

selves, they would short-circuit many of the steps involved in the calculations.

In this respect, Skyrms' theory is open to the same sort of objections as are raised against the tracing procedure, either in the original form as propounded by Harsanyi or in its fortified form as presented in the treatise by Harsanyi and Selten. Despite the problems, such models play an important role in understanding strategic interaction. There are many contexts in which we wish to investigate strategic interactions among boundedly rational individuals. Evolutionary game theory has taken a firm foothold in economics, and the burgeoning literature on games played by finite automata can also be seen as resting on the same view of games. It has long been argued that boundedly rational players produce outcomes that are closer to the outcomes observed in practice. The large literature on cooperation in the finitely repeated prisoners' dilemma is just one instance of this. It could be argued that game theory can have explanatory power only if some grains of sand are cast into the excessively well-oiled wheels of classical game theory. In this context, Skyrms's work will prove invaluable.

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Milton Friedman: Economics in Theory and Practice, ABRAHAM HIRSCH and NEIL DE MARCHI. Ann Arbor: The University of Michigan Press, 1990, viii + 325 pages.

Abraham Hirsch and Neil de Marchi's *Milton Friedman: Economics in Theory and Practice*¹ is a book of an unusual sort and, in this writer's judgment, of unusual interest. Friedman's *Essay on "The Methodology of Economics"* (1953) is known to be the single most influential piece of the postwar era in the field of economic methodology. Its popularity is in part a bootstrap phenomenon – the more commentaries the *Essay* has evoked over the years, the more compelling it has become for anybody in the field to have something to say about it. The features of this proliferating secondary literature are themselves well known by now. First, it is mainly a-historical and even a-contextual, relying as it does on the antecedent philosophical literature rather than Friedman's economics, not to mention the general economics of his time. Second, it is characteristically ungenerous, both to Friedman himself and to the fellow

1. Unless otherwise stated, all references will be to this book.

commentator's work. This feature is best seen as a derivative one, although it bears some relation to the sociological structure of the debate: nastiness is an expedient way of being witty in a crowd.

I wish to emphasize from the start that Hirsch and de Marchi's work does *not* belong to the Friedmanian secondary literature of the above type. It offers a decidedly contextual account of the *Essay*. Not only do the authors connect Friedman's pronouncements to his earlier work as an economist and to some likely influences of his intellectual milieu around 1953, but they also discuss a polemical connection with the long-run methodological tendencies of the economics profession. These, Hirsch and de Marchi submit, are basically in keeping with John Stuart Mill's deductive method. What they claim to be important and even "revolutionary" (p. 2) about Friedman's methodology is that it breaks away from the watered-down forms of apriorism that still prevailed among prewar economists. The assessment that results from such and other highly contextual considerations is soothing – some notorious Friedmanian paradoxes are disentangled and his commentators' contributions are by and large superseded rather than nullified.

These results are reached as early as the middle of the book. For there is more to it than another, even exceptionally competent discussion of "realism of assumptions," "as if reasoning," and similar famous themes. Roughly speaking, Part I is concerned with Friedman's reflective and explicit methodology, whereas the remaining chapters attempt to capture his method at work. Part II explores the latter in the context of the major theoretical achievements, while Part III pleasantly extends the investigation to more transient pieces in "political economy." Hirsch and de Marchi's ultimate goal is to provide the reader with a coherent and encompassing picture of Friedman's way of doing economics: they "used what he did in interpreting what he said, and vice versa, mirroring the iterative and interactive process that [they] have identified in [his] own writings" (p. 153). To what extent have they succeeded in this ambitious task?

A TENTATIVE PICTURE OF FRIEDMAN'S DOCTRINE

Chapter 7 usefully summarizes the results reached after Part I. There are five major methodological rules (pp. 153–60):

1. "Adopt an 'outside' view of behavior."
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."

What is most remarkable about this list is that it consists of relatively unremarkable propositions. Although it is made in a subdued tone, this should count as a major conclusion: barring a certain air of paradox that can be found only in the 1953 *Essay* and is best explained contextually, there is nothing strange, let alone completely original, in Friedman's methodological doctrine. Supporting evidence for this (itself paradoxical) claim can be found in Chapter 6 on pragmatism, a rather unexciting philosophy with which the authors have discovered a significant connection. They particularly discuss John Dewey, explaining by means of well-chosen quotes how he antedated (although did not influence) Friedman on Propositions 1, 2, 3, and 5. Proposition 4 raises special problems, as will be seen later. The unassuming appearance of the list does not mean that it should be taken for granted. After all, to take the authors' word for it, Friedman's doctrine is a special variant of pragmatism, which is itself just one brand of empiricism, with which not everybody agrees, especially in the field of economics. Propositions 1 to 5 have a significant import when they are put in the proper, *polemical* perspective.

Maxim 1 first of all expresses Friedman's rejection of introspection, a recurrent theme in his methodology (pp. 74–76, 80, 124). The authors extend its meaning to cover a similar rejection of the well-established tradition in economics of reaching conclusions by means of plausible arguments only (pp. 45–46, 73–74). The common theme of the two rejections appears to be that subjective assent, be it the individual feeling of certainty or intersubjective agreement, is neither sufficient nor necessary to secure economic knowledge. Such knowledge has to be grounded in outside observations, a point that is, of course, crucial to the proper understanding of utility theory (p. 69). Another important consequence is that Mill's deductive method and its watered-down variants in economics ("reasoning out" from already known premises) should be rejected too (chap. 5).

Maxim 2 is intended to confirm Friedman's break-away from the mainstream methodology. Its pale appearance is deceiving, for what it really means should perhaps read: "start with *as many* observational data as possible" (see Friedman's quotation on p. 156 and the authors' comment on p. 103). The recommendation has a specifically anti-Millian flavor: It is implicitly critical of the practice of *first* studying the effects of causes taken in isolation and *then* balancing these effects against each other. Because the "principle of successive approximations" has such a well-established standing in economics, maxim 2 is invested with a strong polemical import (chap. 5). In the same vein, Hirsch and de Marchi could perhaps have emphasized that Friedman should logically disagree with *most* of the work currently done in economics under the label "modeling." This point will become clearer when maxim 3 is added. Another, rather obvious, side of maxim 2 is its anti-Popperian implications. In keeping with pragmatism, Friedman is normative about

the origin of scientific hypotheses. Accordingly, he rejects any distinction whatsoever between the context of discovery and the context of appraisal; the repeated use in Hirsch and de Marchi's book of Dewey's catchword, "inquiry," is meant to convey that the notorious distinction has been superseded (see chap. 6).

Again, the advice "test implications" in maxim 3 is not as trite as it would seem. Friedman is well aware that hypotheses could only be tested against facts outside the set used to derive them (p. 69). In view of the previous requirement that one should start with as many observational data as possible, an even more relevant concept of empirical testing would involve *truly new* facts, that is, facts unknown when the hypothesis was put forward. Be that as it may, Friedman's conception rejects *ad hoc* explanations, an ever relevant attitude to take in economics. The requirement that implications should be "continuously" tested is illuminated by Friedman's early polemic against Oscar Lange (1946). Lange, of course, stands for the mathematical ("Walrasian") economist in general, who is said first to build a formal apparatus, then to interpret it, and finally leave things at that. By contrast, good economic research involves empirical reality at every step; and there are many such steps (p. 27). Interestingly, Lange could have been used already to exemplify the earlier requirements. For Friedman also takes him to task for discounting certain causal influences on the basis of plausibility only (cf. maxim 1) and for dealing with his target evidence – price-inflexibility – in an exceedingly narrow way (cf. maxim 2). I regard the added clause "although not in order to falsify" as somewhat infelicitous. Even a Popperian scientist would not spend all of his time criticizing his competitors' hypotheses. He would be expected to test *his own* hypotheses certainly not "in order to falsify it," but to have them pass the test, providing that the latter is a challenging one. This is, of course, what "corroboration" is about. I dwell on this elementary point because one sense in which Friedman might be read to be "interested in confirmation rather than falsification" (see p. 213) is that he is not interested after all in *severe* tests, or so I shall argue in a while.

Maxim 4 suggests that Friedman would like to distinguish between the general framework of economic analysis, typically neoclassical economics of the "Marshallian" brand, and the specific hypotheses that will undergo the iterative process summarized by maxims 1, 2, and 3. There is definite evidence that Friedman has entertained such a distinction even at the early stage of the *Essay*. The problem is whether or not he intended it to shield the "framework" from application of maxims 1, 2, and 3. All through the book, this problem is an embarrassment to Hirsch and de Marchi. At one place they claim that Friedman "must believe that (Marshallian theory) has gone through the same process" as lower order regularities (p. 62, my emphasis). This is, of course, a purely logical point. There is no textual evidence to substantiate it. On

the other hand, there is definite textual evidence that Friedman was prepared to support Marshallian economics on *other* methodological grounds than the growth-of-knowledge doctrine sketched thus far. Recall the famous sentence of the *Essay*: "Confidence in the maximization of returns hypothesis is justified by evidence of a different kind" (1953, p. 22) and the ensuing passage on the billiard player, the growing leaves of the tree, and the surviving firm argument. The surviving firm point, for instance, amounts to a *direct and (only) plausible* defense of the maximizing hypothesis (as against a tested implication). Hirsch and de Marchi acknowledge, and even carefully analyze, the discrepancy created by this notorious passage (pp. 95–100). There seems to be no way of escaping from this acknowledgement strategy. It has, however, two serious drawbacks for the authors: Not only does it weaken the coherence of the reconstructed doctrine, it also locally severs the alleged link with Dewey and pragmatism.

Maxim 5 echoes the usual Friedmanian point (which we are taught was made as early as 1941) that a good hypothesis is one that helps resolve the particular problem at hand. There are two sides to this coin. On the one hand, the hypothesis itself must be well chosen, and there we find ourselves on a much trodden path: simplicity is more important than generality, etc. On the other hand, there is the more subtle consequence that the *desirable fit* between the hypothesis and the adduced evidence is problem-dependent (pp. 251–52). What the "problems" are does not emerge very clearly from the early methodological work. The easiest route is to argue, as Hirsch and de Marchi tend to do in Parts II and III (e.g., p. 184), that Friedman had primarily in mind the problems faced by the policy-maker. This would suit the interpretation of Friedman as a *pragmatist*. However, it strikes the present writer as equally defensible to define the "problems" as the particular targets of the analysis, whether or not the latter is policy-oriented. For instance, the joint article with Leonard Savage in 1948 (which Hirsch and de Marchi rightly describe as pivotal) aims at inserting the whole spectrum of individual attitudes toward risk in a revised Marshallian framework. Nothing here would suggest that the analysis is policy-oriented. What is truly remarkable about it is that it is so narrowly focused. Friedman and Savage pursue their aim relentlessly, as it were, without paying attention to the broader context of Marshallian economics, where it made much sense *not* to assume differences in individual risk attitudes (if only because the constant marginal utility of money serves in the derivation of the law of demand). If this intellectual strategy needs a simple label, it may be *instrumentalism*, notwithstanding Hirsch and de Marchi's strong reluctance to apply it to Friedman (pp. 85–88, 143).

This reconstruction dispels many obscurities in the *Essay*. The major example is *realism*. Hirsch and de Marchi write boldly: "If one cuts through the verbiage about 'realistic' assumptions, both Friedman's and

his critics', and gets down to the bedrock of differences in methodological beliefs, one finds that this matter shrinks into insignificance" (p. 80). Nagel, and many after him, had taught that "unrealism" in the *Essay* had an embarrassingly wide range of meanings. Hirsch and de Marchi argue as follows: Friedman is primarily interested in rejecting introspection and plausible reasoning as bases for substantiating or criticizing assumptions; once it is made the focus of the *Essay*, this simple polemical point takes care of *all* of the disparate meanings of "unrealism" for which a case can be made (pp. 73–80). There is one sense of the word that cannot be redeemed: falsity (recall the passage on "wildly inaccurate descriptive representations of reality," 1953, p. 14). But as the authors show, this sense does not significantly occur in Friedman's methodological work *at large*. On the other hand, the following intuitive notions of "unrealism" are redeemed: (1) the lack of subjective certainty, (2) abstraction from certain empirical features, (3) the use of unobservable concepts. As usual, Hirsch and de Marchi argue their thesis on the basis of the methodological environment (in this instance, Mitchell's and Viner's earlier half-baked attempts to deviate from the Millian orthodoxy) as well as of Friedman's own intellectual history. In the 1948 article on expected utility, the disparate meanings of "unrealism" happened to coincide in an economically justifiable way. Part of the hermeneutical problem of "The Methodology of Positive Economics" is that it is such an out-of-focus repetition of the critical points made earlier, above all in the Friedman-Savage article.

FROM METHODOLOGY TO METHOD

There are several ways in which the highly competent survey of Friedman's mature work in parts II and III can be shown to undermine the tentative picture of the middle of the book. Curiously enough, I shall *not* take the authors to task for the "two recalcitrant loose ends" (p. 4) that they admit have worried them. (The first one is the alleged discrepancy between Friedman's methodology and his work as a political economist. The second one relates to the fact that Hirsch and de Marchi provide little analysis of the various technical steps needed by Friedman to carry out an econometric test.) What has struck me as more troublesome is that there are *systematic* deviations between Friedman's best work and the attempted picture of his methodology.

Take, first of all, the Deweyan growth of knowledge doctrine sketched earlier – maxims 1 to 3 – and compare it with the Friedmanian research program on utility, occupational choice, and the distribution of income. This program began in the early 1940s with joint work with Simon Kuznets and ended up with an article published in 1953. Friedman started his inquiry with outside data – statistical figures on "income from independent professional practice" – but perhaps not with an *extensive* set of them. In 1945, he basically restricted his attention to average

income differences, leaving unexploited other interesting statistical features that he had recorded (p. 177). Admittedly, Friedman was tenacious, since he was to return to the discarded features later on and finally managed to provide an explanation for them along the lines of expected utility theory (p. 193).

Roughly speaking, his explanatory scheme combines "actuarial factors" (e.g., the different duration of professional studies) with the assumption that there is one and the same choice set facing the individuals, so that they are distinguished from each other only by their attitude toward risk. This broad scheme could be turned into an explanation of the remaining statistical features by means of various auxiliary assumptions (pp. 182–83). But there was *no independent check* of either the main or the auxiliary assumptions, only a vague appeal to further empirical research (p. 183). Thus, the whole sequence was empirically successful only to the extent that it provided an account for the set of initially available data. Exactly the same critical point can be made (and is, in effect, made by the authors) in relation to the mini-research program pursued on expected utility. Now, if we move to the altogether different field of monetary history (pp. 232–43), the suggestion becomes irresistible that Friedman's method, as against his alleged methodology, can be at peace with *ad hoc* explanations. Interestingly, any time that the double check of hypotheses is missing, Friedman lapses into appeals to *plausibility*: see pp. 182–83, 212, 244, and 196 on the role of the wealth effect.

I have already made something of the problems raised by the testing of the broad theoretical framework (as against specific hypotheses) – see maxim 4. It is instructive to compare these problems with what Hirsch and de Marchi have to say about permanent income (pp. 195–203). It is not clear to me whether Friedman's statistical specifications of the permanent income hypothesis are intended to make the latter a testable hypothesis or to be *themselves* testable. I have to admit incompetence here. But take the already mentioned assumption that the individuals face essentially the same opportunity set and differ only by their risky behavior. This assumption acts as an organizing scheme; it cannot be made testable without adding heavy auxiliary assumptions that will actually bear the brunt of the test. This is but one example of the *meta-physical* statements that guide Friedmanian economics all along.

I think that the authors would be better off if they recognized that the Deweyan process of successively revised conjectures does not apply (even "in principle," p. 157) to such statements, not to mention the maximization-of-returns hypothesis and other building blocks of Marshallian economics. Hirsch and de Marchi have coined the nice expression of *paradigm-stretching* (p. 165) to describe Friedman's practice of preserving endangered categories – utility, income, the velocity of money – while distorting them. This practice is most typical of *both* Friedman's method and his methodology. But to bring it to the center

stage, Hirsch and de Marchi would have to sever their favorite pragmatist connection.

A somewhat similar critical point applies to the remaining maxim 5. Even after the careful survey of Friedman's mature work, the notion of "the problem at hand" remains a blank that can be filled in various ways. This is worrying in view of the fine point made by the authors that not only the hypothesis, but also the fit between the hypothesis and evidence, should be seen as problem-dependent; that is, we do not really know what "a good prediction" is. At many places, Friedman makes it plain that the problem under discussion is to compare two kinds of policy interventions with each other or to compare active economic policy with *laissez-faire*. The accompanying notion of "a good prediction" is plausibly one of the comparative statics sort that is stable across institutional arrangements. However, things are not always as clear-cut as that. For instance, when Friedman writes: "Our aim is to compare the quantity theory with the income-expenditure theory, not with a joint theory" (quote on p. 210), he is pointing to a purely *theoretical* purpose of analysis that is sufficient to constrain the relevant kind of predictions. The organizing concept here is that of *domain of application*; and it seems to have an instrumentalist flavor rather than a pragmatist one.

It goes beyond the reviewer's task to suggest how maxims 1 to 5 could be modified to tighten the connection between methodology and method. But I should be emphatic that all of the relevant material appears to be there already (see the partial retractions on pp. 184–85, 202–3, 243–44, 267–68). Perhaps it has by now become clear along which general lines an encompassing interpretation is forthcoming: The pragmatist sketch of a growth-of-knowledge doctrine should be made to accommodate Friedman's mitigations of empiricism, as well as his related, highly idiosyncratic practice of "paradigm-stretching." I take it that successful interpretative work – and Hirsch and de Marchi's outstanding exploration comes very close to that – always has to strike a balance between coherence, coverage, and originality. The Friedman who emerges from the authors' explicit picture is not so strong on originality and coverage as he is on coherence. But their book is rich enough to suggest alternative trade-offs. This author's preference toward weakening coherence will not go as far as to destroy what has emerged as the hard core of the reconstruction – Friedman's break from the "reasoning out" procedure of standard economics. Perhaps the single most impressive discovery made by Hirsch and de Marchi is that postwar economics – ours – is so deeply rooted in nineteenth century methodology as to make Friedman's half-baked adherence to empiricism a truly "revolutionary" doctrine.

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A Theory of Property, STEPHEN R. MUNZER. New York: Cambridge University Press, 1990, x + 491 pages.

According to Alan Ryan, who is quoted on its jacket, this book is "an encyclopedic treatment of the theory of property rights that does justice to almost all the conceptual, legal, political, and social issues at stake." It may be seen, indeed, as a *Summa* of arguments about property. Furthermore, if one considers its constant attempt to integrate all points of view, to maintain a fair equilibrium between them, and to reduce their conflicting character in such a way that all of them participate in a fully satisfactory solution, it is really akin to medieval *Summae*. Philosophically, the author is overtly pluralistic (pp. 3, 293, etc.). This is not to say that he is content simply to lump together various contradictory views about property. Far from this, Stephen Munzer is always very conscious of the antithetic aspects of the various theories he is analyzing and never tries to underrate such aspects. If, while offering keen criticisms of each of these conflicting views, he maintains his pluralist credo, it is because, according to him, each of these views can "play a role" (p. 289) in the equilibrated conception of property developed in this book.

A Theory of Property is divided into four parts, but it is in Part III that the various principles that bear on the question of property are scrutinized and literally mobilized to serve in the new pluralistic theory. These principles are reduced to three headings, each with a double entry: utility and efficiency, justice and equality, labor and desert. At first glance, this list might seem odd: if labor and desert were the basic principle of Locke's classical justification of property and if utility and efficiency played a similar role in Hume's equally classical justification, an attempt to justify property with the help of a principle of justice and equality (at least, when justice is associated with equality rather than with desert) is surely more unusual. However, this oddity disappears when Stephen Munzer's approach to property is correctly understood. For him, the three basic principles are indivisibly "principles to justify and *limit* property rights" (p. 191, emphasis added). It is not simply a matter of justifying property; it is a matter of assessing an institution that requires both to be justified *and* to be limited. For Munzer, it would be unacceptable to justify such an institution without describing how to neutralize its negative effects