The Methodology of Positive Economics

Reflections on the Milton Friedman legacy

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12 Milton Friedman's stance: the methodology of causal realism

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1 The God of Abraham; the methodology of Marshall[1]

The philosopher Bas Van Fraassen opens his Terry Lectures (published as *The Empirical Stance*) with an anecdote: "When Pascal died, a scrap of paper was found in the lining of his coat. On it was written 'The God of Abraham, Isaac and Jacob, not the God of the philosophers'" (Van Fraassen 2002, 1). Pascal's God talks and wrestles with men; Descartes's God is a creature of metaphysics. Analogously, with respect to the "Methodology of positive economics" (F53) there are two Friedmans. Most of the gallons of ink spilled in interpreting Friedman's essay have treated it as a philosophical work. This is true, for example, for those who have interpreted it as an exemplar of instrumentalism, Popperian falsificationism, conventionalism, positivism, and so forth. And it is even true for those critics, such as Samuelson (1963), whose credentials as an economist are otherwise secure. Mayer (1993a, 1995, 2003) and Hands (2003) remind us that Friedman was philosophically unsophisticated, and in the essay Friedman tried a fall with other economists, not with philosophers. The origin of the essay was the quotidian practice of economics, not abstract epistemology. To know the Friedman of the economists, I propose to read the essay in light of Friedman (and Schwartz's) *A Monetary History of the United States, 1867–1960* (1963a) and "Money and business cycles" (1963b), perhaps his most characteristic economic investigations.

My point is not that there is a particular philosophers' position that can be contrasted with a particular economists' (or even Friedman's)

[1] I thank Uskali Mäki, Thomas Mayer, and the participants in the F53 Conference at the Erasmus Institute of Philosophy and Economics, Rotterdam, December 12–13, 2003 for comments on an earlier draft. I also thank Ryan Brady for research assistance. J. Daniel Hammond (1996) wrote an important book that examines Friedman's economic thought from a causal perspective. While I intentionally did not reread that book in trying to develop my own views somewhat independently, I could not help but be influenced by it.

position. After all, Pascal surely recognized that there were a variety of views of God among the philosophers. Nor would I suggest that philosophy has nothing valuable to say about, or learn from, Friedman's essay nor Friedman about or from philosophy. It is rather a matter of approach. Friedman cares most about doing economics and is largely innocent of the interests of philosophers and, equally, of the distinctions and nuances that philosophers routinely employ. If we – either as economists or philosophers – insist on reading the essay as a philosophical work addressed to philosophers, we must misread it.

The most infamous passage in Friedman's essay runs: "Truly important and significant hypotheses will be found to have 'assumptions' that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions (in this sense)" (14).[2] Samuelson (1963, 232–3) sees this as the extreme version of Friedman's general proposition that "[a] theory is vindicated if (some of) its consequences are empirically valid to a useful degree of approximation; the (empirical) realism of the theory 'itself,' or of its 'assumptions,' is quite irrelevant to its validity and worth," which he stigmatizes with the shorthand *F-twist*.[3]

The view that Friedman does not care about the truth of the assumptions faces a serious difficulty. After recounting the detailed evidence on the cyclical behavior of money and the real economy, Friedman and Schwartz (1963b, 213–14) raise the question, how we can be sure about the direction of influence?

> It might be, so far as we know, that one could marshal a similar body of evidence demonstrating that the production of dressmakers' pins has displayed over the past nine decades a regular cyclical pattern; that the pin pattern reaches a peak well before the reference peak and a trough well before the reference trough; that its amplitude is highly correlated with the amplitude of the movements in general business. It might even be demonstrated that the simple correlation between the production of pins and consumption is higher than the simple correlation between autonomous expenditures and consumption; that the partial correlation between pins and consumption – holding autonomous expenditure constant – is as high as the simple correlation; and that the correlation between consumption and autonomous expenditures – holding the production of pins constant – is on the average zero ... [B]ut even if [these statements] were demonstrated beyond a shadow of a doubt, they would persuade neither us nor our readers to adopt a pin theory of the cycle.

But why not? In Friedman and Schwartz's thought experiment, the pin theory of the cycle has implications that are confirmed facts. Why should the same variety of evidence that supports a monetary theory not equally well support a pin theory? "Primarily, the difference is that we have other kinds of evidence" (Friedman and Schwartz 1963b, 214): (i) pins are a trifling element in the economy, and "[w]e expect the effect to be in rough proportion to the cause," but money is pervasive in economies that experience business cycles; (ii) with money we can, and with pins we cannot, conceive of channels through which large autonomous changes in them might affect the economy; (iii) in contrast to a monetary theory, no serious student of business cycles has ever seriously suggested a pin theory.

It is not good enough for Friedman that a pin theory implies the facts. His criticisms are based on evidence relevant to the truth of its assumptions. The *Monetary History* can be seen as a detailed marshalling, not only of the facts implied by Friedman's monetary theory, but of the evidence for the truth of its underlying mechanisms and initial conditions: its assumptions. A partisan for the assumptions-don't-matter interpretation of Friedman's essay might argue that there is a flat contradiction between his rejection of the pin theory and his methodological position. That would be too easy and too lazy. To say that Friedman does not care about the truth of the assumptions is wrong. On the contrary, Friedman's methodological stance in the essay is best described as *causal realism*, which can be defined as the view that the object of scientific inquiry is the discovery through empirical investigation of the true causal mechanisms underlying observable phenomena.

One object of this chapter is to support this controversial claim. To do so, I propose to read Friedman's essay charitably – not as the work of an amateur philosopher but as the work of an economist, one whose methodological reflections are consistent with his intellectual antecedents and with his own empirical practice. Friedman's most important intellectual antecedent is Alfred Marshall. Marshall's writings are Friedman's economic Bible. When, after a long and illustrious life, Friedman finally passes on, he may well leave a scrap of paper pinned to the lining of his coat on which will be written: "The Methodology of Marshall, not the Methodology of the philosophers."

[2] Except where context would render it ambiguous, references to F53 will be referred to hereinafter by page numbers only.

[3] Replacing Friedman by his initial is an indication that Samuelson (1963, 232) is aware that the F-twist may be "a misinterpretation of [Friedman's] intention." While Samuelson rails against Friedman's apparent abandonment of truth and realism, Frazier and Boland (1983) turn it into a positive virtue. They interpret Friedman as an instrumentalist and defend instrumentalism as a logically sound doctrine. My suggestion here is that they have misinterpreted Friedman – he is not an instrumentalist – and my suggestion in Hoover (1984) is that Frazer and Boland's version of instrumentalism is not sound doctrine.

2 The absence of causal talk

If Friedman is a causal realist, there surely is a puzzle in his work: he rarely talks about causes. In the 814 pages of *Monetary History*, a work that I maintain is an exemplary piece of causal realist research, the words "cause" or "causal" are used – as far as I can tell – only nine times, and four of those nine are attributed to agents of the Federal Reserve (see table 12.1).[4] Similarly, Friedman's claim that the effect should be proportional to cause cited earlier is one of only two instances of causal talk in "Money and business cycles." What is more, Friedman is conscious of his own unease with respect to causal talk. Tobin (1970, 301) asserts that Friedman claims that changes in the money supply are the principal cause (sometimes unique) of changes in money income. In reply, Friedman (1970, 319) not only denies the accuracy of Tobin's claim, but assails the imprecision of Tobin's causal talk. Friedman (1974, 101) writes: "I myself try to avoid use of the word 'cause' ... it is a tricky and unsatisfactory word" (cited by Hammond 1992, 91–2, as part of interview with Friedman).

There is less here than meets the eye. Friedman routinely employs a large variety of synonyms and circumlocutions, so that he clearly engages in causal talk, even when explicit causal words are not in play. Instead of *A causes B*, Friedman frequently writes: *A produces* (or *influences, engenders, affects, brings about*) *B*; *B reflects* (or *is a consequence of, is a result of, is an effect of*) *A*; *A is a dependent variable, while B is an independent variable*. Instead of *causal chains* Friedman writes of *chains of influence*. Table 12.2 gives a number of examples from the *Monetary History*, which plainly show that, while Friedman and Schwartz have avoided the use of "cause," "causal," and etymologically related terms, they use a variety of words that must be regarded as synonyms.

Friedman's avoidance of explicit causal talk in the face of clearly causal intent might be dismissed as a mere quirk. I will argue subsequently that it is of a piece with his overall Marshallian methodology. But first we must examine that methodology somewhat more closely.

3 The F-twist in the light of Alfred Marshall

In describing Friedman's intellectual relationship to Marshall, the metaphor of the Bible was not chosen lightly. Methodologically Friedman is like a fundamentalist Christian who constructs his theological position by direct quotation from scripture. Once one has read Marshall's essay, "The present position of economics" (1885), the methodological antecedents of Friedman's essay and his other well-known methodological work, "The Marshallian demand curve" (1949b), are obvious and fairly complete. The word in Cambridge used to be "It's all in Marshall." This appears to have been true in Chicago as well.

I take the identity of Friedman's and Marshall's methodological positions as a postulate. In juxtaposing arguments from their two essays, I do not aim to persuade anyone of that identity. Rather I explicate what I take to be their commonly held views.

[4] My count is as accurate as I could make it on a single reading of the text, but I cannot warranty that I have caught every instance. Causal language would be exceedingly uncommon in the *Monetary History* even if I had shortened the count by half a dozen instances.

Table 12.1 *Uses of causal language in Friedman and Schwartz's* Monetary History of the United States, 1867–1960

"The **proximate cause** of the world price rise was clearly the tremendous outpouring of gold after 1890 that resulted from the discoveries in South Africa, Alaska, and Colorado and from the development of improved methods of mining and refining." [p. 137]
"And, of course, the Board also took the position that the expansion in the stock of money was **a result, not a cause,** of rising prices." [p. 223]
"[The Federal Reserve Board] argued that changes in U.S. prices were **effect rather than cause,** that the Reserve Board was powerless to do more than adapt to them, and that the Board's policies had prevented financial panic at home and moderate the price changes." [p. 237]
"Doubtless there were effects both ways **and common causes as well.**" [p. 288]
"By relevant standards, the discount policy was abnormally tight, not easy. The System regarded the lack of discounting as a reflection of the large accumulation of excess reserves and hence as a lack of need for accommodation. That view no doubt had some validity, but **the causal chain ran the other way as well.**" [p. 514]
"To his directors, he [Governor George L. Harrison of the Federal Reserve Bank of New York] pointed out in December that 'most of the executives of the Reserve System do not believe that monetary action would afford relief in the present business situation, regardless of whether **the causes of the recession** are monetary or non-monetary but rather that it was felt that improvement in the business situation would be more influenced by actions of the Administration.'" [pp. 528–9]
"Precisely the same is true of a draft of a memorandum prepared at the New York Federal Reserve Bank by John H. Williams in answer to the question **whether the reserve requirement changes caused the 1937–38 depression.**" [p. 544]
"Consideration of the effects of monetary policy on the stock of money certainly strengthens the case for attributing an important role to monetary changes as a factor that significantly intensified the severity of the decline and also probably **caused it to occur earlier than otherwise.**" [p. 544]
"Again, if the common movement of the stock of money and of money income and prices is not coincidental **or the consequence of a common cause,** the direction of influence must run from money to income." [p. 687]

Notes: emphasis in bold type added; page numbers in square brackets. The table attempts to identify every use of explicitly causal language in the *Monetary History*. It was, however, constructed from a single reading, so that completeness cannot be guaranteed.

Table 12.2 *Examples of synonyms for causal language from Friedman and Schwartz's* Monetary History of the United States, 1867–1960

"[The *Monetary History*] traces changes in the stock of money for nearly a century, from just after the Civil War to 1960, examines the factors that accounted for changes, and analyzes **the reflex influence** that the stock of money exerted on the course of events." [p. 3]
"That outcome was widely regarded as, at least partly, **a delayed reaction to** the large wartime increases in the stock of money." [p. 13]
"As a matter of economics, there can be little doubt that [the contrast between a rise of 1.1 per cent per year in the money stock and a decline of 3.5 per cent per year in prices] **reflects primarily** a rise in output." [p. 34]
"On this interpretation **the chain of influence** ran from the attempted deflation to the economic stagnation." [p. 41]
"Not only did [the deflation of 50 percent in the decade and a half after 1865] **not produce** stagnation; on the contrary it was **accompanied and produced** by a rapid rise in real income." [p. 41]
"the amount of high-powered money is **a dependent rather than an independent variable**, and is not subject to governmental **determination**." [p. 52]
"Such shifting expectations could **affect** the price of gold *only* as they **affected** the demand for and supply of foreign exchange ..." [p. 58]
"the government **did succeed in bringing about** a minor reduction in the stock of high-powered money ..." [p. 81]
"The **major channel of influence** was from the stock of money to the level of money income to the level of prices, and thence to the rate of exchange between the dollar and other currencies, though undoubtedly some **influences** ran in the other direction." [p. 89]
"These measures, in turn however, **had offsetting effects**, since debt redemption reduced the amount and raised the price of bonds available to serve as backing for national bank notes, and so **led to a reduction** in national bank notes from a peak of some $350 million in 1882 to a trough of some $160 million in 1891." [p. 128]
"there was no evidence on the length of the lag **between action and effect**." [p. 239]
"the decline in the stock of money and the near-collapse of the banking system can be regarded as **a consequence of** nonmonetary forces in the United States, and monetary and nonmonetary forces in the rest of the world." [p. 300]
"if it did initiate a worldwide disturbance, it would inevitably be **affected in turn by reflex influences** from the rest of the world."
"the rapid rise in the money stock certainly **promoted and facilitated** the concurrent economic expansion." [p. 544]

Notes: emphasis in bold type added; page numbers in square brackets.

Van Fraassen (2002) refers to a "stance" as a kind of commitment to a creed that is maintained by faith. Within the "empirical stance," certain kinds of empirical evidence have force, but the stance itself cannot be justified by evidence of the same kind. Friedman's stance is Marshallian. But Friedman sees himself to be in a kind of Babylonian exile, singing Marshall's song in a foreign land, while other economists sacrifice at the altar of strange gods. Baal in this parable is Walras. Friedman sees the Walrasian

stance as the requirement that, to know anything, one must know everything. A theory must articulate a structure that can accommodate every economic actor in its full particularity. The Walrasian economist recognizes the impracticality of doing this completely. Instead, the Walrasian offers the perfect general-equilibrium model as a transcendent ideal from which one can criticize the compromised and inconsistent realities of applied economics. Walras's is truly the Methodology of the philosophers.

The Walrasian approach suggests viewing the whole economy from an Olympian height. But Friedman denies that there is any such standpoint or that anyone could grasp the economy in its totality. Marshall argues that "[t]here is no use in waiting idly for [a unified social science]; we must do what we can with our present resources." He goes on, "common sense does not deal with a complex problem as a whole. Its first step is to break the problem into its several parts ... the human mind has no other method of inquiry than this (Marshall [1885] 1925, 164).[5] For Friedman knowledge is the product of sweaty labor among gritty facts. Marshall (p. 171; cf. Friedman [1949] 1953b, 90) writes that the economist "must stand by the more laborious plan of interrogating the facts in order to learn the manner of action of causes singly and in combination." Friedman's and Marshall's object, then, is the acquisition of causal truth. The barrier is the complexity. Marshall (157) writes that economic "causes often lie below the surface and are likely to be overlooked by the ordinary observer." Similarly, in his essay Friedman (33) writes:

> A fundamental hypothesis of science is that appearances are deceptive and that there is a way of looking at or interpreting or organizing the evidence that will reveal superficially disconnected and diverse phenomena to be manifestations of a more fundamental and relatively simple structure.

The *Monetary History* can be seen as an attempt to put that hypothesis to the test. In the "Summing up" at the end of the book, Friedman and Schwartz (1963a, 676) echo the Marshallian methodological conclusion: "In monetary matters, appearances are deceiving; the important relationships are often precisely the reverse of those that strike the eye."

Opposed to a naive empiricism, Friedman and Marshall give theory a special role. "[F]acts by themselves," writes Marshall, "are silent. Observation discovers nothing directly of the action of causes, but only sequences in time" (166) ... "[T]he most reckless and treacherous of all theorists is he who professes to let the facts and figures speak for themselves ..." (168;

[5] Except where context would render it ambiguous, references to Marshall ([1885] 1925) will be referred to hereinafter by page numbers without dates.

cf. Friedman [1949] 1953b, 90). In the essay, Friedman concurs: "A theory is the way we perceive 'facts,' and we cannot perceive 'facts' without a theory" (34).

What is a theory? Friedman ([1949] 1953b, 91) draws his interpretation of "theory" directly from Marshall:

> Economic theory, in this view, has two intermingled roles: to provide "systematic and organized methods of reasoning" [Marshall, 164; cf. F53, 7] about economic problems; to provide a body of substantive hypotheses, based on factual evidence, about the "manner of action of causes" [Marshall, 171]

Theory, or what Marshall (164) calls the "economic organon" is "not a body of concrete truth, but an engine for the discovery of concrete truth, similar to, say, the theory of mechanics" (Marshall, 159; quoted in part by Friedman [1949] 1953b, 90). The ideal theory for Marshall is universal, saying nothing about particulars, and needing to be supplemented with particular facts if it is to be useful. Economic theory, however, has not yet reached universality so that it is necessary "to sacrifice generality of form to some extent" (Marshall, 160).

In the essay, Friedman characterizes theory as, in part, "a 'language'" (7), an "analytical filing system" (11) constituted of tautologies that are useful in "organizing refractory empirical phenomena" (12) and, "[i]n part, a body of substantive hypotheses designed to abstract essential features of complex reality" (7).

Both Friedman and Marshall conceive of theory as a purely deductive system, whose claims are universal. Even the particulars that, on the one hand, fill the lacunae of incomplete theory and, on the other hand, tie that theory to empirical applications are strained and filtered to pick out what is "essential": the color of the wheat-trader's hair or number of members of his family are not relevant particulars (F53, 32). There is little difference here between Cartwright's (1999, ch. 2) treatment of theory as fables in the sense of Lessing or some accounts of theories as idealizations (Nowak 1980; Hoover 1994) – views which are compatible with realism. Friedman (F53, 36) himself refers to perfect competition and monopoly as ideal types, the application of which to concrete cases requires judgment about their suitability and about the objects of the analysis.

Cartwright thinks that fables are true; Nowak thinks idealizations get to the genuine heart of the matter. So what do we make of Friedman's famous denial that the realism of the assumptions matters? The key is to read Friedman's claim carefully. What he actually argues is that checking the realism of the assumptions does not provide a test in addition to the empirical implications of a theory. He does not claim that those assumptions are immune from indirect tests (F53, 14, 23). A theory for

Friedman is a deductive system in which some propositions are taken as axioms (or postulates) from which others can be deduced (F53, 23). The axioms are part of "conceptual world or abstract model" simpler than the real world and tied to it through rules for a class of applications and correspondences (F53, 24). The parts of a theory are interdeducible, so that what may, from one point of view, be taken to be an axiom may, from another point of view, be taken to be a theorem (F53, 26–7). How to classify various theoretical propositions is mainly a matter of finding the most economical mode of stating them (F53, 23). Economical axioms need not themselves be superficially obvious nor tied individually directly to reality.

Friedman's view of the status of theoretical axioms is virtually identical to that of Bertrand Russell (1918, 145–6):

> When pure mathematics is organized as a deductive system – i.e. as the set of all those propositions that can be deduced from an assigned set of premises – it becomes obvious that, if we are to believe in the truth of pure mathematics, it cannot be solely because we believe in the truth of the premises. Some of the premises are much less obvious than some of their consequences, and are believed chiefly because of their consequences. This will be found to be always the case when a science is arranged as a deductive system. It is not the logically simplest propositions of the system that are the most obvious or that provide the chief part of our reasons for believing in the system ... Electro-dynamics, for example, can be concentrated into Maxwell's equations, but these equations are believed because of the observed truth of certain of their logical consequences.

Friedman subscribes both to Russell's claim that axioms are often not obvious ("appearances are deceiving") and that they *are* supported by indirect evidence. In dismissing as futile "Walrasian" criticism of the realism of assumptions, Friedman is attacking *naive theoreticism* – the idea that a theory should (or must) mirror casually parsed facts about the world. Instead, Friedman argues that a theory needs to get to the essence of the matter. Generally, that requires the creation of categories of entities that are not directly observable, governed by rules that omit irrelevant details, whose success is to be judged holistically.

In his monetary analysis, Friedman illustrates this strategy. The *velocity of circulation of money*, for instance, is a theoretical construct that can be observed only indirectly (= *nominal GDP/money* or = 1/*money holdings expressed as a number of weeks' income*). Friedman treats velocity as a causally significant, real category. But the evidence that he offers for its causal and ontological significance (in the *Monetary History* or in Friedman 1959, 1966, or Friedman and Schwartz 1982) is the general empirical stability of the entire analytical schema of which it is but one part.[6] What is more, Friedman treats velocity as a variable that measured in one context can be transferred to another context.

Permanent income provides another – and perhaps more compelling – example. Like velocity, permanent income is not directly observable, but is indirectly measured and validated in the context of the consumption function. Permanent income is not only a causally significant category in this context, but Friedman regards it as sufficiently freestanding and independent of its original context, that he routinely uses the measured quantity in other contexts. For example, Friedman takes permanent income to be a causal determinant of the demand for money (i.e. *permanent velocity = permanent income/money*).

Critics who hold that Friedman does not care about the truth of assumptions or even positively endorses their falsehood in some unqualified sense overlook Friedman's claims about the import of indirect evidence. In a famous example (F53, 20), Friedman talks about leaves of a tree arranging themselves as if they were maximizing received light. Since trees do not maximize, this is often taken to mean that the truth of the assumption does not matter. But Friedman goes on to say that we should prefer an explanation in terms of the mechanisms that make the tree behave as if (counterfactually) it were a conscious maximizer. Finding and elaborating such mechanisms is useful. First, a theory that encompasses those mechanisms will be more general and, therefore, more fruitful in making predictions about a wider variety of phenomena. Second, evidence for the predictive power of such a theory in other domains lends *indirect* evidence in favor of the prediction based on the assumption that trees act as if they maximized received light. The assumptions underlying such a theory may also be unrealistic in Friedman's peculiar sense of not giving us a photographically accurate description of reality, but that does not make them untrue. And, just as for Russell, the more accessible implications of a theory lend support to the logically more primitive, but inaccessible "assumptions."

4 Friedman's anti-realism reconsidered

If Friedman is a causal realist after all, what then are we to make of statements in which he denies the necessity of realistic assumptions and even seems to promote the desirability of unrealistic assumptions?

The goal of science for Friedman (F53, 7) is to construct theories that yield "valid and meaningful ... predictions about phenomena not yet observed."[7] Such theories are generalizations, which necessarily omit many particular and, implicit in the theory, irrelevant details. This lack of particularity or "realisticness" (to use Mäki's apt coinage) is probably the sense of realism in play when he attacks the need for a theory to provide "photographic descriptions of reality" (Friedman [1949] 1953b, 91). "Lack of realism" for Friedman is just another (and for many philosophers, perhaps paradoxical) way of referring to the desirable property that a theory captures the essence of a deep relationship.

[6] As an economist, Friedman does not think or write explicitly in these metaphysical terms.

Marshall (166), in arguing for a theoretical approach to empirical phenomena, notes that "every event is the complex result of so many causes, so closely interwoven that the past can never throw a simple and direct light on the future." In much the same vein, Friedman (F53, 25) argues that a model is a half-truth: although "there is nothing new under the sun" (that is, generalization is the essence of the model), "history never repeats itself" (all generalizations omit particularities). Yet, both Marshall and Friedman see the immersion of the theorist in the particularities of factual inquiry as the source of further generalizations. The more the theorist is successful at eliminating necessary reference to particularities, the more general and more powerful the theory becomes, and the more nearly it approaches Marshall's ideal of a universal economic doctrine (Friedman 1953b, 159; Marshall, 14). Paradoxically, it is unrealisticness that serves and underwrites Friedman's realism with respect to the deep causal mechanisms.

Marshall, of course, did not claim that universal economic doctrine was ready to hand. Friedman (F53, 34) sees the complexity of economic phenomena as itself an argument for the incompleteness of economic theory. To accept the complexity of phenomena as an irreducible fact is to deny "the tentative state of knowledge that alone makes scientific activity meaningful." By way of illustration, he heaps scorn on John Stuart Mill's claim that the laws of value (in 1848!) were complete and required no future development. Economic theory is unrealistic in the sense that it is incomplete. But the only completeness that is valuable scientifically is that which reduces its particularity and simplifies the complexity of phenomena.

The essence of Friedman's Marshallian stance is that the pursuit of substantive knowledge cannot be stymied by the incompleteness and, therefore, "unrealism" of theory. We cannot have a complete, realistic theory first and apply it to concrete phenomena afterwards. Nor can we start with atheoretical facts, for there are none; facts are theory-laden. All we can do is to work back and forth between theory and facts, starting with the primitive and highly tentative and working toward the sophisticated

[7] Friedman (F53, 9) is clear that by prediction he means the inference of the unobserved from the observed, and not only forecasts of the future from the past.

and more secure. In this process, we can tolerate a large lack of realism in another sense. Predictions based on theoretical assumptions that are only approximate and, therefore, unrealistic may be as accurate as we can use given our ability to measure or as accurate as we need for some practical purposes (F53, 15–17).

Lack of realism in all the senses just discussed is perfectly compatible with the project of a constructive causal realism – that is, with the project of seeking to identify the true mechanisms underlying observed phenomena. Friedman's implicit commitment to such a project is clear in his empirical practice as a monetary economist. Most obviously, what would be the point of Friedman and Schwartz's massive historical project, not only of the *Monetary History*, but of its sister volumes, *Monetary Statistics of the United States* (1970) and *Monetary Trends in the United States and the United Kingdom 1867–1975* (1982), if somehow it were possible to be unconcerned about the accuracy of the antecedents of economic inferences? One might think of history as just a giant testbed for Friedmanian models with unrealistic assumptions, but that would miss the *modus operandi* of Friedman and Schwartz as historians: far from merely collecting data on which to test predictions, they seek detailed accounts of the institutions and even the sociological or psychological bases for the actions of particular actors; in other words, they seek to describe the actual mechanisms that generated the behavior of the economy accurately and realistically.

In Friedman's Marshallian methodology, it is possible to have causal knowledge without knowing all of the links, yet filling in the gaps is a positive virtue. In his essay (42), Friedman argues that the static monetary theory is satisfactory, but that we are largely ignorant of dynamic monetary theory. One of the promises of detailed empirical investigations is to seek new generalizations to amplify theory. His point is not merely methodological. In Friedman and Schwartz (1963b, 222) it is elaborated:

> We have little confidence in our knowledge of the transmission mechanism, except in such broad and vague terms as to constitute little more than an impressionistic blueprint. Indeed, this is the challenge our evidence poses: to pin down the transmission mechanism in specific enough detail that we can hope to make reasonably accurate predictions of the course of a wide variety of economic variables on the basis of information about monetary disturbances. (Cf. Friedman and Schwartz 1963a, 678–9.)

The dual call for realistic elaboration of the assumptions of the theory and, simultaneously, for securing breadth of generalization could not be clearer.

In principle, any theoretical propositions are at risk of being overthrown in the face of predictive failure. Although the collapse of a bridge might be taken as evidence by some of a failure of the theory of mechanics, Marshall (159–60) cautions that the theory ought to be regarded as secure in this case and its application questioned.

Friedman maintains a similar position with respect to elementary economic theory. He is well known to have attacked the utility of elaborating price theory with a theory of imperfect competition (F53, 34–9). This is an empirical claim of the lack of fruitfulness of imperfect competition. It could not, consistent with Friedman's own principles, be upheld if imperfect competition were discovered to underwrite better predictions. It is important to understand that Friedman does not dismiss criticism of analysis that divides the world into either perfect competition or monopoly, only because good predictions can be had by acting *as if* these assumptions were true. Rather Friedman makes a conceptual claim that the alternative assumption, imperfect competition, undermines the practical division of the economy into distinct industries and that, without this division, practical testing of the hypothesis is impossible. Since every case would be *sui generis*, imperfect competition could not support a general rule needed for prediction. Friedman does not deny the desirability of true assumptions in this case, instead he questions whether certain assumptions lend themselves to even indirect inquiry into their truth. For both Friedman and Marshall facts are theory-laden: the choice of theoretical assumptions helps to define what constitutes the facts. Friedman argues for one set of assumptions and against another set of assumptions on the basis of each set's effectiveness as a potential servant of the truth.

Friedman's deepest theoretical commitments are in principle, but not in practice, at risk in theoretical inquiry. Auxiliary assumptions are, in contrast, very much at risk. One nice example is provided by Friedman and Schwartz's comparison of the banking crises in the contractions of 1907–8 and 1929–33. The assumptions underlying their analysis of each recession are adjusted to fit the facts of the other. Such adjustments provide the basis for conjectural histories in which one can speculate how things might have gone differently under different policies. Such conjectures are grounded in common assumptions about the mechanisms governing different episodes. Friedman and Schwartz (1963a, 168) conclude:

> all truly analytical history, history that seeks to interpret and not simply to record the past, is of this [conjectural] character, which is why history must be continuously rewritten in the light of new evidence as it unfolds.

Friedman and Schwartz practice what they preach. The *Monetary History* provides numerous examples of counterfactual analysis, the most famous example of which is their extended analysis of how the US economy might not have collapsed so deeply in the early 1930s had Benjamin Strong, the influential (and, to Friedman and Schwartz, right-minded) President of the Federal Reserve Bank of New York, not died in 1928 (Friedman and Schwartz 1963a, ch. 7). Counterfactual experiments of this sort make no sense without a commitment to the reality of a causal structure that can be faced with alternative initial conditions (see Hoover 2001, esp. chs 2 and 4).

5 Effects, not causes

We must now revisit the puzzle of why, if Friedman is a causal realist, he is so averse to using causal language. In his interview with Hammond (1992, 92), Friedman said: "The problem that bothers me about cause is that it almost invariably leads into a problem of infinite regress. There is no such thing as *the* cause of anything." Friedman sees every "proximate cause" standing in a chain of influence. What is more, influences between two variables can go in both directions. While much of Friedman and Schwartz's work has aimed to document the dominant influence of money over income and business activity, they have candidly acknowledged that influence runs from income and business activity to money as well (e.g. Friedman and Schwartz 1963a, 695; 1963b, 214; Friedman 1970, 321). It is not only the existence of causal chains that concern Friedman; he also worries that independent factors might influence any variable at one time and that there are no unambiguous ways to count causes (Friedman 1970, 319).

Friedman's squeamishness with respect to causal language can be seen as consistent with his Marshallian stance. The problem with talking about causes is that, working backwards in time, the chain never ends and, working contemporaneously, the array of causes is (perhaps infinitely) wide. Causal talk could be regarded as the enemy of Marshall's strategy of analyzing problems in manageable units: where do we draw the lines delimiting the scope of our causal interests? To think in causal language is to risk losing focus.[8]

As table 12.2 shows, Friedman frequently replaces talk of causes with circumlocutions that concentrate on effects. One might argue that effects are necessarily correlative to causes, so that this circumlocution is pointless. But that would miss the asymmetry of causation. Effects are defined as a terminus. From the point of view of an effect, the whole world and all of history spreads out around and behind us as potential causes. From the point of view of the cause, the effects lie in relatively compact lines. This is

[8] This interpretation is drawn from Hoover 2004, which concerns the use of causal language in econometrics in the postwar period.

obvious in tracing genealogies: it is a relatively manageable task to trace the descendents of a particular man; it is a completely open-ended task to trace his ancestors. Friedman is by no means alone. In his account of causal analysis, the statistician Paul Holland (1986) argues that we should attempt to assess the effects of a cause and not the causes of an effect, on the grounds that the first will produce stable knowledge, while the second is necessarily always an incomplete task. Holland adds, on top of Friedman's worries, that anytime we assign a proximate cause to an effect, we place ourselves hostage to future research that may find a more elaborate causal chain lying between the (now obviously not proximate) cause and the effect. In contrast, once the effect of a cause is established, further knowledge elaborates the mechanism perhaps, but does not threaten the truth status of the original claim. Friedman is clearly thinking along similar lines when he claims that changes in nominal income are an effect of money, while at the same time calling for investigation into the transmission mechanism that would elaborate that channel as well as others.

There is, as he himself recognizes (Hammond 1992, 97) a large element of semantic choice in Friedman's avoidance of explicit use of "cause" and etymologically related terms. It is a semantic choice that reinforces, and is reinforced by, his Marshallian stance. His usage is not, however, substantive in that it in no way undermines his general commitment to causal realism, which is the commitment to the existence of structures governing the causal influences among economic variables.

6 Milton Friedman's causal legacy

My reading of "Methodology of positive economics" turns large amounts of Friedman scholarship upside down. Most methodologists and most practicing economists have read Friedman's essay as offering a rationale for being unconcerned for the truth status of the assumptions of a theory. What matters is not whether markets are perfectly competitive or whether people form rational expectations, but whether they act *as if* they do.[9] Hutchison (1992, 2000) and Blaug (2002a, b) have taken Friedman to have licensed the rise of formalism in economics on the basis of such an interpretation of the essay. Hands (2003) argues that Friedman does no such thing and that Hutchison and Blaug misinterpret his essay (see also Mayer 2003). While Hands is likely right that Friedman should not be regarded as licensing formalism, the average economist interpreted it along the same lines as Hutchison and Blaug and probably regard it as a defense

[9] See Hammond (1992, 93) for Friedman's own criticisms of the rational-expectations hypothesis.

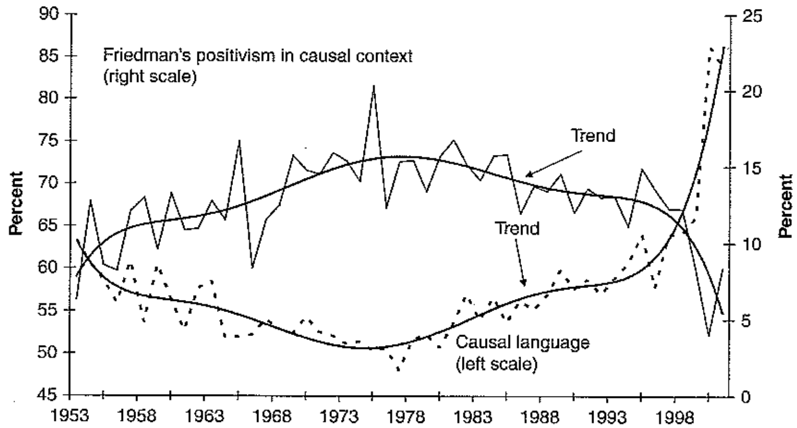

Figure 12.1 The reception of Friedman's positivism and the fate of causal language in econometrics

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 (= "cause," "causes," "causal," "causally," "causality," or "causation") as a fraction of the econometrics family (= "econometric(s)," "regression(s)," "structural model(s)," "estimate," or "estimation"). *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. Heavy, smooth lines are sixth-degree polynomial trends.

against criticism of formalism. That widespread understanding of Friedman's methodology may have had implications for the history of causality in economics.

Figure 12.1 uses the proportion of articles available through the JSTOR journal archive that use a family of terms associated with causality and a family of terms associated with econometrics as a proportion of all articles that use the econometrics family.[10] It shows that causal language fell during the postwar period to reach a nadir in about 1980 and to revive subsequently. The figure also shows the proportion of articles that use "Friedman" and either "positive economics" or "as if" in the same article as a proportion of all articles. The rise and fall of the proportion of such references to Friedman strikingly mirror the fall and rise of causal language in econometrics.

[10] Figure appears as figure 6 in Hoover (2004).

The relationship in Figure 12.1 is itself not necessarily causal. I find it hard to believe that Friedman's own fastidiousness about the use of causal language was either directly influential or even shared widely in the economics profession. But I have argued at length elsewhere that the fall and rise of causal usage is connected to the rise and fall of formalism in econometrics, which is itself closely connected to the rise and fall of formalism in economics generally (Hoover 2004). Thus, to the extent that the common interpretation of Friedman's essay set the stage for overvaluing formalism in economics, we are left with an ironic conclusion: "The methodology of positive economics" is best read as advocating causal realism. At the same time, it was a contributing cause in the suppression of causal language in economics.

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