

for adding nudges to the toolkit of policy makers. But much work still needs to be done in identifying the principles for implementing nudges appropriately and in a way that cuts out the lingering paternalism.

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REFERENCES

- Anderson, J. 2009. Autonomielücken als soziale Pathologie. Ideologiekritik jenseits des Paternalismus. [Autonomy gaps as social pathology: *Ideologiekritik* without paternalism]. In *Sozialphilosophie und Kritik*, ed. Rainer Forst. Frankfurt: Suhrkamp.
- Dworkin, G. 1988. Paternalism: some second thoughts. In *The Theory and Practice of Autonomy*, 121–129. New York: Cambridge University Press.
- John, P., G. Smith and G. Stoker. 2009. Nudge nudge, think think: two strategies for changing civic behaviour. *The Political Quarterly* 80: 361–370.

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The Methodology of Positive Economics: Reflections on the Milton Friedman Legacy, Uskali Mäki, editor. Cambridge University Press, 2009. xvii + 363 pages.

In 2002, Tom Mayer organized a session for the 2003 ASSA meetings in Washington, DC to recognize the 50th anniversary of Milton Friedman's famous 1953 essay, 'The Methodology of Positive Economics' (henceforth F53). The session consisted of a panel of four presenters: Wade Hands, Uskali Mäki, Melvin Reder and me. Friedman was on a conference phone connection, listened to all of the papers and even made a few comments on them. In my contribution to this session, I argued that whether one has a favourable view of F53's instrumentalist methodology depends on one's ideology concerning Friedman-the-man. Specifically, I argued that when many econometricians who are opponents of Friedman-the-man's economics are asked whether they agree with the essential ideas of instrumentalism – but without mentioning Friedman's name or F53 – they will most often say they agree. So the public opposition to F53 often seems rather hypocritical – as their opposition seems ideological rather than intellectual.

Later in 2003, Mäki organized his own conference to discuss F53 and its legacy. The announced list of contributors included Roger Backhouse, Daniel Hammond, Hands, Kevin Hoover, Arjo Klamer, Deirdre McCloskey, Mäki, Mayer, Reder, Chris Starmer, David Teira Serrano, Jack Vromen, Oliver Williamson and Jesús Zamora Bonilla.

The book discussed here includes papers presented at that conference but unfortunately no record of the discussions that took place at that conference. It does include a page-accurate reprint of F53 and it finishes with a one page comment by Friedman-the-man in which he reports: 'I feel like a proud father who has a large brood of bright children – all of them right, all of them wrong, and all entitled to his or her own views' (355).

First, some background: I have a horse in this race. In 1979 I published 'A critique of Friedman's critics', which was about F53. My article was about Friedman's essay and *not* about Friedman-the-man – actually, I had not read any of his other essays before or when writing my article. The purpose of my article was to see what it would take to construct a fair and effective criticism of F53. But the article spent most of its time clearing the deck of unfair critiques of F53. Most of the critiques in question were unfair simply because, in effect, they put words in Friedman's mouth and then proceeded to bash what they claimed he said.

Before addressing the prominent critiques of F53, I provided a reader's guide that explained how F53 was in effect a statement of the well-known *instrumentalist* methodology. As such, F53 was arguing that economists should see their theories and models as instruments to use in forming economic policies and not see them as accurate descriptions of economic reality. That is, F53 says that the realism of the theory's assumptions does not matter; the accuracy of the predictions derived from that theory does. And so, according to F53, a successful test of a theory is the confirmation of its predictions.

Before my article was published, I sent a copy to Friedman who kindly replied with helpful comments which also included a sentence that began, 'As an instrumentalist, which you are entirely correct in describing me as ...'. Later he sent me a copy of a letter he sent to a couple methodologists where he told them that my 1979 article was 'entirely correct'. For me that was extremely important since if you are going to criticize an author's expressed views, unless that author accepts the basis for your criticism, you will only be preaching to the converted and thus not likely to change anyone's mind.

For the record, as I have said in print (e.g. 2003), I have never claimed to be the first to see that F53 represents an instrumentalist methodology (after all, I helped my student Stanley Wong write his well-known 1973 *American Economic Review* article which said F53 is instrumentalist). What I have claimed is that until my 1979 article, most writers referred to F53 as 'positivist' methodology but after my article, almost everyone now recognizes F53 as instrumentalist methodology.

One of the problems that my 1979 article faced was that opponents of Friedman-the-man would willingly accept *any* critique of F53 and so when

I showed all of the major critiques to be unfair as well as wrong, there seemed to be a panic over how to deal with my article. Perhaps critics of F53 would do better to come up with a fair critique that actually addresses F53 as an instrumentalist creed.

Most of the included papers in the present volume are concerned with the history of thought surrounding F53. The major ones are Hammond's excellent history of the writing of F53 and Backhouse's excellent history of the well-known marginalist controversy that preceded F53 and which was likely the reason for writing F53. Several papers are concerned with the history of thought after the publication of F53. While Hands discusses the possible role of F53 in the growth of formalism and Mayer examines F53's effect on subsequent research in mainstream economics, most of the other papers seem to be about marginally related topics and seem just to include a paragraph or two intended to tie the paper to a discussion about F53.

Hands specifically discusses competing views of the question of whether F53's instrumentalism gave license to the growth of formalism in modern economics. He sides with Mayer and criticizes the contrary views of Mark Blaug and Terence Hutchison that blamed F53 for the growth of formalism in modern economics. He says, 'the difference stems from fundamentally different views regarding the function and role of methodological inquiry'. Specifically, 'Blaug and Hutchison approach economic methodology more from the viewpoint of, and with the values of, professional philosophers of science; while Mayer approaches it more from the perspective of a practicing economist' (152). About this, he says that according to the 'approach of Blaug and Hutchison, and for that matter most of traditional philosophy-of-science-inspired economic methodology ... methodology really matters and bad practice must be because of bad methodology. Hence if economics took what seemed to be a wrong turn, then some methodology must have licensed it. F53 is the most famous paper in economic methodology, therefore it must be responsible for the wrong turn' (161).

Mayer did not provide a paper at his 2003 ASSA session but he does here. It is about the general influence of F53 on the profession. In it he reviews the reviews of Friedman's book that contained F53 and he reviews all the Nobel Award lectures. He concludes, 'Although the immediate inspiration for, and the examples given in, F53 are microeconomic, F53 seems to have been more influential in macroeconomics since so much of modern microeconomics consists of building models that explore the implications of various assumptions' (139–40).

One of the most interesting papers here is the one by Teira and Zamora and it is probably the most critical of F53 and its effect on the profession. In consideration of ideology – or what they call 'politics of positivism' – since F53 is saying that models and theories should be judged by their

predictions instead of the realism of their assumptions, they note: 'the sociological turn in science studies raises the question on what basis can we deem a prediction *neutral*? Is it simply that economists produce these positive predictions *disinterestedly*, even while deeply engaged in political debates?' (189–90, emphasis in original). Of course, this raises significant doubt about Friedman's motivation for F53, particularly his seeming preference for the Marshallian versus Walrasian approach to methodology 'on the grounds of its higher political relevance' (190). What seems most objectionable to them is that: 'the way Friedman deals with theoretical concepts in economics in order to obtain predictions makes them particularly susceptible to manipulation by a not so disinterested economist. . . . F53 does not provide any defense whatsoever to counteract this' (*ibid.*).

Despite the announced purpose for the conference being a discussion of F53, too many of the papers seem to be more concerned with discussing Friedman-the-man. As a result, these papers proceed to look either at other writings such as his work with Anna Schwartz (Hoover), with Leonard Savage (Starmor) or his other papers about Marshall or Walras (De Vroey) or their own work that might be relevant such as the theory of the firm (Williamson) or evolutionary economics (Vromen). I must say that had I been invited to this conference I would have been very disappointed with there being so many papers where F53 was not really at issue despite the announced purpose.

Hoover's paper is a version of his July 2003 HES presidential lecture that was about the history of references to causality in econometrics. According to a reported Hammond interview, Friedman-the-man had an aversion to using the word 'cause', so Hoover argues there is an inverse relation between instances of reference to causality in econometric literature and direct or indirect references to F53. Hoover attempts to criticize F53 by turning it upside-down. To do this he focuses on the infamous statement, '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)' (F53: 14). I find it interesting that critics and some proponents of F53 take this statement seriously. After all, it was said tongue-in-cheek.

Hoover's criticism tries to tie F53 to Friedman-the-man's other work. Hoover's purpose is to argue that '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' (305). Presumably, if Friedman-the-man can be shown to be a 'causal realist', then the realism of his Marshallian assumptions can be criticized. But Hoover's argument is not based on

the contents of F53 but instead on Friedman-the-man's advocacy of Marshallian methodology. This, he claims, is because 'Marshall's writings are Friedman's economic Bible' (*ibid.*). Thus he seems to proceed to show that Marshall was a causal realist so that we must conclude that Friedman must be, too. All things said, Hoover's argument is completely unconvincing.

Ironically, Hoover's paper follows Starmer's which seems to give a very contrary view – but based only on Friedman-the-man's work with Savage about expected utility theory. Starmer concludes: 'The simple fact that economics in action ... turns out to be more complex and messy than Positive Economics à la F53 need not necessarily undermine faith in its key methodological assertions. All must surely accept that theorizing necessarily involves simplification and abstraction from complex reality whether it be theory about the ideal behavior of an economy, or theory about the ideal behavior of economists, or whatever. Yet the persuasiveness of Friedman's methodology might be reduced to the extent that his own economics in action were plainly at odds with the prescriptions of F53' (301).

While Reder was one of the participants in the 2003 ASSA session, he did not recycle that paper but instead provided a new paper about the metaphysics of F53 and Friedman-the-man. He calls metaphysics 'frames' – following Kahneman and Tversky's prescription. But, what he calls frames is 'The Invisible Hand'. And he says, 'While I shall not attempt a general discussion of the criteria to use in appraising theories ... logical consistency is an obvious candidate ... The criterion that I shall stress is, I think, underemphasized in F53: consilience of a theory with other [metaphysical?] beliefs, especially those associated with other theories, that have wide acceptance' (173). In this regard, he goes on to say, 'lack of consilience of what economists say about choice behavior with the dicta of psychology and, indirectly, with biology and so forth. This lack of consilience is a major reason, though perhaps not the only one, for Friedman's desire to make prediction about the behavior of aggregate variables – prices and quantities – the primary criterion of theoretical validity, despite the non-realism – non-realisticness pace [i.e., despite the opinion of] Uskali Mäki – of the associated theory of individual choice. All that Friedman – and neoclassicals generally – require of a theory of individual choice is that it be logically compatible with the implications of theoretical propositions about the behavior of prices and related (aggregate) quantities' (173–4).

This brings us to Mäki's chapter – a version of his 2003 ASSA paper published in the 2003 *Journal of Economic Methodology* – the only strictly methodology paper in this book. Interestingly, at the ASSA session, Friedman commented specifically on Mäki's paper saying, in effect, he thought it was silly. Undeterred, nevertheless, Mäki continues to pursue

what on page 62 he calls his 'project'. He says there that he started this project after reading my 1979 *Journal of Economic Literature* article that explains why F53 is Friedman's version of instrumentalism. He says my paper amounted to a claim with two components, 'one about coherence [of F53] and the other about instrumentalism' (62). He says that since 1979 he has been disputing 'both of these claims' (*ibid.*).

One can only speculate as to the motivation for Mäki's 30-year project. Perhaps it is just a fear that by recognizing that F53 is merely a version of instrumentalism and worse an instrumentalist defence of instrumentalism, Friedman-the-man can escape any refutation of his economic pronouncements.

Now, since my 1979 article says that F53 is easily seen to be merely a version of instrumentalism, what would be the opposite? Well, of course, as any elementary philosophy student will reveal, the opposite of instrumentalism is realism. And thus, to dispute my article, Mäki tries to show that F53 *can* be interpreted as a methodology of realism. Of course, if you are willing to be very selective and ignore or even rewrite parts of F53, you could probably interpret any essay such as F53 as anything you want. So what? Moreover, most readers have been convinced by my 1979 article with its explanation of and readers' guide to F53's instrumentalism and nobody but Mäki (and a few Dewey fans who think Dewey has a monopoly on the word instrumentalism) seems to think it wrong-headed.

The final paper is by Blaug, although he was not one of the announced participants in Mäki's conference. It is ostensibly about F53 although, as with many of the participants, it is not always clear whether the issue is the contents of F53 or the methodological beliefs of Friedman-the-man. And it is not clear why this paper was included since it adds little. Perhaps it is because it seems to give some vague support to Mäki's project although it often contradicts such support. It is too bad that Blaug did not take the opportunity to answer my (1985) published question concerning why he changed from calling Friedman a 'positivist' in 1978 to calling him an instrumentalist in 1980. Instead, all we get is a statement on page 350 that 'it was all too easy to agree with . . . Boland (1979)'.

Further down this page Blaug says something very strange. First he says, 'Friedman's frequent appeal to as-if reasoning lacks any grounding in a commonsense realist interpretation' but then goes on later to say he sides 'with Mäki and against Boland in this because Boland's reading implies an uncommonsensical philosophical sophistication on Friedman's part that rings false' (351). There is no such implication. I never ever claimed that Friedman-the-man was capable of engaging in 'philosophical sophistication'. Moreover, if anything, instrumentalism is the least sophisticated philosophical position one could take in methodology. But I made no such claim either way. All I was saying about

F53's being a straightforward instrumentalist methodology was that 'if it walks like a duck, and quacks like a duck... then it's a duck', nothing more.

For a conference volume, this book contains some worthwhile essays about the history of F53. Unfortunately there is also recycled material and some papers spiced up with unnecessary critical comments that seem unfair to this reviewer.

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REFERENCES

- Boland, L. 1979. A critique of Friedman's critics. *Journal of Economic Literature* 17: 503–22.
- Boland, L. 1985. Reflections on Blaug's *Methodology of Economics*: suggestions for a revised edition. *Eastern Economic Journal* 11:450–454.
- Boland, L. 1997. *Critical Economic Methodology: A Personal Odyssey*. London: Routledge.
- Boland, L. 2003. Methodological Criticism vs. ideology and hypocrisy. *Journal of Economic Methodology* 10: 521–526.
- Mäki, U. 2003. 'The methodology of positive economics' (1953) does not give us the methodology of positive economics. *Journal of Economic Methodology* 10: 495–505.
- Mayer, T. 2003. Symposium: Fifty years of Milton Friedman's 'The methodology of positive economics'. *Journal of Economic Methodology* 10: 493–530.
- Wong, S. 1973. The 'F-twist' and the methodology of Paul Samuelson. *American Economic Review* 63: 312–325.

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Across the Boundaries: Extrapolation in Biology and Social Science, Daniel P. Steel. Oxford University Press, 2007. xi + 241 pages.

The problem of extrapolation is of as much philosophical interest as it is of practical significance. We often cannot experiment directly on populations whose wellbeing we ultimately care about because of ethical, technological or financial constraints. When we cannot experiment directly on a population of interest and nevertheless seek experimental evidence about that population, we must experiment on a different but related population and infer what we want to know about the former from what we do know about the latter. This is Steel's core question: what are reliable rules of inference from an experimental population to a target population of interest? Steel's is the first monograph-length study of this question, and it fills an important void.

Suppose you would like to establish the efficacy of a new drug or welfare programme for an actual population such as that composed of