

LOST CAUSES

BY

KEVIN D. HOOVER

Often papers begin with an idea. Once the paper is written, sometimes little, sometimes much effort goes into finding a title. This lecture worked the other way round. It started with the title, which I passed on to Roy Weintraub, my successor as President of the Society, when I still had but the vaguest idea of what I would write. When Roy heard my title he pointed me to a passage from C. Vann Woodward, that he had himself quoted in *Stablizing Dynamics*:

Lost causes, especially those that foster loyalties and nostalgic memories are among the most prolific breeders of historiography. If survivors deem the cause not wholly lost and perhaps in some measure retrievable, the search of the past becomes more frantic and the books about it more numerous. Blame must be fixed, villains found, heroes celebrated, old quarrels settled, old dreams restored, and motives vindicated. Amid the ruins controversy thrives and books proliferate (quoted by Weintraub 1991, p. 125, from Vann Woodward 1986).

Like Vann Woodward, I am a son of the South. With a title like “Lost Causes” one might imagine that, standing in Durham, North Carolina, the talk has something to do with the faded glory of the Confederacy. But no. The passage comes from Woodward’s essay on the New York socialist, Irving Howe. And, despite the fact that I was once a Civil War buff who would instinctively choose the gray over the blue, I am no Trent Lott, and I willingly concede that the right side won. My fellow Balliol man, Matthew Arnold, once referred to Oxford as “the home of lost causes.” Yet, I do not want to argue for royalism or Anglo-Catholicism or any of the causes that exercised the passions of Oxford men. There may well be any of number of lost causes implicit in my talk, but I wish to mention only two explicitly.

I. INTERNAL HISTORY

Most of my lecture will be devoted to an historiographic problem—the history of causal language in econometrics—but let me begin with something more in the heroic vein that Woodward contemplated. My account of the history of

Department of Economics, University of California-Davis, Davis, CA 95616-8578. This lecture was delivered as my presidential address to the History of Economics Society on July 6, 2003 at the annual History of Economics Society conference held at Duke University, Durham, North Carolina. I thank Ryan Brady for his research assistance.

ISSN 1042-7716 print; ISSN 1469-9656 online/04/020149-16 © 2004 The History of Economics Society
DOI: 10.1080/1042771042000219000

causal language is unabashedly internalist. The tide of History is clearly against internalist history. It is passé in the history of science. Historians now prefer a history in which the wider social context—the interpretive communities that give meaning to mere chronologies—are given center stage. The history of economics is regarded as somewhat of a backwater—in part because it concentrates on the chronology and exegesis of economic analysis, in part because it is often produced by historians who retain their identity as economists, and in part because it cares about its product in relationship to the larger discipline of economics rather than as a fully distinct History of Economics. The history of other sciences has been largely professionalized and has seceded into science studies. And the history of economics is moving in that direction. In the vanguard of this movement, Roy Weintraub (2001) has stigmatized internalist history, quoting Bruno Latour (1988, p. 218): “internal history . . . does not count as history at all. At best it is court historiography, at worst the Legends of the Saints.”

I want to defend internalist history. To defend it is by no means to attack the “thick” approaches to history advocated by Weintraub and many others. Indeed, good thick history is highly valuable. But it is not the only way. For me, the key element in history is the “story.” As all the world knows, there are many interesting ways to tell a story. We do not have to choose one. What distinguishes history from typical storytelling is that it trades in *true* stories. That is not to say that it does not interpret. Every creative writing teacher knows that *every* story has to have a point of view. But that is, in itself, no barrier to telling the truth. There is less irony than usually intended by those who cite the quip: “what we need is an objective history of the Civil War from the southern point of view.” The great social historian, Lawrence Stone, wrote: “The slightest tampering with archival facts is the one unforgivable sin of history” (Stone undated). History’s first virtue is Truth.

History’s second virtue is Interest. The historian A. J. P. Taylor (quoted by Boot 1995) once remarked: “A lot of historians forget that one purpose at any rate of writing a book is that someone should read it.” Interesting true stories can be told in many ways, for many purposes. If what many historians of economics write is court historiography, that is in large part because it is interesting to the courtiers.

Court historiography may yet have broader value. At the *History of Political Economy* Conference held April 2003, Roy Weintraub said something to the effect that models are objects, and objects do not have histories.¹ History belongs to those who create, interpret, and use the models, not to the models themselves. I am not so sure. When I was a boy, my parents had a book published by the U.S. Navy on the history of the machine gun—my aunt had been a Navy clerk who worked on the project. This book was as internalist a piece of historiography as ever there was, interesting mostly to those in the military court or to the audience of the History Channel’s *Tales of the Gun*. Yet, I think it is easy to imagine that the thick historian of the nineteenth or twentieth century might

¹“The IS-LM Model: Its Rise, Fall, and Strange Persistence,” History of Political Economy Conference organized by Michel De Vroey and Kevin D. Hoover at Duke University, Durham, North Carolina, April 25–27, 2003.

wish to consult such a book for a relevant fact with the end of telling a true story. We have all seen newspapers, textbook writers, and economists mistake or distort facts about the court historiography of economics. Sometimes these errors are important, producing misleading conclusions or misdirecting passions. In all cases, they are unaesthetic. The history of the gun is interesting to gunners. And it may be important to a wider world whenever guns matter to a wider outcome. Equally so with the internal history of economics.

II. WHERE HAVE ALL THE CAUSES GONE

Let me now turn from one lost cause to a story of causes more literally lost. The story begins with my asking my colleague, Colin Cameron, a microeconometrician, how he would distinguish econometrics from statistics applied to economic problems. He answered that econometrics was interested in establishing causation, while statistics was content with correlation. I filed this away as an interesting, if idiosyncratic, definition of econometrics until I came across a paper by James Heckman, who won the Nobel prize for his contributions to microeconometrics. In a paper subtitled “A Twentieth Century Retrospective” Heckman wrote:

Most econometric theory adapts methods originally developed in statistics. The major exception to this rule is the econometric analysis of the identification problem and the companion analyses of structural equations, causality, and economic policy evaluation (Heckman 2000, p. 45, emphasis added).

The major contributions of twentieth century econometrics to knowledge were the definition of causal parameters ... the analysis of what is required to recover causal parameters from data ... and clarification of the role of causal parameters in policy evaluation (Heckman 2000, p. 45, abstract, emphasis added).

Other colleagues in microeconomic fields confirmed that they too saw the pursuit of causal knowledge as the distinguishing difference between econometrics and statistics. But I was baffled. As a historical claim, putting causality forward as the essential defining feature of econometrics seemed unwarranted. The Econometric Society, which, more than anything else, established the word “econometrics” among economists, saw econometrics as “economic theory in its relation to statistics and mathematics” and its object as the “unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems” (cited by Frisch 1933, p. 1). While the early history of econometrics (documented in Mary Morgan’s (1990) splendid history and in David Hendry and Morgan’s (1995) anthology of foundational works in econometrics) is chock full of causal discussions, my impression was that causal talk had died away in the post World War II period, so that Heckman’s view of causal analysis as *the* major contribution was a considerable projection of the present into the past—Whig history perhaps—and that most likely Heckman’s understanding of econometrics represented a causal revival.

Being above all an empiricist, I began a bibliometric investigation, taking advantage of the JSTOR archive of economics journals with its facility to search complete texts. Figure 1 shows the number of articles that use words in a *causal*

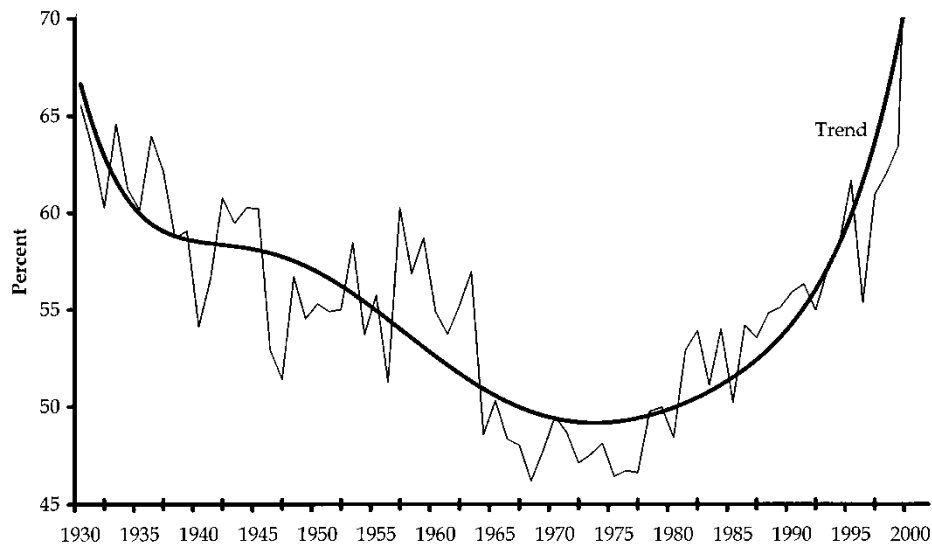


Figure 1. Lost Causes in Economics

Notes: Figure shows articles using the causal family as a fraction of all articles in the JSTOR archive of economics journals, 1930–2001. *Causal Family* = “cause,” “causes,” “causal,” “causally,” “causality,” or “causation.” Searches conducted June 2003. Heavy, smooth line is a sixth-degree polynomial trend.

family (“cause,” “causes,” “causal,” “causally,” “causation,” or “causality”) as a fraction of all articles. The heavy line in the figure is a polynomial trend that smooths out some of the year-to-year variability of the time-series. Notice that the data confirm my conjecture that the postwar period is characterized by a dip of about twenty percent in the occurrence of the causal family from the 1950s plateau in the trend to the low point in the early to mid-1970s. (The drop from 1930 to the low point of the unsmoothed data is a third.) Also notice how high a fraction of articles use the causal family. This should not be too surprising, because “cause” and “causes” are pretty common English words, not always used with metaphysical or methodological baggage. But that makes the valley even more striking.

I wondered how much of the change in usage was attributable to the rise of econometrics itself. Figure 2 documents that rise: it shows the fraction of all articles that belong to the *econometrics family*. The definition is broad, counting any article that uses the words “econometric,” “regression,” “structural model,” or their plurals, “estimate,” or “estimation.” The rise is dramatic and the peak is an amazingly high proportion of all articles (seventy-six percent). Economics may be dominated intellectually by theory, but economics journals are dominated practically by articles developing or applying econometric methods. Figure 3 shows the fraction of econometrics articles that refer to the causal family. The dip in the use of causal language is even more pronounced among econometrics articles.

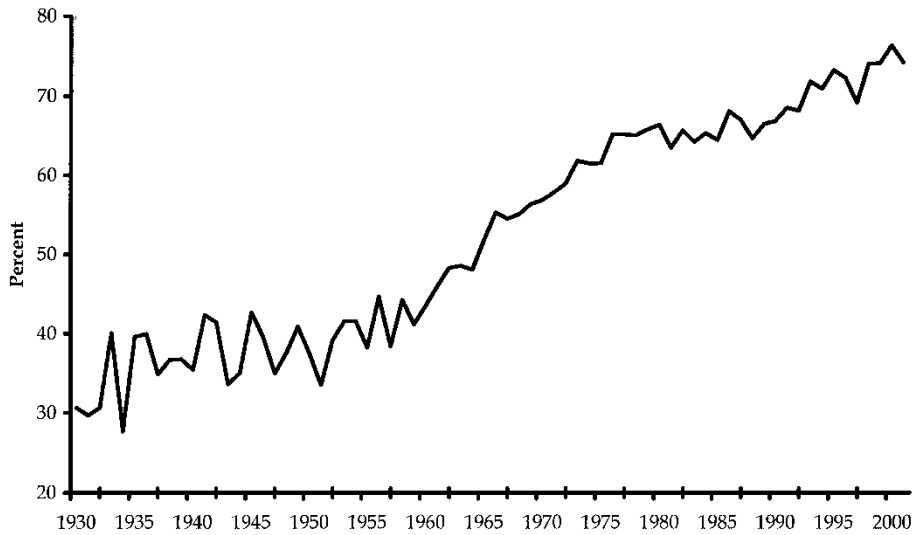


Figure 2. The Rise of Econometrics

Notes: Figure shows articles using the econometrics family as a fraction of all articles in the JSTOR archive of economics journals, 1930–2001. *Econometrics Family* = “econometric(s),” “regression(s),” “structural model(s),” “estimate,” or “estimation.” Searches conducted June 2003.

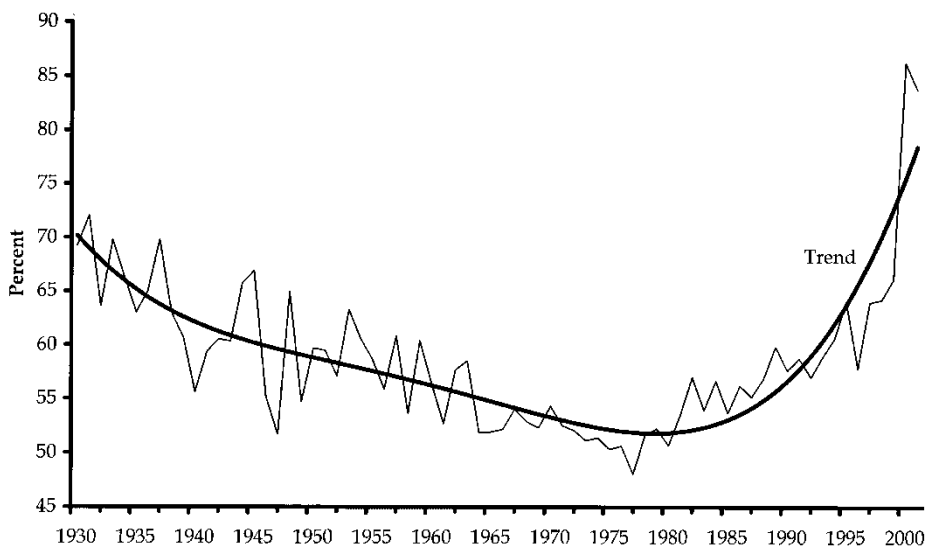


Figure 3. Lost Causes in Econometrics

Notes: Figure shows fraction of articles using the econometrics family *and* the causal family as proportion of the econometrics family. Sources and definitions: see notes to Figures 1 and 2. Heavy, smooth line is a sixth-degree polynomial trend.

The historiographic problem is to account for the lost causes evident in these time series. I will draw on two types of evidence: bibliographic evidence of the sort that I have already employed and the econometrics textbooks of the 1950s to the 1990s. I will canvass three possible explanations. While none provides the whole story, there is something to be learned from each. In the end, I will offer a fourth, synthetic explanation that I think gets us some way to understanding this episode. Nevertheless, I would claim neither originality for my explanation, nor that it is the final word. If I have set a good historiographic puzzle, I will be happy.

III. PROCESS ANALYSIS: DEFLATED AND REFLATED

In a fine essay entitled “The Stamping Out of Process Analysis,” Mary Morgan (1991) described two visions of econometrics that competed for the hearts and minds of the economics profession from the 1940s to the early 1960s. The first, which originated in the work of the Swedish theoretical macroeconomists of the prewar period, is represented in the econometrics of Ragnar Frisch, Jan Tinbergen (a Dutchman), and Hermann Wold. Economic relations are conceived of as *dynamic* processes in which variables are dated and change in response to various economic forces in real time. Wold, in particular, argued for a causal interpretation in which *post hoc ergo propter hoc* defined the admissible set of potential causes if not causes themselves. The other vision also originated with a Scandinavian, Trygve Haavelmo (1944), and was developed by the Cowles Commission (1950, 1953). The Cowles Commission embraced a Walrasian vision of economic theory and developed the econometric techniques needed to estimate the parameters of general equilibrium models. Morgan characterizes the Cowles Commission vision as static equilibrium analysis in contrast to the dynamic process analysis of Wold and company.

Morgan’s thesis is that process analysis was stamped out, first, because, practically, it was conceived with individual actors (causal decision makers) in mind. Aggregated data introduced a theoretically spurious, but practically ineliminable, simultaneity that was better dealt with using Cowles Commission methods. Second, Walrasian economics dominated economic theory intellectually and, to the degree that econometrics was about quantifying theory, came to dominate econometrics. Third, the Cowles Commission was technically successful in developing methods that could cope with simultaneity, so that there were no barriers to implementation. Finally, Basman (1965) delivered the *coup d’grace* when he showed that every recursive model of the type that Wold insisted upon was observationally equivalent to a simultaneous model. Data could not choose between the two visions.

Morgan’s story is right. If process analysis were a character in a cartoon, by 1965 the Cowles Commission steamroller had just passed over and flattened it paper thin. Yet, no sooner had the steamroller passed, than process analysis—in the great tradition of cartoon figures—popped back into shape. Figure 4 documents the stamping-out of process analysis. The lower left series is the fraction of econometrics articles that mention both Wold and the causal family: that is, the fraction that place Wold in a causal context. High in the 1950s, by

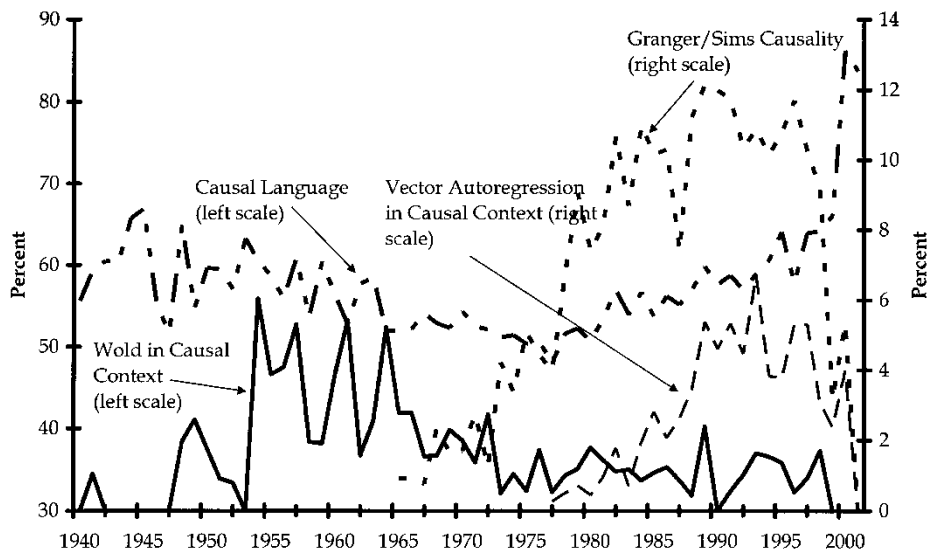


Figure 4. The Fall and Rise of Process Analysis

Notes: Each series based on counts of articles in the JSTOR archive of economics journals, 1940–2001. *Causal Language* shows articles in the econometric family and causal family as a fraction of the econometrics family. Other series show articles described by each keyword as a fraction of articles in the econometrics family and causal family. Keywords: *Granger/Sims Causality* = (“Granger” or “Sims”) and causal family and econometrics family. *Vector Autoregression in Causal Context* = “vector autoregression(s)” and causal family and econometrics family. *Wold in Causal Context* = “Wold” and causal family and econometrics family. Other sources and definitions: see Figure 1 and 2.

the mid-1960s his stock has clearly fallen. Near the end of her article, Morgan (1991, p. 258) suggests that the recent popularity of the vector autoregression (VAR) might represent a revival of process analysis. In addition to the VAR, I would add Granger-causality tests popularized by Christopher Sims (Granger 1969; Sims 1972). Granger-causality depends on exactly the one-way, time-ordered conception of causality that Wold championed. The two lower right-hand series in Figure 4 document the rise of Granger-causality tests and of VARs. A comparison of the sweep of the lower three series as a measure of the stamping out and revival of process analysis to the upper line as a measure of the use of causal language in econometrics shows that the fall and rise of causal language mirrors the fall and rise of process analysis.

The textbooks reinforce this explanation. Recent general econometrics textbooks rarely mention causality at all. But specialized time-series books generally do, albeit almost always as Granger-causality. For example, Granger-causality alone is discussed in Hamilton’s (1994, pp. 305–309) text, although he implies that it does not adequately capture “the standard meaning of ‘causality’” or “true causation.” He does not further articulate that standard or true meaning.

Similarly, while Hendry's (1995, pp. 175–77) text acknowledges that Granger and Simon offer different concepts of causality, the word “cause” in an operational context is nearly always in conjunction with “Granger.”

Unfortunately, the history of process analysis does not eliminate the puzzle. First, the uses of causal terms associated with Wold, Granger, Sims, or VARs are too small to account for the whole episode of causes lost and causes regained. Second, there was a competing analysis of causality within the Cowles Commission tradition. Herbert Simon (1952, 1953) provided a fairly complete analysis of causality in systems of equations that did not rely on time order and integrated completely with the Cowles-Commission program. His conception of causality competed head to head with Wold's (Simon 1955, Wold 1955). The stamping out of process analysis, therefore, did not in itself necessitate a stamping out of causal talk. Yet, this alternative causal analysis was also eclipsed: Figure 5 shows that the decline in the fraction of econometrics articles referring to Simon or the Cowles Commission in a causal context occurs simultaneously with the stamping-out phase of the history of process analysis. The stamping out of process analysis does not explain enough.

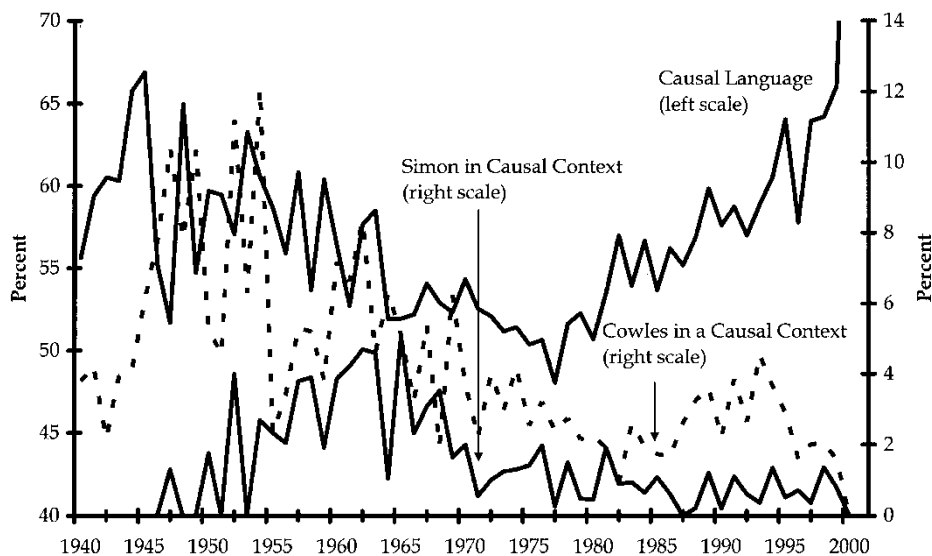


Figure 5. The Fall of Cowles-Commission Causal Analysis

Notes: Each series based on counts of articles in the JSTOR archive of economics journals, 1940–2001. *Causal Language* shows articles in the econometrics family and causal family as a fraction of the econometrics family. Other series show articles described by each keyword as a fraction of articles in the econometrics family and causal family. Keywords: *Cowles Commission in a Causal Context* = “Cowles” and causal family and econometric family. *Simon in Causal Context* = (“Herbert A. Simon” or “Herbert Simon” or “H. Simon” or “H.A. Simon”) and the causal family and the econometrics family. Other sources and definitions: see Figure 1, and 2.

IV. THE RISE OF FRIEDMAN'S POSITIVE ECONOMICS

A second explanation for the lost causes blames it all on Milton Friedman. Friedman published "The Methodology of Positive Economics" in 1953. He argued that economic models should be judged by their ability to rationalize data (particularly to make accurate predictions) rather than by the accuracy of their underlying assumptions as photographic representations of the economy (Friedman 1953). One way to read Friedman is as a child of Hume. Hume (1777, p. 165) famously wanted to cast all works involving metaphysics into the flames. Causal power for Hume is a quintessentially metaphysical notion. Hume denies that we have any access to knowledge about causal powers and that causal statements are really about constant conjunction and time-ordering. Bertrand Russell (1918, p. 180) took up Hume's position condemning the notion of causality as "a relic of a bygone age, surviving like the monarchy, only because it is erroneously supposed to do no harm." Functional relationships among variables, in Russell's view, were an adequate replacement for whatever work causality had been thought to do. Whether or not he himself would acknowledge it, Russell was regarded as a grandfather of logical positivism, and Friedman has been regarded as having spread that seed into economics. The argument, then, is that the collapse of causal language mirrors the rise of Friedman's positivism. This view gains plausibility from Friedman's strong aversion to using the word "cause," amply documented in *Theory and Measurement: Causality Issues in Milton Friedman's Monetary Economics* by J. Daniel Hammond (1996).

Figure 6 plots the fraction of causal econometrics articles in which "Friedman" and either "positivism" or "as if" also occurs. The pattern is clearly inverse to the lost causes.

There is, I think, something in attributing the lost causes to the rise of positivist rhetoric in economics and in attributing that rise to Friedman's essay as it was received and interpreted by economists. It would nonetheless be wrong to take Friedman himself as representative of the Russellian view. I say "Russellian" rather than "Humean," because there are two Humes. Hume the philosopher was skeptical of causes. But Hume the economist wrote: "it is of consequence to know the principle whence any phenomenon arises, and to distinguish between a cause and a concomitant effect . . . nothing can be of more use than to improve, by practice, the method of reasoning on these subjects" (Hume 1742, p. 304).

Likewise, when Friedman *practices* economics he is not content to stick to the surfaces. Friedman and Schwartz write in the *Monetary History*, "In monetary matters, appearances are deceiving; the important relationships are often precisely, the reverse of those that strike the eye" (Friedman and Schwartz 1963a, p. 676).

My colleague, Tom Mayer (1993), as well as Hammond (1996), has pointed out that Friedman's 1953 essay must be understood not as a work of philosophy, but as the reflections of an economist. Hammond and I (Hoover 1984, chapter 9) have stressed that Friedman's methodological vision is captured less in positivism than in his interpretation of Marshall's methodology, in which economics is, according to Friedman, quoting Marshall, "an engine for the discovery of concrete truth" as well as "substantive hypotheses, based on factual evidence

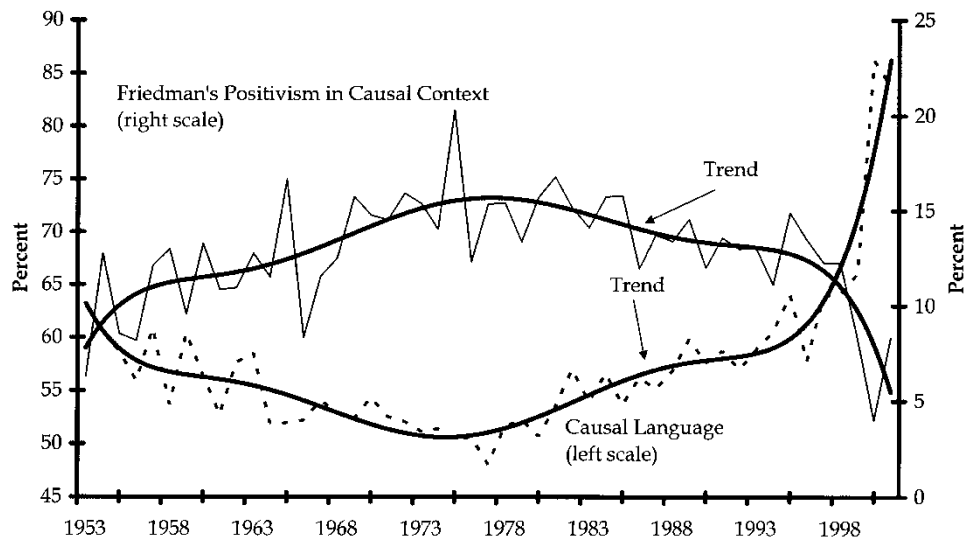


Figure 6. Does Friedman's Positivism Account for the Lost Causes?

Notes: Each series based on counts of articles in the JSTOR archive of economics journals, 1953–2001. *Causal Language* shows articles in the econometric family and causal family as a fraction of the econometrics family. *Friedman's Positivism in a Causal Context* = “Friedman” and (“positive economics” or “as if”) and causal family and econometric family expressed as a fraction of articles using the econometrics family and causal family. Other sources and definitions: see Figure 1, and 2. Heavy, smooth lines are sixth-degree polynomial trends.

about the ‘manner of the action of causes’” (Friedman 1949, p. 490—quoting Marshall 1885, pp. 159, 171). Causes lie at the very root of Friedman's methodology, underlining Hammond's flawless judgment in making them central to his methodological history of Friedman's monetary economics.

But why then is Friedman so reluctant to speak of causes? He argues that any cause that we isolate is never the whole cause and that every direct cause itself has its own direct causes, so that networks of causation spread synchronically across the economy and diachronically back into the mists of time: causal talk appears to be the enemy of the Marshallian strategy of isolation. Although Friedman rarely uses the word “cause” unless forced to by a critic such as Tobin (1970; see Friedman's 1970 reply), he finds it hard to avoid synonyms. Friedman and Schwartz (1963a, b) frequently use words that carry substantial causal baggage: “affect,” “influence,” “response,” “prime mover,” among others.

Friedman's avoidance of explicit causal language appears, then, to be almost a personal quirk, rather than a genuine methodological commitment. This reading is, I think, borne out by Alan Walters's (1970) econometrics textbook. While not mentioning Friedman in the textbook, Sir Alan (later the avatar of monetarism in Britain and an economic adviser to Mrs. Thatcher) draws a sharp contrast, grounded in Friedman's 1953 essay, between “assumptionists” and

“predictionists.” At the same time, Walters’s book is one of the two most explicitly and extensively causal of the postwar textbooks, as well as the most Marshallian in Friedman’s sense of that term.

V. EQUAL SIGNS HAVE NO POINT

The computer scientist and causal analyst Judea Pearl (2000, p. 349) provides a hint of a third explanation. He writes, “Early econometricians were very careful mathematicians; they fought hard to keep their algebra clean and formal, and they could not agree to have it contaminated by gimmicks such as diagrams.”

While Pearl is trying to explain the disappearance of a particular strategy of policy analysis that he attributes to Hermann Wold, his larger point is that the absence of appropriate mathematical and diagrammatic notation inhibited the development of causal analysis in econometrics (cf. Pearl 2000, p. 169). The diagram he favors captures the asymmetry of causes as an arrow running from cause to effect. Such diagrams were not unknown in early econometrics; Figure 7 shows such a diagram from Tinbergen’s (1951) econometrics textbook.

Tinbergen, Wold, and the other partisans of process analysis typically combined these diagrams with time order. The asymmetry of time order is easily translated into equations. Orcutt (1952) used similar diagrams without the time order. He captured causality in a Cowles-Commission framework. Once time order is omitted, it is trickier to translate causal asymmetry into mathematics. The equal sign is a symmetrical, reversible relationship—very unlike an arrow.

Despite the fact that econometricians report regression “equations,” regres-

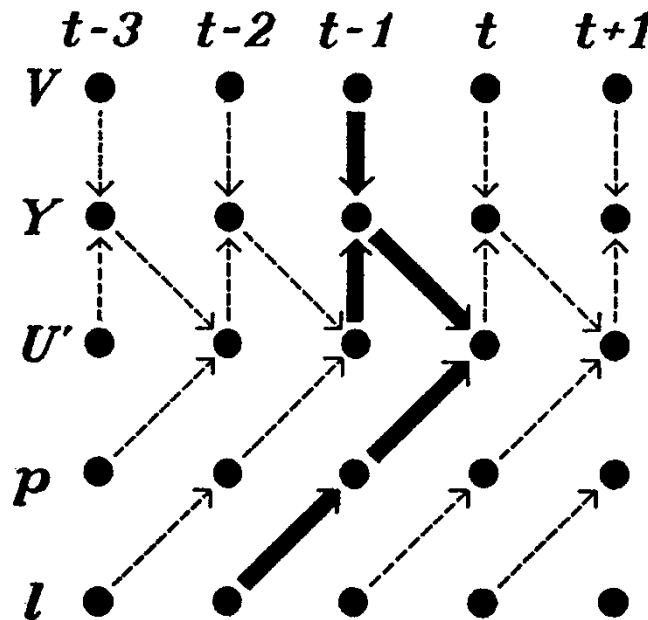


Figure 7. Causal Asymmetry Indicated by Arrows
 Source: Tinbergen (1951), Figure 1, p. 38.

sions are asymmetrical: if the coefficient from the regression of Y on X is β , the coefficient from the regression of X on Y is *not* $1/\beta$. If one starts with causes, then the equal sign of the regression equation is turned into an implicit arrow through the convention of putting causes on the right-hand side.

Throughout the 1950s and 1960s, those authors of econometrics textbooks who most stress the asymmetry of regression or who use arrow diagrams, explicitly or implicitly, are also the users of causal language (Tinbergen 1951, Valavanis 1959, Ezekiel and Fox 1959, Klein 1962, Lange 1962, Walters 1968). In contrast, those textbook authors in thrall to the equal sign—generally, the authors who emphasized the statistical theory of econometrics—virtually never use causal language (Tintner 1952, Goldberger 1964, Johnston 1963, Brennan 1965, Leser 1966, Kane 1968).

The Cowles Commission had analyzed identification in simultaneous systems in terms of *exogenous*, *endogenous*, and *predetermined* variables. Simon (1953) had shown that identification was isomorphic to causal ordering. But the Cowles-Commission terminology did not function on same rhetorical plane as causal terminology. Where “cause” expresses the asymmetry of a relationship, “exogenous,” “endogenous,” and “predetermined” are classifications of the status of variables. Valavanis (1959, p. 63), himself an advocate of a causal econometrics, understood the Russellian drift of the Cowles-Commission program:

“Everything depends on everything else” is the theme song of the Economic and the Celestial Spheres . . . [The endogenous variables] hang from one another by means of several *distinct* causal strings. Thus there are *two causal* **[more politely, functional]** relations between aggregate consumption and aggregate income. [Original emphasis in italic; added emphasis in bold.]

By the 1960s and 1970s most authors of econometrics textbooks have mastered the new etiquette and have become so courtly in their devotion to the equal sign that they are even able to discuss Wold’s recursive models (i.e., process analysis) politely, without invoking causes (Goldberger 1964, pp. 382–87; Theil 1971, pp. 461–62). Theil (1971, p. 436) goes one better by employing arrows without invoking any causal significance in describing exogeneity in one of Klein’s macroeconomic models of the United States.

Another explanation of the lost causes is, then, the triumph of the mathematics of the equal sign in econometrics. But naturally this raises two questions: Why did econometrics take that particular turn? And what has changed to account for the recovery of causal language from the 1990s to the present?

VI. THEORY OR WORLD?

I would like to end this lecture by trying to answer those questions with an explanation of the lost causes that is complementary to the three that I have previously canvassed. Virtually all econometrics textbooks in the postwar period paid lip service to the Econometric Society’s ideal of econometrics as a cooperative enterprise between economic theory, mathematics, statistics, and data. They nevertheless formed three streams (see Table 1). The *data-first* stream viewed the econometric problem as one of characterizing the data statistically

Table 1. The Orientation of Econometrics Textbooks of the 1950s–1970s on the Data—Econometric Theory—Mathematical Economic Theory Spectrum

Data First (Statistics + Data Confront Theory)	Pure Econometric Theory	Theory First (Statistics + Theory Confront Data)
Tinbergen 1951		
	Klein 1953	Tintner 1952
Valavanis 1959 Ezekiel and Fox 1959 Klein 1962 Lange 1962		
	Goldberger 1964	Johnston 1963
		Brennan 1965
	Leser 1966	
Walters 1968		Kane 1968
	Theil 1971	

and bringing it to confront the theory. This was clearly the attitude of Tinbergen (1951), but was also exemplified by Klein (1962) and, indeed, by all of the textbooks in which causal language was important. The *theory-first* stream viewed the econometric problem as giving a statistical characterization of economic theory and then bringing it to confront the data. Brennan's *Preface to Econometrics* (1965) provides a clear example. It is an elementary mathematical economics textbook joined to an elementary statistics text. Real data play no part in the exposition. In contrast, Klein's *Introduction to Econometrics* (1962), a prequel to his more econometric theoretical textbook of 1953, takes the problem of characterizing actual data as a central theme. The *pure-econometric-theory* stream exemplified by Goldberger's (1964) text largely ignores the problem of mapping from econometric techniques to either theory or data in favor of developing those techniques. The theory-first and the pure-econometric-theory streams tend to run together over time, probably because the problems of mapping between econometric theory and the elementary mathematical economic theory common in applied work is essentially trivial. Both streams are virtually devoid of causal language.

My contention, then, is that causal language flourishes when empirical economists start with data, but withers when they start with mathematical theory. That causes are lost is a corollary to the rise of mathematical formalism—especially the dominance of Walrasian general-equilibrium models—in the post-war period, developments recounted and criticized by Weintraub (2002), Mark Blaug (2003), and others.

Morgan (1991, p. 256) made virtually this same suggestion to explain how static, simultaneous-equations systems had come to dominate process analysis. I wish to enter only one caveat. To Morgan, Klein is the agent of Walrasian theory (is “villain” too strong a word?). But my reading places Klein on the side of data and causes. Yes, Klein stood for simultaneous systems, not Wold's recursive systems; and, yes, Klein thought of causes in a Cowles Commission framework.

But, crucially, he did continue to think of causes; and he started with the data. His framework is not really static. The equations in his large-scale macromodels have lagged terms, and those lags are not justified by *a priori* theory, but by the exigencies of the data. It is worth recalling that when that true agent of Walrasian theory, Robert Lucas (1976), attacked the *bona fides* of large-scale macromodels, essentially on the grounds that they were not true to the pure Cowles Commission vision, Klein was the trophy of that day's sport.

This explanation of the lost causes gains credibility from its power to rationalize other elements in the history of econometrics.

We can see, for instance, why causal asymmetry was ignored. Starting with theory, the equal sign rules: if a demand curve is expressed as $p = \beta q$, then unlike the case of regression, it is true that $q = (1/\beta)p$. Adding the error terms after the fact allows the implicit causal order to be whatever the whimsy or inattention of the theorist chooses.

We can also see why the alternative, Cowles Commission-friendly causal analysis of Herbert Simon (1953) never took the place of the causal language of the faded process analysis. The world of actual data is open-ended, the world of mathematical theory is closed and complete. Within a closed world, a world in which I already know everything important, a world in which some variables are labeled "predetermined" (meaning that they are fixed facts) or "exogenous" (meaning that they are the switches or buttons for manipulating the system), causal language is unnatural. In normal speech, I say "I turned on the light," *not* "I caused the light to be turned on." "Cause" is a diagnostician's word. If we know exactly how something works and we just want to describe it with accurate measurements, we are unlikely to refer to causes. It is when we do not know how things work or when they go wrong in ways that we do not understand, that we ask "what caused that?" The complacent omniscience of economic theory is the enemy of causal talk. Friedman starts with data and has the diagnostician's attitude in spades, which is what makes him a staunch, if reticent, advocate of a causal econometrics.

This consideration may also explain why causal talk has been much more common in postwar macroeconomics than microeconomics. Microeconomic theory is more self-contained and self-referential. It does not need causal talk. Macroeconomics is more worldly, and when its expeditions into the world end in depression or hyperinflation or persistent unemployment or currency collapse or, more rarely, in unexpected booms, it is natural to perform a post mortem—to seek the causes and to learn from the failures and successes.

Finally, and now we come full circle back to Heckman's initial observation, we can see why casual talk has revived somewhat in *microeconomics* as well. A quick survey of my empirically oriented micro colleagues reveals that it is labor economists and public economists, far more than, say, industrial-organization economists, who are likely to think that econometrics is about causes. This appears to be because there is a *methodenstreit* within labor and public economics between those who advocate structural estimation (that is, theory first) and those who advocate nonstructural techniques (that is, data first). The nonstructuralists are dissatisfied with the fruit of structural econometrics. They see puzzles that they cannot explain, and they hope to resolve them through "natural experi-

ments.” Their problems are diagnostic and, therefore, quite naturally cast into causal language. The nonstructuralists appear to be gaining the upper hand.

Having spent the last twenty-five years as a macroeconomist and methodologist advocating for causal analysis, I was tempted to take heart and change the title of the lecture to “Causes Redux.” But that really would be a Whiggish move. While I am content to write court historiography, I really wish to avoid the Lives of the Saints.

REFERENCES

- Basmann, R. L. 1965. “A Note on the Statistical Testability of ‘Explicit Causal Chains’ Against the Class of ‘Interdependent’ Models.” *Journal of the American Statistical Association* 60 (312): 1080–93.
- Blaug, M. 2003. “The Formalist Revolution of the 1950s.” *Journal of the History of Economic Thought* 25 (2): 145–56.
- Boot, M. 1995. “A Peculiarly English Provocateur.” *Wall Street Journal* October 30, 1995, p. A16.
- Brennen, M. J. 1965. *Preface to Econometrics*. Cincinnati, OH: South-Western Publishing Company.
- Ezekiel, M. & Fox, K. A. 1959. *Methods of Correlation and Regression Analysis*. New York: John Wiley & Sons, Inc.
- Frisch, R. 1933. Editor’s Note. *Econometrica* 1 (1): 1–4.
- Friedman, M. 1949. “The Marshallian Demand Curve.” *Journal of Political Economy* 57 (6): 463–95.
- Friedman, M. 1953. “The Methodology of Positive Economics.” In *Essays in Positive Economics*. Chicago, IL: Chicago University Press, pp. 3–43.
- Friedman, M. 1970. “Comment on Tobin.” *Quarterly Journal of Economics* 84 (2): 318–27.
- Friedman, M. & Schwartz, A. J. 1963a. *A Monetary History of the United States, 1867–1960*. Princeton, NJ: Princeton University Press.
- Friedman, M. & Schwartz, A. J. 1963b. Money and Business Cycles. *Review of Economics and Statistics* 45 (supplement). Reprinted in Milton Friedman, *The Optimum Quantity of Money and Other Essays*. Chicago, IL: Aldine, pp. 189–236.
- Goldberger, A. 1964. *Econometric Theory*. London: John Wiley.
- Granger, C. W. J. 1969. “Investigating Causal Relations by Econometric Models and Cross-spectral Methods.” *Econometrica* 37 (3): 424–38.
- Haavelmo, T. 1944. “The Probability Approach in Econometrics.” *Econometrica* 12 (Supplement): iii–vi + 1–115.
- Hamilton, J. 1994. *Time Series Analysis*. Princeton, NJ: Princeton University Press.
- Hammond, J. D. 1996. *Theory and Measurement: Causality Issues in Milton Friedman’s Monetary Economics*. Cambridge: Cambridge University Press.
- Heckman, J. J. 2000. “Causal Parameters and Policy Analysis in Economics: A Twentieth Century Retrospective.” *Quarterly Journal of Economics* 115 (1): 45–97.
- Hendry, D. F. 1995. *Dynamic Econometrics*. Oxford: Oxford University Press.
- Hendry, D. F. & Morgan, M. S. 1995. *The Foundations of Econometric Analysis*. Cambridge: Cambridge University Press.
- Hume, D. 1742. *Essays: Moral, Political, and Literary*, edited by Eugene F. Miller. Indianapolis, IN: Liberty Classics, 1985.
- Hume, D. 1777. *An Enquiry Concerning Human Understanding*. In L. A. Selby-Bigge, ed., *Enquiries Concerning Human Understanding and Concerning the Principles of Morals*, second edition. Oxford: Clarendon Press, 1902.
- Johnston, J. 1963. *Econometric Methods*. New York, NY: McGraw-Hill Book Company, Inc.
- Kane, E. J. 1968. *Economic Statistics and Econometrics*. New York: Harper & Row.
- Klein, L. R. 1953. *A Textbook of Econometrics*. Evanston, IL: Row, Peterson and Co.
- Klein, L. R. 1962. *An Introduction to Econometrics*. Englewood Cliffs, NJ: Prentice-Hall, Inc.
- Koopmans, T. 1950. *Statistical Inference in Dynamic Economic Models*. Cowles Commission Monograph 10. New York: Wiley.

- Koopmans, T. & Hood, W. eds. 1953. *Studies in Econometric Method*. Cowles Commission Monograph 14. New Haven, CT: Yale University Press.
- Lange, O. 1962. *Introduction to Econometrics*. Oxford: Pergamon Press.
- Latour, B. 1988. *Pasteurization of France*, translated by Alan Sheridan and John Law. Cambridge, MA: Harvard University Press.
- Leser, C. E. V. 1966. *Econometric Techniques and Problems*. New York, NY: Hafner Publishing Company.
- Lucas, R. E. Jr. 1976. "Econometric Policy Evaluation: A Critique." In Karl Brunner and Allan H. Meltzer, eds. *The Phillips Curve and Labor Markets*, Carnegie-Rochester Conference Series on Public Policy, Vol. 11, Spring. Amsterdam: North-Holland, pp. 161–68.
- Marshall, A. 1885. "The Present Position of Economics." In A. C. Pigou, ed., *Memorials of Alfred Marshall*. London: Macmillan, 1925, pp. 152–74.
- Mayer, T. 1993. "Friedman's Methodology of Positive Economics: A Soft Reading." *Economic Inquiry* 31 (2): 213–23.
- Morgan, M. S. 1990. *The History of Econometric Ideas*. Cambridge: Cambridge University Press.
- Morgan, M. S. 1991. "The Stamping Out of Process Analysis in Econometrics." In Neil De Marchi and Mark Blaug, eds., *Appraising Economic Theories: Studies in the Methodology of Research Programs*. Aldershot: Edward Elgar, pp. 237–65.
- Orcutt, G. H. 1952. "Actions, Consequences, and Causal Relations." *Review of Economics and Statistics* 34 (4): 305–13.
- Pearl, J. 2000. *Causality: Models, Reasoning, and Inference*. Cambridge: Cambridge University Press.
- Russell, B. 1918. On the Notion of Cause. In Bertrand Russell, *Mysticism and Logic*, London: Allan and Unwin, pp. 180–208.
- Simon, H. A. 1952. "On the Definition of the Causal Relation." Reprinted in Herbert A. Simon, *Models of Man*. New York: John Wiley & Sons, 1957.
- Simon, H. A. 1953. "Causal Ordering and Identifiability." Reprinted in Herbert A. Simon, *Models of Man*. New York: John Wiley & Sons, 1957.
- Simon, H. A. 1955. "Causality and Econometrics: Comment." *Econometrica* 23 (2): 193–95.
- Sims, C. A. 1972. "Money, Income, and Causality." *American Economic Review* 62 (4): 540–52.
- Stone, L. Undated. "The Future of History", unpublished typescript.
- Theil, H. 1971. *Principles of Econometrics*. New York: John Wiley & Sons, Inc.
- Tinbergen, J. 1951. *Econometrics*. New York: The Blakiston Company.
- Tintner, G. 1952. *Econometrics*. New York: John Wiley & Sons, Inc.
- Tobin, J. 1970. "Money and Income: Post Hoc Ergo Propter Hoc?" *Quarterly Journal of Economics* 84 (2): 301–17.
- Valavanis, S. 1959. *Econometrics*. New York: McGraw-Hill Book Company, Inc.
- Walters, A. A. 1970. *An Introduction to Econometrics*. New York: W. W. Norton & Company, Inc.
- Weintraub, E. R. 1991. *Stabilizing Dynamics*. Cambridge: Cambridge University Press.
- Weintraub, E. R. 2001. "Re: HES: QUERY—History of Political Economy Readers." E-mail message on the History of Economics Society listserv, August 16, 2001.
- Weintraub, E. R. 2002. *How Economics Became a Mathematical Science*. Durham: Duke University Press.
- Wold, H. O. A. 1955. "Causality and Econometrics: Reply." *Econometrica* 23 (2): 196–97.
- Woodward, C. V. 1986. "The Lost Cause." *New York Review of Books*, Vol. 33, January 30, 1986, pp. 26–29.