

Still Trying to Understand Positive Economics

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Milton Friedman, Economics in Theory and Practice, by Abraham Hirsch and Neil de Marchi, Ann Arbor: The University of Michigan Press, 1990. Pp.viii + 325, index. ISBN 0-472-10175-7

Discussion of Friedman's methodology has always centred on "The Methodology of Positive Economics" (1953). The literature on this famous essay is large and still growing - perhaps never in the field of economics has so much been written by so many about so few pages. Why *another* review of that essay? Because Friedman is invoked in most conversations on methodology, and yet after 40 years the issues are still unsettled.

Friedman's essay is evidently easily misunderstood. There is a mass of apparently conflicting reviews, with some reviewers claiming that *every* interpretation other than their own is wrong. The essay has proved, like the sphinx, an enigma - and like the sphinx, Friedman has been just as silent. He recently said that the 1953 essay

has given rise to more comments and attacks than any other I've written. I never replied to any of them, because I decided that I'd rather spend my time doing economics than talking about how economics should be done. (Milton Friedman, quoted in Parkin 1990, p.99)

Friedman wrote in response to the debate over the realism of marginalist assumptions about the behaviour of firms. The essay was originally a continuation of this 1930s-40s controversy, but created a new controversy which overshadowed the earlier debate. His essay now set the agenda. Initially discussion centred on the status of assumptions and Friedman was characterised as a positivist. By the 1970s the focus of the debate had widened to a discussion of methodology, centred on instrumentalism: the view that theory is not descriptive but only an instrument for generating predictions.

Instrumentalism in Friedman's essay was first hinted at by Nagel (1963); Bear and Orr (1967) offered a pragmatic justification for his methodology and first labelled the essay "instrumentalist" (after Popper). Since Wong (1973) and especially Boland (1979) became the standard references, the essay has been generally accepted as the paradigm of instrumentalism in economics.

But there is not complete consensus. Others have argued that the essay is rather (and/or also) positivist, falsificationist or pragmatist. Coddington (1972 and 1979) had recognised Friedman's methodology as "pragmatism or instrumentalism", identifying it with the pragmatism of William James; Wible (1984) characterised Friedman's instrumentalism as a special case of Deweyan pragmatism.

Hirsch and de Marchi reject the positivist and Popperian falsificationist, as well as the generally accepted Popperian instrumentalist, characterisations of Friedman, and characterise him instead as a Deweyan pragmatist. They consider all other reviews (except Coddington 1979) inadequate in accounting for the uniqueness of Friedman's methodology.

The Friedman literature is unsettled because of its particular characteristics. (1) His essay evidently contains enough ambiguities to allow a wide range of plausible readings (and thus create a viable secondary industry producing Friedman reviews). The essay has usually been taken out of context and treated in isolation (2) from the history of methodology, obscuring its relationship to

developments before and after 1953; and also in isolation (3) from the wider body of Friedman's own work. (4) The narrow focus on Friedman as the sole paradigm of instrumentalism has limited methodological discussion; and (5) Popper's hostile definition of instrumentalism has been standard but its adequacy has rarely been questioned (Shionoya 1990 is an exception).

The Friedman debate also highlights more general problems in the history of methodology: (6) there are problems of textual analysis. One phrase may appear at variance with a reading of the total text or of the body of an author's work as a whole: is there one correct interpretation or is the author inconsistent? Add to that Friedman's rejection of self-testimony - then what weight do we give Friedman's own comments on the reviews? And (7) the classifications and terms we use ("instrumentalism", "positivism", "pragmatism", etc.) are slippery enough to allow confusing and inconsistent interpretations; and those within different schools can talk past each other.

Hirsch and de Marchi's book on Friedman's methodology is important because they recognise and deal with these problems more thoroughly than any previous review. Friedman's 1953 essay is considered within the context of his work as a whole, and great care is taken in textual analysis, using his other methodological discussions and his practice of economics to interpret the essay. Terms are carefully defined. Hirsch and de Marchi recognise that Popper's definition is problematic. The "problem of labels" is noted: not only can they be imprecise but they can limit understanding by discouraging further analysis.

The only two problems not directly addressed are outside the scope of this book: other exemplars of instrumentalism, and Friedman's place in the wider history of methodology (although there are comparisons between Friedman and Mitchell, Viner, Cournot and Marshall).

This book was 13 years in the writing, including several papers published earlier, and the care and time taken are evident. The first half is primarily Hirsch's and the second de Marchi's; there is a slight change in style and I found the second half clearer and easier to read. But it is a joint effort, and both claim responsibility for the book as a whole.

By "Friedman's economics" Hirsch and de Marchi mean Friedman's approach to economics rather than his theoretical contributions: the subject is his *methodology* in theory and practice, and his theoretical and empirical work are considered as illustrations of his methodology. They maintain that few economists have been so concerned with methodology as Friedman, even though he wrote little explicitly on it.

Part I (chs.1-6) deals with Friedman's methodology in theory and is an extended analysis of his famous essay. Parts II and III (chs.7 - 12) summarise his method with characterisations and examples, and compare his stated methodology with his own practice of positive economics and political economy. The authors have been even-handed in their appreciation and criticism of his methodological contributions and his practice as an economist. Their starting point is that using the philosophy of science to analyse methodology

all too often ... amounts to labelling in lieu of detailed analysis. ... When working economists ... write about economic methodology they are probably trying to tell us something about their way of doing economics and not to make a contribution to the philosophy of science. (Hirsch and de Marchi 1990, pp.1,2)

They attempt to show that Friedman's methodology is compatible with John Dewey's pragmatic philosophy, although Friedman probably developed the same ideas independently. They maintain that Deweyan "instrumentalism" (i.e. "pragmatism") has little in common with what Popper called instrumentalism; those working within methodological programs other than Dewey's pragmatism (e.g. falsificationism) may thus find it hard to understand Friedman's method.

The authors maintain that reviewers have not recognised the revolutionary impact of Friedman's methodology because of a paradox: he uses a heterodox methodology to support orthodox theory. He defends neoclassical orthodoxy, but with a method that conflicts with the orthodox tradition from Senior to Robbins and contains instead strands of Mitchell's heterodoxy. Thus his defence places a radically different interpretation on the theory he inherited. The authors argue that, despite his current reputation, his methodology still remains unconventional.

For the authors the essential element is pragmatism's dynamic view of science as a continuous process of inquiry, as opposed to the Euclidean, static view of the logic of demonstration. Theory and observation are integrated by the *process* of inquiry.

They contrast the philosophies of science of John Dewey and J. S. Mill to show how revolutionary Friedman's method is, standing against the deductivist tradition. His inductivist approach to logic, and the solutions it brings, conflict radically with Mill's prescription of the *a priori*, deductive method, founded on true assumptions derived by introspection: for Friedman introspection and the realism of assumptions are irrelevant, and theories are judged on their implications.

The authors further put Friedman's method in conflict with positivism and Popper's deductivist falsificationism - for Popper rejected inductivism. Despite their differences, Popper and the positivists are descended from the same tradition as Mill in that they also search for truth and deductive justification. But pragmatism is inductive and looks for workable implications; theories are justified pragmatically, not by deductive logic.

Much more is involved than merely the 'realism' of assumptions ... There are two very different conceptions of science confronting each other, the one Euclidean and the other process. The two are not compatible with each other ... (ibid., p.139).

Friedman's methodology is summarised in 5 propositions: (1) adopt an "outside" view of behaviour [using external observations only]; (2) start with observation; (3) test implications, continuously, although not in order to falsify; (4) use the best knowledge available as a framework in doing empirical research; (5) do not look for answers "in principle", but address concrete problems (taken from ibid., pp.154-8). Theory arises with and out of the data. The process of inquiry is directed to solving a specific problem rather than constructing a general theory; studying sections of the economy piecemeal because we lack the knowledge about interrelationships to build total models. The goal is empirically reliable theory which can make predictions useful for policy, and the ultimate question is whether the theory works - i.e., solves the problem sufficiently well for purposes at hand.

A 3 way conceptual framework is set up. (1) Walrasian abstract formalism: theory as a language without observation, concerned with the realism/plausibility of assumptions; (2) empiricism: observation unconnected by theory; and (3) the middle Marshallian ground: theory and observation combined into "substantive hypotheses". Friedman rejected (1) and (2) and advocated (3). (The authors claim this tripartite framework has been overlooked, but it was used by Machlup (1955) to defend Friedman against the "extreme apriorism" of Mises *et al.* and the "ultra-empiricism" of Hutchison.)

Friedman is characterised as following Marshall rather than Cournot. Two research strategies used by Marshall are made to fit much of Friedman's applied work. First, the "paradigm-stretching model" (Kuhnian puzzle-solving), where Friedman attempted to extend the maximisation hypothesis and Marshallian price theory to accommodate apparent anomalies: e.g. rationalising gambling behaviour, the consumption function or income distribution. Second, the "factors of influence model" (Marshallian action of causes) where Friedman attempted to assign weights to various causes, e.g. detailed episodes in history of money.

They hold Friedman's distinctive methodological contribution to be the thesis that plausible/realistic assumptions are not necessary. The key is his rejection of the "inside" view of behaviour (introspection and self-testimony) and insistence on the "outside" view.

The impact on economic methodology is radical. Since only behaviour and not motives can be observed from the outside, theory deals with unobservables (permanent income, utility functions, "as if" maximisation) which may appear unrealistic or implausible. Moreover, the direction of theory is reversed: instead of reasoning out from certain (plausible) assumptions known by introspection, economists must work backwards from observable implications.

Friedman's methodology is seen as an attempt to solve the problem of unobservables in economics, which further explains the issue of realistic assumptions. If a motive such as

maximisation is unobservable (self-testimony being taken as uninformative) then the maximisation assumption will be "unrealistic" and will not necessarily tally with observation.

The authors show that Friedman's method can be seen in terms of Kuhnian paradigms. Friedman recognised the Duhem-Quine problem: theoretical models are idealisations containing unobservable theoretical terms, and we must operationalise the theory and define the relevant range of phenomena before testing. Hence we test both a theoretical model and a particular specification of an empirical model. Given the implicit valuations, analytical choices and judgments that are made in the process, no proof is conclusive. Instead, Friedman relies on cumulative evidence to reach plausible but tentative conclusions: his predictions are surrounded by qualifications like "minor", "temporary" and "appreciable". (Friedman is charged with inconsistency for not making these qualifications clear in his popular writing.)

Hence, the authors claim that he goes outside the standard deductive-nomological model of explanation and does not try to test in a Popperian way. However, he has also attempted to test predictive performance across paradigms (Keynesian versus Monetarist), something Kuhn did not accept.

The authors indicate two important problems in Friedman's practice of positive economics:

Friedman at times goes beyond empirical analysis and intrudes his own political or value preferences into what is supposed to be positive economics. ... [And he] is not able to come up with predictions that are quantitatively accurate enough for positive policy advice. (ibid., pp. 171-2)

Hirsch and de Marchi view Friedman's political economy as Knightian and note that Friedman's insistence on freedom as a *summum bonum* is inconsistent with the pragmatic ideal of inquiry in which the positive-normative distinction is denied, absolutes are rejected and each action is assessed by its consequences. Hence, the authors conclude that his (pragmatist) positive economics conflicts with his normative (non-pragmatist) political economy: "Some of Friedman's views about the methodology of political economy are almost diametrically opposed to what he believes about how positive economics should be done". (ibid., p.5)

But Friedman himself explicitly uses the non-pragmatic positive-normative distinction in his essay on positive economics (Friedman 1953, pp.3-7). There is no conflict between the methodology of his positive economics and his political economy under a positivist, instrumentalist or falsificationist interpretation, all of which include the positive-normative distinction Friedman uses; but the authors identify a conflict between the positive-normative distinction and pragmatism. The apparent inconsistency they mark between Friedman's positive economics and political economy arises only because of their pragmatic interpretation: it thus remains a critical argument against their central thesis.

However, they also remind us that as much as we'd like to have a concise methodological system, there are critical limits in interpreting economic methodology:

In the pragmatic way of thinking methodology is simply part of the background knowledge ... It is taken for granted and nothing is said about it. ... When he [Friedman] says something of a methodological sort he is trying to tell us about the way he does economics and believes it should be done, usually in opposition to some prevailing alternative, and usually in some specific context. The resulting observations are therefore concrete, directed, partial. To try to make them into general principles may well be attempting to put on them a burden they were not meant to carry and will not bear. (Hirsch and de Marchi 1990, pp.153-4)

The book's strengths are the careful, thorough and wide-ranging analysis of Friedman's work, with a background in the philosophy of science. The authors' principal contributions are that they have given Friedman's methodology a wider context, identified links with the U.S philosophical tradition of pragmatism, and stressed significant themes which have been neglected in most reviews: the "outside" account of behaviour, the problem of unobservables, the importance of induction, the influence of Marshall and the Kuhnian view of theory.

A major problem in the methodological literature in economics is that the relationships between positivism, instrumentalism, falsificationism and pragmatism have not been clarified; when they are

clarified, much of the conflict within the Friedman reviews turns out to be more apparent than real. It is not the case that every other review is inadequate; most have contributed some aspect to the discussion.

Further, the literature has not so far discussed much in terms of behaviourism, an application of the radical positivist "outside" view to the study of behaviour. The authors stress themes in Friedman's methodology like the "outside" view, the problem of unobservables, theory as a language, the symmetry and unity of science theses, and the 3-way framework (formal, empirical, middle-ground), without emphasising their positivist nature or allowing behaviourism as a possible characterisation which explains and links Friedman's "outside" view of behaviour and his approach to unobservables.

Behaviourism, logical empiricism, instrumentalism and some versions of falsificationism are all mutually consistent explanations of Friedman's methodology; a point rarely made (except e.g., Seccareccia 1988, Frazer and Boland 1983). Machlup's (1955) logical empiricist-instrumentalist view of theory (written in defence of Friedman, amongst others) still retains its relevance as the classic exposition of Friedman's methodology.

I find that the argument for Friedman's pragmatism is pushed too hard; some philosophers of science have minimised the distinction between pragmatism and instrumentalism. Conversely, the differences with Popper are over-emphasised; the authors do admit similarities (*ibid.*, ch.7).

The Friedman literature is interesting because it charts the on-going conversation among economists over the methodology of our discipline: his essay remains a focal point but over time the character of the commentaries has changed, from concentration on the realism of assumptions to methodology and now to the history of methodology. The issues have become both wider and deeper; we are still trying to understand ourselves and the nature of our discourse.

This book is an important addition to the debate over Friedman's methodology, and thus over the methodology of economics. It raises interesting questions and opens up new connections within the philosophy of science and methodology, though not all will be convinced by the authors' conclusions. It will not set the controversy to rest, but it does set a new standard in its thoroughness and will reset the agenda for further debate.

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