinbergen are usually characterized as having incom-

3. Tinbergen (1969, 43) wrote, "Returning to models, I am sometimes wondering whether, upon looking at some recent work by planners, I should not repeat the famous words by Goethe's Zauberlehrling ... 'The ghosts I called I can't get rid of now'. Sometimes indeed some of our followers overdo model building."
patible views on econometrics. Yet Tinbergen's Nobel lecture can be viewed as the opening volley of the "orgy of self-criticism" (Blaug 1980, 253) that descended on the economics profession when the predictive powers of many macroeconomic models were found to be lacking (Leontief 1971, 3; Worswick 1972, 79; Phelps Brown 1972, 6). Given that some aspects of Keynes's warnings still retain their validity (Samuelson 1992, 243–44; Ormerod 1994, 92–112), we need to disaggregate his concerns about econometrics.

Keynes appreciated the qualities of "a real trained statistician" (CW 15:12) and was "in fundamental sympathy with the deep underlying conceptions of the statistical theory of the day" (CW 8:468). The subject of his second known letter to a newspaper was the interpretation of statistics. And one of his earliest academic disputes was with Karl Pearson over the appropriate statistical methods of studying the effects of parental alcoholism on offspring—a dispute that illustrated "the pitfalls of statistical inference" (Harrod 1951, 154). His final posthumously published article was "solely concerned with the available statistics" (CW 27:428; see also 430). One of the themes of his career was the analysis of "the logical basis of statistical modes of argument" and the search for "the principles of sound induction," which might constitute "a good scientific argument." Keynes had planned to specialize in logic and statistical theory, and A Treatise on Probability attempted to "cover the whole field of empirical thinking. . . . It would be difficult to find a parallel for a comprehensive attack of this kind since the days of Aristotle" (Harrod 1951, 126, 133–34). The final section of his fellowship dissertation was titled "The Foundations of Statistical Inference," which concluded with an "Outline of a Constructive Theory." The union of descriptive and inferential statistics was

the occasion of a great deal of confusion. The statistician who is mainly interested in the technical methods of his science, is less concerned to discover the precise conditions in which a description can be legitimately extended by induction. He slips somewhat easily from one to the other, and having found a complete and satisfactory mode of description he may take less pains over the transitional argument . . . [but] he must pay attention to a new class of considerations and must display a different kind of capacity. . . . He is faced, in fact, with the normal problems of inductive science . . . [involving material which] will be necessarily incapable of exact, numerical, or sta-
tistical treatment. . . Generally speaking, therefore, I think that the business of statistical technique ought to be strictly limited to preparing the numerical aspects of our material in an intelligent form, so as to be ready for the application of the usual inductive methods. (CW 8:359–60, 428; see also 419, 427, 468; 15:12, 20–21)

Most of the mathematical methods applied to statistical inference were invalid and could "only lead to error and to delusion" (CW 27:428, 430).

Keynes informed the Macmillan Committee on Finance and Industry that although "the empirical method is not by any means successful for the diagnosis [of the depression] it is not by any means valueless for seeking the cure" (CW 20:99). He concluded volume 2 of A Treatise on Money (1930, 408) with a plea for greater quantitative knowledge: "Statistics are of fundamental importance to suggest theories, to test them and make them convincing . . . [and] to eliminate impressionism." He opened The General Theory with a call for a statistical examination of the relationship between changes in money wages and changes in real wages. It was on statistical grounds that he asserted that the wage units could "only be reduced amidst the decay and dissolution of economic society" (1936a, 340, n. 1; see also 9–10, 40–41, 102–4).

Keynes was particularly opposed to the statistical method underpinning the American Keynesian Phillips curve tradeoff (CW 23:181–93). Chapter 6 of the unfinished Footnotes to the General Theory was titled "Statistical notes" (CW 14:134). He told Austin Robinson (1972, 535) that "all his best ideas came from messing around with figures and seeing what they must mean." But throughout his career he opposed the use of mathematical methods in both statistics and economics. When it came to questions of inference, experimental methods were often to be preferred to statistical methods (CW 11:216). Certain methods of statistical analysis led to invalid results. Investigations of samples rather than complete populations were also suspect. For statistics to be decisive, they had to extend over a period long enough to eliminate other influences (1936a, 104; CW 19:1:122).

Keynes was suspicious of all numbers derived by formulae from non-experimental data, especially when the original data had been suppressed. To "enable the reader to form some sort of independent judgement . . . the real character of the evidence" must be displayed, not just the products derived from applying "mathematical machinery" (CW 11:191). Graphs were highly suitable for "publicity or propaganda pur-
poses," as Florence Nightingale discovered; but Keynes warned of the "horrid examples of the evils of the graphical method unsupported by tables of figures. Both for accurate understanding and particularly to facilitate the use of the same material by other people it is essential that graphs should not be published by themselves but only when supported by the tables which will lead up to them. It would be an exceedingly good rule to forbid in any scientific periodical the publication of graphs unsupported by tables" (CW 11:234). But he was unstiming in his support for the statistician Udny Yule in his quest for a lectureship in Cambridge (Skidelsky 1983, 222). Appropriately, Yule (1926) went on to produce some classic work on "nonsense correlations."

In 1923, Keynes helped launch the regular London and Cambridge Economic Series barometric survey of business conditions, and he repeatedly campaigned for improved economic statistics, not to be used for regression analysis, but to offer intuitive insights into reality (Stone 1978, 64–72; Skidelsky 1992, 106, 414, 270; CW 27:371). He was very supportive of James Meade, Richard Stone, and the new central statistical department, and he thought that it was "most dangerous for too wide a gap to develop between inside and outside statistical information" (CW 22:329, 331). His first concern was whether the data were "good enough to stand the strain which has been placed upon them." The accuracy of statistics whose "sole purpose is to satisfy the . . . troublesome and often trifling curiosity of the academic statistician" could not be relied upon (CW 11:229; 15:36; see also 18:152; 22:82). "The suspicion of quackery has not yet disappeared [from statistics]. . . . There is still about it for scientists a smack of astrology, of alchemy" (CW 8:367). But he looked forward to a systematic theory of statistics and the continued quantification of economics: "Whether the uniformity of economic settings is sufficient to enable the economist to make full use of this kind of work, time will show" (CW 11:226; see also 50–51). The "excellently complete statistics now available in the United States" could be used to illustrate aspects of the theory of the trade cycle (Keynes 1936a, 332). If Tinbergen simply examined "statistically particular cases, regarding them as particular cases, and no more . . . I am entirely in favour of him" (CW 14:302).

Keynes was a leading exponent of the 1930s revolt against nonquantitative economics: "It was [Keynes's] natural inclination to approach any problem from the angle of measurement of phenomena. . . . But just as he was skeptical of ideas that could not be verified by measure-
ment, so he was skeptical also of the adventures of the statisticians into the world of correlations built on insufficient logical foundations” (Robinson 1947, 44; 1992, 211). Keynes argued that with statistical methods “elaborate calculations . . . confuse, though they might also impress, all readers outside a very restricted class.” It was “the nature of valid argument which is in dispute . . . Professor Pearson may cover up by elaboration of method . . . [but] it is difficult to know how properly to characterise the work of a statistician who uses in controversy a table of this description with complete dogmatic assurance and without making plain to the reader the principles of its construction” (CW 23:191–92, 199, 205).

With respect to Tinbergen, the problem of multicollinearity between variables exposes econometricians to “the extraordinarily difficult and deceptive complications of ‘spurious’ correlations.” Yule’s discovery “sprang a mine under the contraptions of optimistic statisticians . . . . It becomes like those puzzles for children where you write down your age, multiply, add this and that, subtract something else, and eventually end up with the number of the Beast in Revelation” (CW 14:309–310; see also Tinbergen 1992, 278). It was essential to investigate whether correlation coefficients were stable across subseries. Such was the excessive emphasis on “the mathematical complications, that many statistical students hazily float from defining the correlation coefficient as a statistical description to employing it, as a measure of the probability of a statistical generalisation between quantitative variations” (CW 8:464; see also 428). The coefficients derived from the method of applying multiple correlations to “unanalysed economic material, which we know to be non-homogenous through time . . . are not constant. There is no reason at all why they should not be different every year. . . . How are these coefficients arrived at? . . . One gets the impression that it is a process of fitting a linear equation through trial and error” (CW 14:286–87, 292).

Tinbergen believed that he had tested for the constancy of the coefficients (CW 14:286–87, 292). Keynes, however, was absolutely correct. H. Pesaran and R. Smith noted the complete absence of parameter stability when they reestimated Tinbergen’s investment equations: “The estimated coefficients move all over the place” (1985, 144). Tinbergen (1969, 43) acknowledged that he and his fellow researchers found it safer “to ask industrialists for their investment programs rather than rely on an econometric explanation.” The econometric modeling
of investment remains one of the most notoriously unsuccessful areas of applied econometrics.

Keynes objected to econometrics for the same reason that he criticized the “classical” economists: “Progress in economics consists almost entirely in a progressive improvement in the choice of models. The grave fault of the later classical school, exemplified in Pigou, has been to overwork a too simple or out-of-date model. . . . But it is the essence of a model that one does not fill in real values for the variable functions. To do so would make it useless as a model” (Keynes 1936a, 297). Economics was a method of thinking.

The object of our analysis is, not to provide a machine, or method of blind manipulation, which will furnish an infallible answer, but to provide ourselves with an organized and orderly method of thinking out particular problems; and, after we have reached a provisional conclusion by isolating the complicating factors one by one, we then have to go back on ourselves and allow, as well as we can, for the probable interactions of the factors amongst themselves. This is the nature of economic thinking. Any other way of applying our formal principles of thought (without which, however, we shall be lost in the woods) will lead us into error. (CW 14:296; for Friedman’s approving echo of this Marshallian theme, see 1953, 7)

Keynes was not “content with the sort of broad general impression of how things worked that contents so many macro-economists” (Robinson 1972, 534). He warned his students that “the stuff of economics was not sharp or precise, and it was too easy to distort it and create for it the impression of exactitude that it really lacked, and by subjecting it to mathematical manipulation also to wind up with a seriously distorted picture of the economy” (Tarshis 1977, 73). He was concerned about “the appalling state of scholasticism into which the minds of so many economists have got which allows them to take leave of their intuitions altogether. Yet in writing economics one is not writing either a mathematical proof or a legal document” (CW 29:150). “The real tool is thought, and [equations] are not a substitute for it, but at most a guide or embodiment” (cited by Young 1987, 13). Almost identical concerns were echoed a generation later by Frisch and T. C. Koopmans.4

4. Frisch (1970, 165) approvingly quoted Norbert Wiener’s remark about the economists’ habit of “dressing up their rather imprecise ideas in the language of the infinitesimal calculus,” which was analogous to the vague feelings that “these magic rites and vestments will at
Like Friedman (1967, 88; 1974), Keynes had a corresponding nosological concern about the economics profession. Keynes had "a very poor opinion of Marschak" and described Colin Clark as "almost the only economic statistician I have ever met who seems to me quite first class" (CW 29:57, n. 11; O'Donnell 1992, 16). He had long-held opinions concerning the fruitlessness of certain statistical rather than experimental methods of analysis, of the impossibility of reducing human conduct to a set of equations, and of using "the collection of facts for the prediction of future frequencies and associations" (CW 8:368). There was "great danger in quantitative forecasts which are based exclusively on statistics relating to conditions which are by no means parallel" (CW 23:192).

Keynes was concerned that the statisticians' occupational disease should not become the economists' occupation. As he wrote Roy Harrod (in reference to Tinbergen's work), "I think it most important, for example, to investigate statistically the order of magnitude of the multiplier . . . [but] to convert a model into a quantitative formula is to destroy its usefulness as an instrument of thought. . . . By filling in figures, which one can be quite sure will not apply next time, so far from increasing the value of his instrument, he has destroyed it" (CW 14:299). Most of the claims derived from statistical inference, he argued, were inadmissible from the perspective of logic (of which economics was a branch) and were evidence of "mathematical charlatanry" (cited in Skidelsky 1983, 223). Keynes, like Harrod, held Tinbergen in the highest regard, yet Tinbergen's econometric work, he wrote in a note to Richard Kahn, was "all hocus" and simply a "mess of unintelligible figurations" (CW 14:289). The influences on investment were variables,
and, therefore, "it is logically impossible to discover by Tinbergen's method the comparative dependence on profit lagged... I complain that this sort of logical point is not first discussed—or even mentioned. Until it is, the whole thing is charlatanism in spite of Tinbergen's admirable candour" (CW 14:305; see also 304, 332).

In *The General Theory*, Keynes argued that one of the origins of macroeconomic instability was in the "animal spirits" of those undertaking investment, which depended upon "the nerves and hysteria and even the digestion and reaction to the weather of those upon whose spontaneous activity it largely depends" (1936a, 161–62). Crucial variables, such as the rate of interest and the marginal efficiency of capital, "are particularly concerned with the indefinite character of actual expectations; they sum up the effect in men's market decisions of all sorts of vague doubts and fluctuating states of confidence and courage. They belong, that is to say, to a stage of our theory where we are no longer assuming a definite and calculable future... Our precision will be mock precision if we try to use such partly vague and non-quantitative concepts as the basis of a quantitative analysis" (1936a, 39–40). Statistical comparison could be useful, "depending on some broad element of judgement rather than strict calculation" (1937, 151).

Multiple correlation analysis was "too elaborate and adds little or nothing" (cited in Epstein 1987, 143). This type of analysis requires that a complete set of relevant variables is included and is accurately measurable. (For Friedman's elaboration of this point, see 1953, 32, 49.) There will be a "serious misrepresentation of the causal process, if in fact some significant factors have been omitted" (Keynes 1939, 566). As a piece of "historical curve fitting and description... it is not a very lucid way of describing the past" (Carabelli 1988, 291, n. 10). Deriving inductive generalizations from statistical descriptions is a hazardous operation that requires environmental conditions to remain homogeneous and uniform in future time periods (CW 8:359–470). The material to which economic models are applied is, "in too many respects, not homogenous through time" (CW 14:296; for similar sentiments, see Alfred Marshall, approvingly cited by Friedman 1953, 90). This implies that econometrics is inappropriate in cases when "political, social and physiological factors, including such things as government policy, the progress of invention, and the state of expectations may be significant. In particular, it is inapplicable to the problems of the Business Cycle" (Keynes 1939, 561). In a letter to Gerald Shove, Keynes
wrote that "as soon as one is dealing with the influence of expectations and of transitory experience, one is, in the nature of things outside of the realm of the formally exact." Keynes concluded that "one feels a suspicion that the choice of factors is influenced (as is indeed only natural) by what statistics are available, and that many vital factors are ignored because they are statistically intractable or unprocurable." On a visit to the United States, he cautioned younger economists such as Gilbert, Humphrey, and Salant against neglecting important theoretical considerations "in the interests of simplifying their statistical task" (CW 14:2, 287; 23:192).

Prophetically, as Friedman (1991, 36) pointed out, Keynes predicted that econometrics had acquired a momentum of its own that would make its practitioners resistant to criticism. Tinbergen will probably "engage another ten computers and drown his sorrows in arithmetic. . . . The worst of him is that he is much more interested in getting on with the job than in spending time in deciding whether the job is worth getting on with. He so clearly prefers the mazes of arithmetic to the mazes of logic" (Keynes 1939, 568, 559). Keynes was clearly not opposed to statistical analysis, but "he hated stupidity, not only with aesthetic but also with a moral hatred: stupidity prevented the accomplishment of what was best for the world" (Robinson 1947, 29). He was primarily concerned that mechanical econometric practices might become a tangled web for the economics profession. He conducted a "ferocious campaign to discredit the activities of Tinbergen and later Kalecki. . . . Keynes' opposition to [multiple correlation analysis] was extraordinarily unyielding" (Epstein 1987, 142–43). "It was the unjustifiable inductive pretensions that provoked his venom" (Pesaran and Smith 1985, 147). "Keynes attacked Tinbergen's efforts with an astonishingly fierce barrage of arguments" (Skidelsky 1992, 618). Keynes also referred, perhaps mockingly, to "nefarious econometrics" (Stone 1978, 63), and in 1946 he told Jacob Viner that he "disowned any responsibility for their [his disciples] reliance on restricted and mechanical manipulations of a few statistical series, rather than making a broad survey of the significant factors and using judgement in asaying their importance and the nature of their impacts" (Viner 1964, 265).

W. C. Mitchell provided the momentum that led to the establishment of the Oxford Institute of Statistics (Young and Lee 1993, 119–20; Harrod 1949). Keynes was determined to establish a Cambridge depart-
ment of what he called "statistical realistic economics" in opposition to Tinbergen's macroeconometrics. It may have also been intended as a rival to the Oxford Institute of Statistics, which had appointed Jacob Marschak (who had fled both Lenin and Hitler) as director, and where Lawrence Klein would seek refuge during the McCarthyite period. Keynes favored the use of balance sheets and survey data (which elicited preferences) in the investigation of quantitative policy issues (Epstein 1987, 142–43). Richard Stone (1978, 83–87) became the first director of the Cambridge Department of Applied Economics in April 1946, the month of Keynes's death. In one sense, Stone and his coworkers acknowledged Keynes's critique by focusing their research efforts on the econometric analysis of modern demand theory, which is widely regarded as an econometric success story, in contrast to the rather disappointing performance of the large macroeconomic models (Gilbert 1991).

Finally, and perhaps most important for Keynes, was the question of the likelihood of self-deception and of the integrity and biases of the econometrician—"the spirit with which the subject is tackled," as David Hendry called it (1980, 403). Richard Feynman argued that the first principle of scientific integrity is that "you must not fool yourself, and you are the easiest person to fool" (cited in Warsh 1988, 251). For Keynes, "the more complicated and technical the preliminary statistical investigations become, the more prone inquirers are to mistake the statistical description for an inductive generalization" (CW 8:361). In particular, ad hoc specifications of time lags introduce the possibility that the econometrician will fidget "about until he finds a time-lag which does not fit in too badly with the theory he is testing" (1939, 565; see also 563–64). With respect to the assumption of linearity, Keynes warned that "it would certainly seem that quite easy manipulation on these lines would make it possible to fit any explanation to any facts" (1940, 155; see also Klein 1992, 184).

With respect to Tinbergen: "There is no-one, therefore, so far as human qualities go, whom it would be safer to trust with black magic. That there is anyone I would trust with it at the present stage or that this brand of statistical alchemy is ripe to become a branch of science, I am not yet persuaded." It might be fruitful to use these methods to investigate more elementary cases, such as the estimation of the various influences on the net investment in railway rolling-stock (CW 14:320, 288, 295, 317; see also 287, 289). Keynes spoke highly of fore-
casts derived from statistical analysis—when applied to cases such as the international corn trade (1938a). But regression analysis could not legitimately be applied to macroeconomic problems such as the “problem of imports as a whole” (cited in Carabelli 1988, 291, n. 10).

**Friedman and Keynes**

Econometric Disputation

Keynes and Friedman are both associated with the idea that predictive failure is damaging to scientific status; both doubted the existence of “conclusive” tests or evidence in economics (Keynes 1936a, 33, vii–viii; Friedman 1953, 30). They perceived themselves to be heirs to an “oral tradition” in monetary theory (CW 11:375; Friedman 1956; Patinkin 1969). But methodologically, The General Theory is a tract on the importance of examining the realism and relevance of assumptions (see, for example, 1936a, 276); and Friedman has confirmed that his own methodology was constructed in opposition to this tendency (personal correspondence, 18 April 1995).

When it came to the “scientific problems” (1957, ix) associated with data analysis, for over half a century Friedman has elaborated and echoed many of the themes discussed by Keynes. In his seven-page centenary article for the Economic Journal (1991, 36–38), entitled appropriately “Old Wine in New Bottles,” Friedman humorously refers to some of his own regressions as a “clear case of GIGO” (Garbage In, Garbage Out). On a more serious note he concluded that the capacity to put data through the computer-based “econometric wringer” had “induced economists to carry reliance on mathematics and econometrics beyond the point of vanishing returns. I generate multiple regressions these days at a rate that I never would have contemplated three or four decades ago—and many more than I would have if I followed my own prescription for proper research procedures.”

Friedman (1991, 36–38) also displayed an appreciation of the way in which “the Keynesian revolution changed the language and tools with which economists analysed the aggregate economy.” He noted that the structure of professional incentives—“the tendency to count rather than to evaluate publications”—had created an inbuilt bias toward generating low-quality econometric research, derived from data mining: “There is wide agreement that GIGO . . . is a real problem.” His train-
ing as a statistician had made him acutely aware that all statisticians “like to use our fancy techniques to see what the data show” (1963, 8).

These themes occur throughout Friedman's career. His famous and influential methodology of positive economics (1953, 301–19, 3–43) had been formulated in the context of some highly misleading Keynesian macroeconomic forecasts: “Errors in forecasting may have nothing to do with the validity of many of the underlying theories. . . . These [other] more accurate predictions do not prove that their methods are superior to those that failed” (Klein 1946, 289). For Friedman, in contrast, the chief obstacle to the attainment of positive status was the difficulty of testing the validity of tentative hypotheses. Economic data were difficult to interpret: “This hindered greatly the permanent weeding out of unsuccessful hypotheses. They are always cropping up again” (Friedman 1952, 456–57). This led again to an emphasis on the realism of assumptions, “the battle cry of institutionalists and the closely related emphasis on extensive statistical studies of economic phenomena which constituted an easier test of hypotheses. . . . Alfred Marshall's emphasis on the construction of an ‘engine for the discovery of concrete truth’ has tended to be submerged under the urge for descriptive realism” (1953, 56–57). Friedman approved of the Marshallian method used by Keynes to explore the theory of employment, but he disapproved of the Walrasian method employed by some Keynesians (ibid., 92). But from the mid-1930s, the formalist general equilibrium revolution began to supplant the Marshallian “engineers.” Marshall's ambiguities, it was claimed, had “paralysed the best brains in the Anglo Saxon branch of our profession for three decades” (Samuelson 1967, 109).

Robert Lucas and Thomas Sargent noted that the “Keynesian revolution was, in the form in which it succeeded in the United States, a revolution in method” (1978, 50). Friedman (1953, 277–300) led the “Methodological Criticism” of Oscar Lange's 1944 Cowles monograph, *Price Flexibility and Employment*: An economist who is concerned about economic reality “is not likely to stay within the bounds of a method of analysis that denies him the knowledge he seeks. He will escape the shackles of formalism, even if he has to resort to illogical devices and specious reasoning to do so.” For almost a decade, Frank Knight and Friedman led the “fairly intense struggle” against the Cowles Commission at the University of Chicago (Reder 1982, 10). As part of his critique of the Cowles Commission approach to economet-
rics, Friedman noted that "we have fallen into the habit of not trying to
test the validity of many hypotheses even when we can do so. . . . After
all most experiments are destined to be unsuccessful; the tragic thing is
that in economics we so seldom find out that they are" (1951, 107).

Friedman took over much of the Keynes critique and made it his
own. Yet the evaluation of econometric evidence became the "space-
time" arena of the disputes between Keynesians and monetarists—
who began to resemble electrons with opposite spin, in the same orbit.
Paradigmatic challengers have to fight on grounds chosen by the dom-
inant orthodoxy. Both sides confidently concluded that the evidence
supported their a prioris and was "so strikingly one-sided" (Friedman
side in particular, there was a belief that precision econometric mod-
ing would eliminate the "ambiguous use of language—[the] Marsha-
lian legacy shamelessly indulged in by all sides" (Desai 1981, 64). This
episode of intellectual history revealed that econometrics was not
powerful enough to unambiguously discriminate between alternative
weltanschauungs.

There is a paradox here. Keynesian macroeconometrics, at least ini-
tially, retained a faith in structural estimation as a tool for discrimi-
nating between the "true" and the "false" model, and also for effecting
the type of government policies that would mitigate, if not eliminate,
the business cycle. The losing side (in terms of policy influence from
the mid-1970s) suffered a double defeat. The winning side scored a
double victory: monetary targeting (based, in part, on the results
derived from monetarist macroeconometrics) temporarily replaced
Phillips curve targeting; Friedman's (and Keynes's) suspicions about
macroeconometrics also appeared to have been partly vindicated.

Harry Johnson, in his remarkable Richard T. Ely Lecture titled "The
Keynesian Revolution and the Monetarist Counter-Revolution," wrote
that one of the reasons for the success of the Keynesian revolution was
that "The General Theory offered an important empirical relationship
for the emerging tribe of econometricians to measure." Likewise, mon-
etarism advanced "a new and important empirical relationship, suitable
for determined estimation by the budding econometrician. That rela-
tionship was found in the demand function for money." The methodol-
ogy of positive economics "offered liberation to the small-scale intel-
lectual, since it freed his mind from dependence on the large-scale
research teams and the large and expensive computer program." The
monetarist counterrevolution would “peter out” because “monetarism is seriously inadequate as an approach to monetary theory”; given its “abnegation of responsibility for explaining the division of the effects of monetary changes between price and quantity movements . . . one should not be too fastidious in condemnation of the techniques of scholarly chicanery to promote a revolution or a counter-revolution in economic theory” (Johnson and Johnson 1978, 189, 196–98).

Milton Friedman (personal correspondence, 18 April 1995) recalls that “Harry Johnson was an extremely subtle and sophisticated person. . . . Harry was originally a very strong Keynesian who was converted to monetarism. He remained something of a Keynesian whenever he was in Chicago and was a strong monetarist whenever he was in London.” Johnson was clearly fueled by a variety of motives and inputs; jealousy of his Chicago colleague may have been one of them. But Friedman’s genius (like Keynes’s) extends to an understanding of the sociology of knowledge in the economics profession (see, for example, Friedman 1955b, 902).

Keynes (1936a, 21, 81) highlighted the power of the “optical illusion” of Say’s law. In his defense of Mitchell, Friedman drew the contrast between Mitchell’s work and “the shoddy work that passes for scientific” (1950, 470). He also bemoaned the success of the Cowles Commission econometricians in cultivating the “illusion that Mitchell was antitheoretical” (1950, 467). He noted that “worthless” Keynesian national income models (Friedman and Becker 1957, 68), which mis-represented the underlying macroeconomic structure, could nevertheless become hegemonic on the back of a “Statistical Illusion” (ibid., 73)—when accompanied by an analytical system that “once mastered, appeared highly mechanical and capable of yielding far-reaching and important conclusions with a minimum of input” (Friedman 1970, 207, n. 6). His early sophisticated theoretical work on stabilization policy (1948a; 1953, 117–32) had not noticeably undermined Keynesian confidence; nor had it stimulated much further research (see, for example, Neff 1949a, 1949b), despite his assertion that “the question is empirical” (Friedman 1949, 954). The Cowles—National Bureau of Economic Research (NBER) methodological dispute had produced only “desultory skirmishing” (1951, 114). His theoretical work left him feeling “as if I were preaching in the wilderness and behaboring the obvious.” Even “distressingly obvious” conclusions could be “widely neglected” (1953, 131; for almost identical words, see Keynes 1936a, viii). Likewise, his
plea for a Marshallian redirection of economics—Léon Walras’s “divorce of form from substance” had led to some “nonsense”—failed to persuade (1955b, 908–9; for almost identical words, see Keynes, cited by Skidelsky 1992, 615). Friedman (1955a, 402) found it “fantastic” that his empirical estimates of the effect of unions on the wage structure should lead to only an unproductive theoretical rebuttal: “I guess the farther grass looks greener to both of us.”

*A Theory of the Consumption Function* (1957)—which was labeled, in part, the “Friedman Effect”—was perceived to have contributed to putting “trade cycle theory on what one might call ‘a fully expectational footing’” (Farrell 1959, sections 7–8, 694). It also appears to be a transitional piece demonstrating Friedman’s increasing ability to engage his opponents in a statistical dispute. In his assault on one of Keynes’s (1936a, 95) central propositions regarding the stability of the consumption function, Friedman (1957, 86, 231) argued that Keynesians such as Klein had presented, as supporting evidence, regression results “revealing a high degree of sophistication and ingenuity in statistical techniques and economic analysis” that were, nonetheless, “almost worthless . . . an illusion attributable to the method of analysis . . . The consumption analyst as it were, has been priding himself on his success in adding yet more epicycles.”

Friedman’s book (1957, ix) is notable also for the “almost complete absence of statistical tests of significance”—but is widely regarded as “one of the masterpieces of modern econometrics” (Blaug 1985, 63). It also provoked an intense statistical exchange between H. S. Houthakker (1958a, 1958b) and Robert Eisner (1958b). Eisner was simultaneously (1958a) defending Harrod—Domar—Hicks growth models against the “Neo-Classical Resurgence,” which was led by James Tobin and Robert Solow. Friedman thought he had been addressing the “statistically sophisticated reader” (1958, 991). Houthakker, somewhat on the back foot, thought that “Friedman had strained the statistical sophistication of his readers to the limit.” Part of the debate centered around “alleged correlation[s],” and Houthakker (1958b, 991, 993) concluded by stating that “the process of testing the hypothesis has only just begun.”

Houthakker’s article was “the first full frontal statistical assault on my work” (personal correspondence from Friedman, 18 April 1995). Friedman and a growing body of associates and students were venturing “into almost virgin territory,” which they expected would “provoke
controversy. . . . What the calculations of our critics do is to establish
a presumption that further research along similar lines may be more
rewarding than we thought was likely” (Friedman and Meiselman
1965, 753, 784). What followed was the contest between the radio sta-
tions, FM versus AM. Friedman had found, in econometric disputation,
the soft underbelly of the Keynesian system.

Schumpeter wrote, in the now almost forgotten article “Keynes and
Statistics,” that “throwing discretion to the wind, they [orthodox Keynes-
ians] have attempted to rush trenches that are stronger than they
looked to them. Econometricians behaved like the inexperienced
arms of 1914–18, and with exactly analogous results. . . . Keynes did
not order these attacks” (1946, 196). It seems that those who neglect the
study of history may be condemned to repeat it: The first time as
tragedy, the second time as farce.

Friedman’s Critique of Econometrics

Monetarism was projected and interpreted as a belief in the existence of
a stable, empirically identifiable relationship between the rate of
growth of the stock of money and the corresponding rate of inflation.
However, although he is happy to describe himself as “an empiricist,”
a perennial theme of Friedman’s writings is a suspicion about the reli-
ability of empirical results. 5 It was the “impact of experience” of infla-
tion that led to the “rediscovery of money,” not the “serried masses of
statistics massaged through modern computers.” What Koopmans in
the 1940s called the “Friedman critique” involved an “assault on struc-
ture” and was designed to pour cold water on the postwar enthusiasm
about deriving causal relations from data. The “exaggerated claims” of
“scientific magic” could not disguise the fact that “every attempt . . . to
forecast economic activity has to date met with failure.” In particular, it
was “a pure act of faith to assert” that Klein’s econometric model can
predict the effect of policy changes, and there is no reason to share this

5. Friedman knows more about the use and abuse of statistics than most economists; he
studied at Columbia University under the mathematical statistician Harold Hotelling
(1933–49); he was a statistical assistant to Henry Schultz at the University of Chicago (1934–
35); he worked with economic measurement and data analysis at the NBER (1937–40); and he
was statistical director of the Wisconsin Income Study (1940–41). Friedman’s early career
either combined or alternated between mathematical statistics and economics. For most of
the war years, at least, he was exclusively concerned with mathematical statistics (R. Fried-
faith until some evidence for it is presented” (Friedman 1981, 30; 1975, 176; 1948b, 140–41; 1951, 111). Lucas (1976, 20) also found in Friedman’s *A Theory of the Consumption Function* a forerunner of his critique of econometric policy evaluation.

A few citations from Friedman will illustrate this theme: “Tinbergen’s results are simple tautological reformulations of *selected* economic data. . . . The methods used by Tinbergen do not and cannot provide an empirically tested explanation of business cycle movements. As W. C. Mitchell put it some years ago “a competent statistician with sufficient clerical assistance and time at his command, can take almost any pair of time series for a given period and work them into a form which will yield coefficients of correlation exceeding ±.9”” (1940, 659). High \( t \) statistics and correlation coefficients are “a test primarily of the skill and patience of the analyst” (1951, 108). Statistical evidence could be “extremely misleading” (1962, 170) and was only available to confirm “general reasoning” and to offer a guide to what is “reasonable” (1953, 231, 312, n. 8); “in view of the record of forecasters, it hardly needs to be argued that it would be better to shun forecasting and rely instead on as prompt an evaluation of the current situation as possible” (1948a, 253).

The opening words of *A Monetary History* were from Alfred Marshall: “Experience in controversies such as these brings out the impossibility of learning anything from facts till they are examined and interpreted by reason; and teaches that the most reckless and treacherous of all theorists is he who professes to let facts and figures speak for themselves, who keeps in the background the part he has played, perhaps unconsciously, in selecting and grouping them, and in suggesting the argument *post hoc ergo propter hoc*” (Friedman and Schwartz 1963). “Facts by themselves are silent . . . The economist must be suspicious of any direct light that the past is said to throw on the problems of the present. He must stand fast by the more laborious plan of interrogating facts” (Marshall, cited in Friedman 1953, 90; see also 1950, 465; 1957, ix). The interpretation of evidence “cast up by experience, as opposed to controlled experiments, generally requires subtle analysis and involved chains of reasoning, which seldom carry real conviction” (1953, 10–11).

Friedman’s emphasis on spurious correlation and on the corresponding suspicion regarding statistics such as a high \( R^2 \) (1957, 149–50; 1991, 36) echoed Keynes’s sentiments (1939, 561) and also foreshad-
owed later works such as Tobin 1970, Granger and Newbold 1974, and Cooley and LeRoy 1981. It is consistent with Hendry's (1980) demonstration that cumulative rainfall outperforms the money stock in price equations, with $R^2$ approaching unity. Indeed, the problem of "nonsense correlations" was commonly acknowledged in the interwar period (Yule 1926); Friedman was expressing a widely held view. The implication of Friedman's cynicism is that the "shootout at high noon" approach of the econometrics movement could only end inconclusively, at least at the level of conventional statistical criteria. (As it turned out, this was a highly accurate prediction of the forthcoming bouts between monetarists and Keynesians.) The "winner" would have to emerge on grounds other than conventional levels of statistical significance. Yet Friedman and David Meiselman were interpreted as having taken their stand in favor of the monetarist macroeconomic model on the basis of superior econometric performance, as measured by the size of the correlation coefficient (1963, 160). Rather late in the day, econometricians came to realize "the futility of the $R^2$ game" (Poole and Kornblith 1973, 916 (quote); Brainard and Cooper 1975, 169–70; Samuelson 1973, 389), that is, the validity of parts of Friedman's critique of econometrics.

There were "sharp differences of judgement" between members of the Cowles Commission (during its sojourn at Chicago) and economists at the University of Chicago (Hildreth 1986, 5). During 1946–48, Friedman was a frequent participant at the Cowles Commission seminars. His relentless criticism prompted Koopmans to ask, "But what if the investigator is honest?" (cited in Epstein 1987, 107). Friedman predicted that the Cowles Commission macroeconomic models would be revealed to be unsuccessful: "The construction of a model for the

6. Critics argued that they did not test the restrictions imposed. They were accused of a misspecification that influenced the outcome of the race—"letting up two strawmen and crowning one of them" (Dorni 1981, 112, 104–6; see also Amd and Modigliani 1965; de Franco and Mayer 1965; Hester 1964).

7. Koopmans's rejoinder (1947) indicated that the pioneer econometricians did not regard the Keynes-Friedman critique as fatal to their project. This brilliant group of scholars (including seven future Nobel Laureates—Simon, Debreu, Becker, Arrow, Tobin, Koopmans, and Klein) proceeded to lay the theoretical foundation of econometrics. Eleven of the thirty-three research associates (1939–55) were elected to membership in the National Academy of Sciences, and twenty-two become presidents of major professional associations (Hildreth 1986, 111; Klein 1978, 326). Frisch and Tinbergen shared the first Nobel Prize in Economics. This econometric work was in stark contrast to the "statistical economics" of Bums and Mitchell at the NBER, who, Koopmans believed, studied business cycles "as if they were the eruptions of a mysterious volcano" (cited in Epstein 1987, 64).
economy as a whole is bound to be almost a complete groping in the dark. The probability that such a process will yield a meaningful result seems to be almost negligible." Structural estimation was a "blind alley for empirical research. . . . Despairing of their abilities to reach quantitative answers by a direct analysis of these complex interrelationships, most investigators have sought refuge in empiricism and have based their estimations on historical relationships that have appeared fairly stable." Like Keynes, he argued that prejudices or the "psychological needs of particular investigators" would tend to predetermine the outcome; "the background of the scientist is not irrelevant to the judgements they reach." Friedman drew an analogy with Heisenberg's indeterminacy principle and "the interaction between the observer and the process observed that is so prominent a feature of the social sciences. . . . Both have a counterpart in pure logic in Gödel's theorem, asserting the impossibility of a comprehensive self-contained logic" (1943, 114; 1951, 113; 1953, 12, n. 11, 30, 5, n. 3).

In his contribution to The Lives of the Laureates, Friedman concluded that "I've been very sceptical of the economic forecasts that people like myself and others make by using multiple regression analysis" (1988a, 88). "I have long been sceptical of placing major emphasis on purely statistical tests, whether t-values, Durbin-Watson statistics, or any others. They are no doubt useful in guiding research, but they cannot be the major basis for judging the economic significance or reliability of the results and cannot be a substitute for a thorough examination of the quality of the data used" (1988b, 232, n. 11). "Low standard errors of estimates, high t values and the like are often attributes to the ingenuity and tenacity of the statistician rather than reliable evidence of the ability of the regression to predict data not used in constructing it. . . . In the course of decades [my] scepticism has been justified time and time again" (Friedman and Schwartz 1991, 49). These judgments were not original to Friedman; it would be equally appropriate to describe them as an elaboration of the Keynes critique or, indeed, as part of the Keynes-Tinbergen-Friedman-Phillips critique.

The Keynes–Friedman Critique

Keynes noted that "the inductive verification of the adherents of the [quantity] theory have been, I think, nearly as fallacious as those of its opponents." Tinbergen's inclusion of a trend term was close to being "a
method for correcting imperfect results and obscuring the fact that the explanation given is the wrong one." Superimposed on all of these problems is the "frightful inadequacy of most of the statistics employed." Keynes also highlighted what would later be called the model selection problem (CW 12:765; 1939, 567; CW 14:287; 1940, 155–56). Statistical tests cannot prove a theory to be correct or incorrect; the latter requires the theory's proponents to accept that all the test's auxiliary conditions are neutral with respect to the refutation. The "fiction" that econometricians can test the relationships provided by economic theory is retained only for the consumption of undergraduates (Pesaran and Smith 1985, 145, 148, 139).

According to Roy Weintraub (1983, 18), in the 1930s there were two centers of formalist work in the United States: the Cowles Commission and Paul Samuelson (later joined by Solow). Samuelson observed that "by 1935 economics entered into a mathematical epoch. It became easier for a camel to pass through the eye of a needle than for a non-mathematical genius to enter into the pantheon of original theorist. A kind of Gresham's Law operated as those of us who benefited from it know only too well" (1976, 25). Keynes—-with his "tremendous capacity for mastery of detail" (Robinson 1972, 534)—was concerned, in this context, that economists might "lose sight of the complexities and interdependencies of the real world in a maze of pretentious and unhelpful symbols." He cautioned against the "pitfalls of a pseudo-mathematical method" (Keynes 1936a, 298; see also 305). He warned Sidney Alexander "against the insidious disease of mathematics" (Samuelson 1977, 73). And he wrote mockingly about "those who feel a special confidence in a proposition which is expressed algebraically" (CW 11:380–81). Samuelson (1946, 197) traced this animosity back to A Treatise on Probability. Economists were already too prone to "specious precision" (cited in Skidelsky 1992, 540). Attempts to turn economics into a "pseudo-natural science" would be counterproductive with respect to the training of economists: "The pseudo-analogy with the physical sciences leads directly counter to the habit of much which is most important for an economist proper to acquire." In his obituary of Marshall, Keynes emphasized that "the master-economist must possess a rare combination of gifts. He must reach a high standard in several different directions and must combine talents not often found together. He must be a mathematician, historian, statesman, and philosopher—in some degree. He must understand symbols and speak in words" (Keynes 1924, 321).
W. S. Jevons brooded over his charts “to discover their secrets. It is remarkable, looking back, how few followers and imitators he had in the black arts of inductive economics in the fifty years after 1862. But today he can certainly claim an unnumbered progeny, though the scientific flair which can safely read the shifting sands of economic statistics is no more common than it was” (1936b, 524).

This was the essence of Keynes’s concern about the practices and habits likely to be acquired through the mechanical practice of econometrics: “The question to be answered, however, is whether the complicated method . . . employed [by Tinbergen] does not result in a false precision beyond what either the method or the statistics actually available can support. It may be that a more rough and ready method which preserves the original data in a more recognisable form may be safer” (CW 14:289). “The truth is that sensible investigators only employ the correlation coefficient to test or confirm conclusions at which they have arrived on other grounds. But that does not validate the crude way in which the argument is sometimes presented, or prevent it from misleading the wary,—since not all investigators are sensible” (CW 8:466; see also CW 14:296–97, 300). Thus, mechanical econometric procedures, Keynes thought, would “displace insight and intuition and confine the scope of economics” (Pesaran and Smith 1985, 146). Unlike intuition, they could not offer a privileged description of economic reality (Keynes cited in Robinson 1972, 536; cited in Dyson 1979, 56–57).

Friedman’s work sometimes exactly echoed Keynes’s. For example: Friedman highlighted the objections to the Cowles Commission approach, based on “the choice of ‘model’ in their terminology . . . the choice of a ‘structure’ . . . [and] the so-called ‘identification’ problem.” Like Keynes (CW 14:287), he also discussed “trial and error” specifications searches and how they invalidate classical statistical inference procedures: “Tinbergen’s results cannot be judged by ordinary tests of statistical significance . . . [His variables] have been selected after an extensive process of trial and error because they yield high coefficients of correlation.” Friedman objected to the use of trend terms, which were “highly questionable on statistical grounds.” He demonstrated that Tinbergen’s coefficients were too specific to the data that had been examined, and did not agree with other data. Friedman was alarmed by the “excessively crude” data. He questioned the validity of drawing meaningful interpretations from Tinbergen’s results: “The methods used by
Tinbergen do not and cannot provide an empirically tested explanation of business cycle movements.” Just as Keynes did, Friedman cautioned against economic theory becoming a species of “disguised mathematics . . . a retreat into purely formal or tautological analysis” (1940, 659–60; 1953, 11–12, n. 11, 77–78, n. 37; 1991).

The Market for Influence

The third perennial theme of Friedman’s writing (in addition to confidence about monetarism and doubts about econometrics) is the Smithian case for competition as an irresistible force undermining the market power of producer groups (Friedman and Kuznets 1945; Friedman 1962). Large-scale structural macroeconomic modeling was erecting (for non-Keynesians) a considerable barrier to entry into the policy marketplace (Walters 1977, 834; Friedman cited in Frazer 1988, 707). Friedman had been preoccupied with monumental scholarly work; but from the mid-1950s, he began to address a wider audience (see, for example, 1962; other evidence includes his Newsweek column, his appearances before congressional committees, and his association with U.S. presidential candidate Barry Goldwater). As if to demonstrate the “fertility of the market” and the “generally unstable” and “brief” (1962, 158, 131) nature of these barriers to entry and other anticompétitive forces, he began to engage at this time in intense competition with Keynesian macroeconometrics: “We were then trying to meet an argument on its own ground. I would never have been comfortable with the conclusions reached if the only basis for them had been the statistical correlations we were presenting. However, by 1963 the bulk of the Monetary History book had been written. I felt very confident in the evidence from history independently of the evidence from the statistical correlations, and hence regarded these as confirmatory rather than decisive evidence” (personal correspondence from Friedman, 2 November 1993).

Friedman is widely regarded as the most persuasive debater in the economics profession (Blaug 1985, 62; Stigler, cited in R. Friedman 1977, 20; Galbraith 1987, 271). This stems, in part, from his conviction that “you cannot be sure that you are right unless you understand the arguments against your views better than your opponents do” (1974, 16). His initial lack of influence has been attributed to “his early habit of extreme aggressiveness in debate” (Breit and Ransom 1971, 256, n.
57). Indeed, in an article entitled “Libertarians at Bay,” Lincoln Gordon (from Harvard University) argued that “there has emerged in recent years a new fashion of egregious rudeness among self-styled libertarians . . . the Hayek-Mises-Jewkes-Graham manner. . . . One can hardly escape the conclusion that Mr. Graham’s swimming suffers from a failure to understand which way is down” (1949, 976–78). But later, when the Keynesian tide turned, it was those who were losing policy-influence who displayed a “bitterness beyond reason” (McCloskey 1986, 184). A fair-minded observer noted that “modern econometricians may well look askance at some of [Friedman’s and Anna Schwartz’s] econometric methodology” (Goodhart 1982, 1542). But the primary structural failure was not Friedman’s lapse from best-practice structural estimation but the econometrics fraternity’s lack of a suitably trained historical subdiscipline.

**Phillips and Phillips Curve Econometrics:**
**Fresh Textual Evidence from “The One and Only True and Complete Set of the Bones of the Saints”**

Jacob Marschak had been minister of labor in the short-lived Menshevik government of the Terek Republic in the North Caucasus. His first encounter with perspicacious forecasts was a colleague’s warning that this paedocecy (a government of children) would fall “when the corn has grown high enough to conceal a man on horseback” (Koopmans 1978, xii). He became director (1943–48) of the Cowles Commission at Chicago, initially believing that structural estimation possessed a unique epistemological status: it was “the Gospel. . . . I hope we can become ‘social engineers’” (cited in Epstein 1987, 69, 61, 67; Hildreth 1986, 3–8; Malinvaud 1988, 194; Klein 1978, 326; Arrow 1978). Frisch’s description of the first European meeting of the Econometric Society at Lausanne in 1931 captured this heady enthusiasm.8 Klein (1947, 111) believed that econometric models “eventually should lead all investigators to the same conclusion”; Tinbergen believed that “dif-

8. “We the Lausanne people, were indeed so enthusiastic all of us about the new venture and eager to give and take, that we had hardly time to eat when we sat together at lunch or at dinner with our notes floating around on the table to the despair of the waiters” (Frisch 1970, 152). It was what Martin Beckman called “the heroic age of econometrics” (cited by Craver and Leijonhufvud 1987, 181).
ferences of opinion can, in principle, be localised" (Tinbergen 1937, 73; see also Samuelson 1992, 243).

At the risk of oversimplification we can describe the "econometrics movement" as an attempt to locate the general theory of macroeconomic structure—the quest for a "single final equation" (Schumpeter 1954, 1168, n. 20), or series of equations, with reliable estimated coefficients; a form of econometric fundamentalism. Combined with this pioneering confidence was a willingness to directly confront as many theoretical problems as their critics could muster. In addition, there was a deep understanding of the unsatisfactory nature of economic data. This sense of integrity gradually eroded such pioneering confidence and contributed to the "retreat from structure." In many ways, Klein (1992, 184) symbolized the ongoing faith in large-scale macroeconometric models, in opposition to the principle of parsimony.

The third "wave" of macroeconometric enthusiasm—primarily associated with the construction of Keynesian Phillips curves—was the most damaging to the prestige and scientific credibility of the economics profession. The opposition to the first wave of macroeconometric enthusiasm (associated with Tinbergen's work in the 1930s) was led primarily by Keynes. The opposition to the second wave (associated with the Cowles Commission) was led by Friedman.9 This enthusiasm did not survive what Koopmans called the "Friedman critique" (Epstein

9. In the postwar period the "economics miracle" really took off; it was as if economics became the language of government, and there was a great demand for those who spoke the language (H. Stein 1986). Samuelson described the period from 1932 to 75 as "the great wave of a Kondratieff expansion [for economists]. The New Deal and Welfare State created a vast new market for economists in government... Then came the post-war boom in education" (1988, 60–61; Desai 1981, 55; Pesaran and Smith 1985, 148). The 1940s and 1950s were the decades of enthusiasm and optimism for government planning, and this created a massive demand for advice from economists, often of a technical nature. The entire economies of Japan and West Germany were available for experimentation. The price mechanism had, it was believed, failed in the 1930s and had subsequently played little role in allocating resources during the war (at least at the governmental level). Foreign aid and the Marshall Plan were supposed to restructure the noncommunist world. A new subdiscipline, development economics, emerged, much influenced by the structuralist approach to planning (Little 1982, 76–85; Meier and Soren 1984). The emerging welfare state required a broad tax base to fund it. At the same time, taxes came to be perceived as a technical tool to be manipulated by policy makers in order to contain inflation. This confidence can be sampled in the papers of Marschak, Klein, and Edward Teller, and Marschak and Klein (the latter delivered to the Econometric Society meeting in 1946), which advocated an expenditure of twenty billion dollars per year over fifteen years to relocate all inhabitants of cities with over fifty thousand inhabitants to ribbon cities or underground cities in order to minimize the effects of an atomic assault on the United States (Epstein 1987, 81, 95, n. 8).
Disappointing empirical results led to the "retreat from structure" after 1947.\textsuperscript{10} Yet large-scale Keynesian macroeconometric models continued, often with ad hoc monetary sectors, and, following Klein and Arthur Goldberger (1955, 1), an ongoing "constant adjustment." Keynesian macroeconometricians abandoned their optimism concerning the revealing nature of structural estimation and came to rest, in part, on the \textit{judgment} of the researcher ( Tinbergen 1969, 44; Zarnowitz 1968, 427; Klein 1971a, 48; Hildreth 1986, 60; see Desai 1981, 154, for the "endogenise a bit more" approach to the pursuit of structure). The Keynesian Phillips curve macroeconometric models that collapsed in the 1970s were, together with their underlying method of research, effectively orphaned thrice: disowned by Keynes, abandoned by most of the Cowles Commission workers, and antithetical to both the spirit and the detail of Phillips's work.

Nevertheless, the third wave of econometric optimism coincided with the increased availability of computing power in the 1960s. Phillips was one of the most insightful critics of the Keynesian Phillips curve estimation industry. Like H. L. Moore (Stigler 1962), Phillips avoided controversy, but in many important respects his work does not belong in the same category as most of the macroeconometric exercises of the 1960s. First, he pioneered the role of inflationary expectations in

\textsuperscript{10} The pioneering optimism of this second wave was almost immediately confronted with skepticism. In August 1946, the Institute for Mathematical Statistics meeting in Ithaca, New York, concluded that the Cowles group approach was unlikely to result in meaningful estimated parameters. Little confidence was held out for future developments: "Data as bad as economic data" was incapable of accurately discriminating between alternative models (Tinney, cited in Epstein 1987, 100). The attempt to derive an exact model of the capitalist system was derided by Schumpeter; Irving Fischer concluded that he had seen "a lot of people burn their fingers over discoveries of cycles. The discoverer ‘sees things’ almost as bizarre as drunkards" (cited in Epstein 1987, 103). Friedman, the most persistent critic, presented to the 1947 Econometrics Society meeting a manifesto titled "A Monetary and Fiscal Framework for Economic Stability" (1948a), which offered an alternative to the short-run stabilization perspective of the Cowles workers. Koopmans asked, "Can we meet the Friedman critique: that Christ's experiments have shown that the information contained in the data so far processed have been insufficient for good forecasting" (cited in Epstein 1987, 111). Marshak retreated from his previous position with respect to the NBER survey research method. The Friedman critique, plus reviews by Kenneth Arrow, Guy Orcutt, Solow, Samuelson, Wassily Leontieff, Abraham Wald, and others, together with disappointing empirical results and an increasing awareness of the paucity of reliable data, effected a "retreat from structure" from 1947: "The empirical work was an exhausting disappointment both for the tedium of computation and the lack of professional acceptance" (Epstein 1987, 110). "The econometric approach of the Cowles Commission seems to be petering out rapidly or not getting anywhere beyond extensive methodological discussions" (Tinbergen 1949, 84).
this type of macroeconomics. Second, many of these models did not adequately deal with money, but Phillips's model and his famous machine were based on monetary dynamics. Third, Phillips was opposed to the idea of trading higher inflation rates for supposed lowered unemployment rates. Fourth, a decade before Robert Clower and Axel Leijonhufvud, Phillips was teaching Keynesian macroeconomics as a disequilibrium phenomenon (Lipsey 1981, 547). Phillips's dynamic stabilization exercise was concerned with minimizing the deviations of the business cycle "pendulum," not with attempting to locate the macroeconomy at a point other than "rest." Phillips provided the theoretical explanation behind Christopher Dow's (1967) subsequent empirical analysis of the destabilizing effects of fine tuning. His curve, however, came to be interpreted as a proposition that ongoing inflation would reduce the rate of unemployment, which Phillips had specifically cautioned against (Leeson 1997d).

The complete, recently rediscovered M^2T Seminar records capture the flavor of Phillips's influence. (The records of the M^2T Seminar series were titled by the words that head this section.) Richard Lipsey and his colleagues attempted to reconstruct economics as a series of empirically testable propositions. Arnold Harberger—who had been closely associated with the Cowles Commission during the 1950s (Hildreth 1986, 64) and who shared Friedman's views of econometrics (personal correspondence from Friedman, 18 April 1995)—attempted to persuade the M^2T economists that their project was flawed because of the "back door alibi. . . . Testing is subjective... infinite number of possible hypotheses to explain anything... Trouble in econometrics—people will agree neither on which hypotheses are the most plausible nor on what experiment would be crucial. . . . Can't convince man who won't be convinced. Have to depend on what 'seems sensible'... Scientist builds up picture of the world. The more open to surprise the better. When surprised he amends picture. World complicated, need intuition." Lipsey explained that "we want to be formal because we associate with people whose intuition we don't like and Harberger doesn't. Why does Harberger wince whenever Archibald says 'rule'?" Harberger replied: "Because these matters are always subjective among the inquirers" (M^2T Seminar notes, February and March 1958).11

11. The Cowles econometricians were perceived to be seeking a "social analogue for Newtonian mechanics... Tycho and Kepler are becoming fairly regular attenders of economic discussions these days" (Vining 1949, 80, 77). This analogy was also prevalent among the
Phillips presented the paper "The Problem of Refutation," which apparently has not survived, on 27 April 1960 and 18 May 1960. He argued that static theory could not be tested from time-series data. Because of unstated maintained hypotheses, categorical statistical refutation was impossible. Autocorrelated time-series were treacherous data. "Testing" was, in reality, little more than "measurement plus" (M&T Seminar notes). It seems that Phillips was influential in the retreat from the "Popperian notion of refutation," which Lipsey mentioned in the second edition of his Introduction to Positive Economics (1966, xx).

J. C. R. Rowley and D. A. Wilton (1973, 385, 387) reestimated various Phillips curves using generalized least squares, concluding that the "pseudo" t-values had been inflated by at least 100 percent in most cases: "One can only speculate whether the various authors would have advanced the Phillips curve model had they been faced with the GLS estimates rather than the OLS estimates." Most of these models from the 1960s and early 1970s had been plagued by the unacknowledged problem of autocorrelation. Yet in the discussion following his paper

LSE econometricians. In September 1608, a trader at the annual Frankfurt Fair offered for sale a telescope that could magnify seven times. In March 1610, Galileo published his first booklet, a short but dramatic work called Sidereus Nuncius, or Messenger from the Stars. The universe would never appear to be the same again. The walled-in Aristotelian universe, with its immutable social order, would be destroyed by a seventeenth-century retreat to the heliocentric perceptions of Aristarchus (Koestler 1959, 43–63; Butterfield 1957, 55–76). Exactly three and a half centuries later, Phillips (November 1958) and Lipsey (February 1960) turned the newly refined econometric telescope onto the problem of the behavior of money wages during the business cycle. With pioneering optimism the M&T economists sought to use this telescope to turn economics into a fully empirical science. There was also an Aristotelian authority to be vanquished. In the first edition of his textbook, Lipsey cited Lionel Robbins on empirical analysis: "But is it not desirable to transcend such limitations? Ought we not to be in a position to give numerical values to the scales of valuation, to establish qualitative laws of supply and demand? . . . No doubt such knowledge would be useful. But a moment's reflection should make it plain that we are here entering into a field of investigation where there is no reason to suppose that uniformities are to be discovered. . . . Is it possibly reasonable to suppose that coefficients derived from the observations of a particular housing market at a particular time and place have any permanent significance—save as Economic History?" Lipsey bemoaned that these views were "still held by economists" (1966, 219, 218, n. 1). In a seminar titled "Refutation and Comparison," Kurt Klappholz also mentioned Robbins. Chris Archibald retorted: "Robbins Aristotelian, not relevant" (M&T Seminar notes, 7 March 1958). In Lipsey's best-selling textbook, further evidence of the importance of this historical analogy is provided by the opening extract from Beveridge and the final sentence (1966, xi–xii, 860–61). In seeking to rigorously scrutinize economic data they were aspiring to the highest standards of science. They hoped to resolve conflicts over perceptions and policies and to effect a Newtonian-style revolution in economics.
"The Problem of Refutation," Phillips emphasized that with respect to data analysis he was only "happy if not autocorrelated" (MPT Seminar notes, 18 May 1960). The Keynes-Tinbergen-Friedman-Phillips critique is, therefore, an appropriate label for these doubts about the macroeconometric practices that culminated in the Keynesian Phillips curve.12

**Concluding Remarks**

The history of econometrics is worthy of greater attention among practicing econometricians; the subject should stand in equal status with other subdisciplines within econometrics. When studying Keynes's critique of econometrics, a disaggregated approach reveals his objections to the underlying logic, and pretensions, of this relatively new tool for the analysis of economic data. His contemporaries were in no doubt as to the intensity of his hostility (Klein 1951, 450–51). Keynes did not soften his position; in fact, Tinbergen came increasingly to recognize the validity of some of Keynes's criticisms.

There is a large degree of similarity between Keynes's position on econometrics and that of Friedman. The econometric disputes between Keynesians and monetarists that raged in (and perhaps disfigured) the profession were, from a methodological perspective, a sometimes ill-tempered conversation between Keynesians and the modern represen-

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12. Econometric agnosticism, or at least reservations about policy relevance, remained a minority taste in the 1960s, with potentially explosive critiques such as Phillips's (1966) being almost entirely ignored. The exchange between Robert Basmann and Klein and associates (in Breuer 1972) reflected the determination of practitioners to press on almost regardless. Unorthodox and problematic ideas tended to be ignored because they "would have inconveniently impeded the progress of econometrics at the time of its most rapid growth" (Desai 1981, 116–17; see also 120). In the age of the computer and in the presence of an increasing demand for financially lucrative expert consultancy from government agencies, applied econometrics acquired an ad hoc character that was often cut adrift from professional disquiet. The statistical economists believed that potential regularities and relationships could be revealed by an interaction with the data. Econometricians believed that the data would "speak" when a model had been imposed upon it. Tinbergen was also very knowledgeable about his data and was concerned about its quality. Econometrics gradually entered a less creative, more mechanical phase, where concern about the quality of the data were less prominent. Frisch and his coworkers were aware of the possibilities of deriving "fictitious" results from econometric analysis (Epstein 1987, 91). Koopmans persistently, if vainly, emphasized the need to report all results, not just the preferred set. Coal-faccd enthusiasm for model estimation appeared to be largely oblivious to the skepticism and concern expressed by some about the lack of model evaluation.
tative of Keynes. Keynes's opposition to macroeconometrics was based on his suspicion that the results would come (illegitimately) to be regarded as decisive evidence. This was also Friedman's suspicion. Likewise, Phillips delivered a good deal of insight into the fundamental weaknesses of the Phillips curve estimation industry.

Gardiner Ackley (1961, 109) argued that historical misrepresentations (with respect to the myth of the "Classical" whipping boys) could be analytically truthful. But analytically, Keynesian macroeconometrics left behind some "jerry built structures" (Lucas 1977)—most notably the tradeoff interpretation of the Phillips curve—although Phillips (1968) developed parts of the critique that was subsequently named after Robert Lucas (Court 1998). Friedman, Marschak, and Tinbergen have been credited with a similar approach to econometric policy evaluation (Lucas 1976, 20; Pagan 1987, 20). Thus, the collapse of the Keynesian Phillips curves in the 1970s was a vindication of both Keynes-Tinbergen-Friedman-Phillips and the Friedman-Marschak-Tinbergen-Phillips critiques.

In his final, posthumously published article, Keynes bemoaned how much "modernist stuff, gone wrong and turned sour and silly is circulating" (1946, 177). Much of his critique of econometrics retains its validity with respect to contemporary practices.19 Yet the tradition of "faucier econometric footwork" (Lucas 1976, 257) continues, often oblivious to some of the issues that alarmed Keynes, Friedman, Tinbergen, Phillips, et al.

Stigler (1963, 63) suggested that "methodological controversy has never had a marginal product (of scientific progress) above zero"; this seems to capture Friedman's sentiments exactly. Friedman echoed Marshall's description of theory as "a 'language', designed to promote 'systematic and organised methods of reasoning'". Mitchell's style of research had fallen out of favor, in part, for reasons of "language rather than substance...and he uses no mathematics...[but] his theoretical discussion can readily be translated into current jargon" (Friedman 1953, 7; 1950, 489). Max Weber noted the tendency for intellectual

13. Don Patinkin found it "somewhat depressing to see how many of [Keynes's critics] are, in practice, still of relevance today" (1976, 1095). Maurice Allais, a theoretical physicist, in addition to being the recipient of the 1988 Nobel Prize in economic science, bemoaned "the crop of pseudo-theories based on the mechanical application, devoid of any real intelligence, of econometrics and statistical techniques... pseudo-models, accompanied by a mathematico-statistical panoply of untamed, totally unjustified economics which seem to the naive to be scientific theories, whereas they are generally just empty shells" (1992, 35).
opponents to avoid “the other’s terminology as though it were his toothbrush” (cited in Haberler 1961, 40). Samuelson’s formalist work, *Foundations of Economic Analysis* (1947), included on its title page the statement “Mathematics is a Language.” Keynesian methods overran NBER statistical business cycle research, but Friedman’s polemical genius led him to use Keynesian language (IS-LM, income-expenditure, money demand, and econometrics) to effect a remarkable, if temporary, counterrevolution. Clearly, econometrics has a subterranean history of which too many econometricians are unaware.

I do not wish to be misunderstood. As David Hendry (1980, 395) put it, “Some editors can be persuaded to publish on the basis of econometric fools-gold: *caveat emptor*, but do not denigrate the whole project.” Econometric evidence has illuminated many debates and clarified some issues. It was, for example, careful empirical work which revealed that the consumption-income ratio appeared to be constant over long periods (Kuznets 1942, Goldsmith 1955), in contrast to the simple linear Keynesian consumption function (Keynes 1936a, chapter 8; Davis 1952). Yet the large macroeconometric forecasting models have not drastically improved in predictive accuracy in the last three decades (Hendry 1980, 388; Leamer 1983, 42; Pagan 1987, 3–4; Epstein 1987, 4; Rivlin 1987, 2; Zarnowitz 1992; Ormerod 1994, 3). Neither can new classical macroeconometrics claim a greater degree of academic respectability than the Phillips curve equations that preoccupied applied econometricians in the 1960s. Monetary targeting, often based on applied econometric research, was also a disappointment. In addition, Friedman’s forecasts of a surge in U.S. inflation, beginning in mid-1984, and of a growth in real GNP of just 1 percent for 1984:Q1 both proved to be inaccurate (Gordon 1987, 441). He also made an unfortunate prediction that “the world crude oil price cannot stay at $10$ a barrel; it will drop drastically in the next six or nine months” (Friedman 1974, 12). Likewise, Granger-Sims-style tests of exogeneity of the money supply have yielded mixed results (Cagan 1989).

Macroeconometric modeling, however, remains a lucrative business (Tobin 1977, 760; Galbraith 1987, 261-62). Frisch spoke of the “service to the econometrics fraternity by being critical and outspoken” (1970, 152). Clive Granger has also appealed to model builders to pay more attention to econometric theory: “One wonders what has been the purpose of the work of the majority of theoretical econometricians for the last twenty years, or of a third of the pages of *Econometrica*” (1981,
124). David Hendry stated in his inaugural lecture that Keynes's critique should be "compulsory reading" for econometricians (1980, 396). If applied econometricians paid as much attention to the history of their subject as they do to running regressions, this might improve the quality and reliability of the empirical side of our profession.

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