Book Review Column

Friedman’s Stance Towards Economic Orthodoxy: A Review of Hirsch and de Marchi’s *Milton Friedman, Economics in Theory and Practice*. Ann Arbor: University of Michigan Press, and Hemel Hempstead:

Maarten C.W. Janssen
Erasmus University Rotterdam

Has not everything there is to say on Milton Friedman’s work in economics been said? His Monetarism is taught to students, his (and Anna Schwartz’s) Monetary History of the United States is the subject of a continuing debate, his Permanent Income Hypothesis has perhaps been the most frequently tested hypothesis in economics and his Methodological Essay has been the most discussed essay in economic methodology. Of course, most aspects of his work are controversial, but is his work not so familiar to all proponents and opponents (and does not almost every economist belong to one of those two categories) that a new book on Friedman’s methodology and his positive economics is superfluous? This is the challenge Hirsch and de Marchi are confronted with and the present reviewer has to admit that they meet this challenge: the authors give many fresh insights and new perspectives on how both Friedman’s positive economics and his essay on economic methodology might be interpreted.

The book is a good example of the more modest tendency that is developing in economic methodology. The normative position (from a supposedly superior philosophical position telling economists how they should practice the discipline) is largely abandoned in favor of a descriptive position in which the implicit methodological attitudes of working economists are made explicit. The combination of history of economic thought and methodology that can be found in this book is a sign of this new development. The authors attempt not to put a label on Friedman and the reader who is looking for the final (!) interpretation of Friedman’s work will probably be disappointed. Instead, the book should be interpreted as Hirsch and de Marchi’s contribution to the discussion on Friedman’s methodology and his positive economics.

What makes the book unlike most other works on Friedman is that it combines a discussion of his methodology and his positive economics and that it confronts the two with each other. By doing so, the authors are able to put the Methodological Essay in perspective of his other work. The book is in three parts. Part I is mainly on the famous 1953 essay *Methodology of Positive Economics*. It traces positions defended in the essay in earlier writings on positive economics. To clarify Friedman’s methodological position its pragmatic foundations are illustrated by comparing Friedman and the pragmatic philosopher John Dewey. Another way by which Friedman’s unorthodox methodology is clarified in this first part is by emphasizing the differences with John Stuart Mill’s *Logic*. Part II discusses Friedman’s contributions to positive economics, notably his attack on Keynesian economics and his Monetarism. The focus in this part is on the question whether his positive economics is consistent with the methodological tenets outlined in the 1953 essay. Two questions come up many times in this part. First, according to Friedman’s methodology a good theory is a theory that makes good predictions. So, one might expect that in his positive economics Friedman is also mainly interested in the predictive aspects of science. The natural question then is what predictions Friedman actually made and whether these predictions (if any) are corroborated. Second, Friedman has also been very active as a political economist and one can ask oneself to what extent his positive economics (or, for that matter, his methodology) is framed by his political views. These two questions are considered as “recalcitrant loose ends” (p. 4) and discussed in part III of the book.

After giving some details of the insights Hirsch and de Marchi’s contextual account of Friedman’s Methodological Essay yields, I want to take up some of the other points that are made by the authors and provide a slightly different interpretation. By doing so, I hope to be truly in the spirit of their book: adding some new elements that might help complementing the picture we have of Friedman’s work. More specifically, I will deal with (i) the role of theoretical terms in Friedman’s applied work, (ii) the relation between Friedman’s *Monetary History* and Dewey’s pragmatic philosophy, (iii) the similarities between Friedman and Mill, (iv) the role of predictions in Friedman’s positive economics.
1. Friedman's Methodological Essay in Relation to his Positive Economics

Much of the debate on Friedman's methodology centers around the following famous phrase: "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)" (quoted on p. 73). As noted also by others, there are many ways assumptions can be (un)realistic. The advantage of Hirsch and de Marchi's approach to the question what Friedman might have meant by the above quoted phrase is that they are able to relate the discussion on the realism of assumptions to Friedman's own positive economics. At first, we are presented with an apparent paradox, because if one looks at Friedman's positive economics one cannot but be impressed by the detailed study he makes of the data. How can an economist for whom, in his positive economics, detailed study of his empirical material is very important be claiming, in a methodological piece, that assumptions have to be unrealistic to be truly important?

As a collaborator of Mitchell at the NBER, Friedman has been engaged in extensive empirical research. Unlike Mitchell, however, Friedman felt the need to go beyond the data in order to explain why the data are what they are. In "reality" there are many (only indirectly observable) mechanisms at work and it is only if we go beyond the data that we can try to unravel the particular mechanism(s) at work. In order to see what Friedman might have had in mind let us have a closer look at Friedman and Savage (1948). In their article, Friedman and Savage observe that people seem to make risky choices more often than standard utility theory with plausibly looking utility functions suggests. The question then is whether this behavior can be consistent with utility theory. In their article they show that consistency can be obtained, but at the expense of a utility function that does not look very plausible at first sight. They go on to argue, however, that this strangely looking utility function can be given a plausible interpretation.

This brief outline of the Friedman and Savage article suggests two points on the realism of assumptions issue. First, when Friedman observes that truly important hypotheses have unrealistic assumptions, he probably should not be interpreted as saying that assumptions should be descriptively false, because if they were then the hypothesis in question would simply be false. However, assumptions might appear to be false at first sight. Second, Friedman rejects the at that time prevailing idea that we have direct access (through introspection) to the truth of some fundamental hypotheses. Hirsch and de Marchi convincingly show that this interpretation of the Friedman and Savage article explains why it does not matter that the utility function they used seemed implausible at first sight and why, in the same article, they were also engaged in rationalizing that a utility function might have such a strangely looking form after all.

There are two other features of the realism issue mentioned by Hirsch and de Marchi that have to be treated here. The first is that in doing theory one has to abstract from certain features of the world and as one cannot know in advance whether a particular departure from descriptive realism is acceptable or not (note again that Friedman rejects introspection as a means to obtain knowledge about human behavior), the only criterion that is left is that the implications of the hypotheses are in line with our data. A second point, more closely related to Friedman's own positive economics, is that he continuously emphasized that we know all too little about the working of an economy. In general, there are so many factors at work that it is impossible to build a general model based on realistic assumptions that can be fruitfully applied to real questions. (This is also why he considered the Walrasian approach inadequate). Instead, Friedman argues that it is only through a detailed study of concrete cases that we can determine the relevant causal factors for that particular case.

2. Going beyond the Data: Theoretical Terms in Friedman's Applied Work

Above we have seen that Hirsch and de Marchi argue extensively that in his time Friedman was an unorthodox economist. In the forties, the discipline's methodology was to a large extent Millian in nature and applied work was not highly regarded. For Friedman, however, looking at the data was an essential ingredient of doing serious economic analysis. But, what exactly is the role of data analysis in Friedmans' work and what is the relation between data and theoretical constructs as "permanent income" and "utility"? At different places in their book, Hirsch and de Marchi provide us with parts of an answer: Theory gives us "a framework for deriving new theoretical insight with the help of lengthy and careful investigation of the data" (p. 26), "the empirical worker must go to the data as part of the process of deriving the concepts he needs in his empirical work" (p. 44), "when one theorized, on Friedman's view, one has to go beyond the data... and imagine a simple pattern that could account for the data; such a pattern was not 'real' because it was not in the statistics, even though it might be interpreted
as implied by them” (p. 50-1), we need comprehensive information to see what are the phenomena to be explained (p. 156), “observation, the derivation of hypotheses, the testing of implications and the use of revised hypotheses in generating new, testable implications, succeed each other in a never-ending round” (p. 156-7).

From the above, the reader gets the impression that Hirsch and de Marchi believe that for Friedman data analysis was not only essential to see what there is to explain or to test the implications of hypotheses, but also to suggest new theoretical constructs and hypotheses in which they occur. It is true that Friedman was unorthodox in the sense that he does not start from some assumptions that are held to be true, but, in my view, it goes too far to suggest that the theoretical constructs that are used to interpret the statistics are implied by them. In line with Popper, I think that the discovery of new hypotheses is a creative act that is not implied (at least not in the ordinary sense of the word) by the data. As Hirsch and de Marchi rightly point out there is, however, also something distinctively un-Popperian in Friedman. He doesn’t go out to try to falsify a hypothesis. A disconfirming test is for Friedman only a sign that something important has been left out and a next step is then to come up with a revised hypothesis that accounts for the disconfirming evidence. In this way, there is a “never-ending round”. The revised hypothesis is, however, also not implied by the data.

3. Friedman’s Monetary History and Dewey’s Pragmatic Philosophy

In the previous section, I have briefly noted that there is something non-falsificationist in Friedman. One way, Hirsch and de Marchi try to account for this un-Popperian feature of Friedman is to relate his method of inquiry to John Dewey’s pragmatic philosophy. Dewey took seriously Hume’s idea that we never can be certain about empirical generalizations. He concluded from this, however, that we should not look for truth and certainty, but instead for a high degree of probability. An important notion in his thinking is that of abduction, a notion he borrowed from Charles Peirce. Dewey was fully aware that we cannot infer that p is true when we only know that p→q and q are true. We can infer, however, from p→q and ¬q (not-q) that p cannot be true. Dewey (in line with Peirce) argued from this that when we know p→q and we observe q that, although perfect certainty about the presence of p cannot be obtained, there is some evidence that p was indeed the case. This method of reasoning is called abduction.

Looking at Friedman and Schwartz’s *A Monetary History of the United States* (1963) and taking Hirsch and de Marchi’s idea serious that Friedman is engaged in some sort of abductive reasoning, it is tempting to interpret p as the fluctuations in the money stock and q as fluctuations in nominal income. The abduction involved would then be as follows: we know that over a long period there have been substantial fluctuations in the level of nominal income, we also know that if there had been “corresponding” fluctuations in the level of the money stock, the substantial fluctuations in the level of nominal income could be accounted for, therefore we have reasons believe that there have been “corresponding” fluctuations in the level of the money stock. Friedman could then, in a next step, consult the statistics to see whether the money stock has been subject to fluctuations corresponding to those in the level of nominal income. Looking at *A Monetary History*, however, does not reveal this abductive pattern. On the contrary, Friedman and Schwartz start out by examining the available data on nominal income and the money stock and the purpose of their analysis is to see whether the one implies the other. (So, unlike abduction, Friedman and Schwartz’s process of inquiry does not presuppose knowledge of the relation between p and q). As we all know, Friedman and Schwartz (1963) concluded from their extensive analysis that money causes income. So, if we put their reasoning in a logical scheme, it seems more appropriate to represent their argument in the form of another type of logical fallacy, namely p, q→p→q.3

How do Friedman and Schwartz make their case; how do they conclude that “money causes income”? Hirsch and de Marchi mention three points: “First, one identifies statistical regularities which maintain their features across time...Next, one selects a potential causal factor or factors, based on timing and analysis of those elements that could be necessary and sufficient to account for significant changes in the variables judged to be dependent. Finally, one makes sure that the postulated cause(s) are in fact historically present and active in the way supposed” (p. 229).

In view of the observations made above, the third step seems to be superfluous, because it is already known (from examining the data) that the potential cause(s) is, in fact, present. Also, the quote suggests that the analysis is more clear-cut than it actually is. What Friedman actually does is dividing fluctuations in nominal income in major and minor changes. For the major changes he shows that the changes in the money stock can be regarded as
autonomous with respect to changes in nominal income. For the minor changes, however, the relation between money and income is not very clear and these changes do not confirm the general hypothesis (that money causes income). From a truly Deweyian point of view, one would expect Friedman to investigate how his hypothesis can be adapted in order to account for the disconfirmations. However, Friedman never gives up his preferred hypothesis. He simply says “clearly, the view that monetary change is important does not preclude the existence of other factors that affect the course of business” (Friedman, 1969, p. 222) and he adds somewhat rhetorically “is not a common explanation for both (major and minor fluctuations) more appealing than separate explanations?” (Friedman, 1969, p. 223). These statements are provided as a “substitute” for a proper Deweyian analysis.

Another point, which is also related to an issue I want to take up in the next section, is the importance Friedman attaches to having theoretical arguments to support his findings. He says “however consistent may be the relation between monetary and economic change, and however strong the evidence for the autonomy of monetary changes, we shall not be persuaded, unless we can specify in some detail the mechanism that connects the one with the other” (Friedman, 1969, p. 229). He subsequently makes an attempt to provide such a mechanism. For Friedman it is thus important that the proposed causal relation between changes in the money stock and changes in nominal income is embedded in an existing theoretical structure. This point is, in my view, not stressed enough by Hirsch and de Marchi.

4. Similarities between Friedman and Mill

In the previous section, I have mentioned some un-Popperian elements in Friedman and Schwartz’s method of inquiry in A Monetary History. In this section, I want to go into one of Friedman’s other major contributions to economics, namely his Theory of the Consumption Function (1957) and point at some important Millian elements in this monograph.

Hirsch and de Marchi use Mill’s thinking to show how unorthodox Friedman’s work in economics actually was. As outlined in section 2 above, they are certainly right that there are basic differences between Friedman and Mill, most notably on the role of introspection in economic analysis and on the truth of economic propositions. However, I think they have overlooked some important similarities. The point can be made clear by emphasizing that Mill made a sharp distinction between the truth of economic laws and their applicability. Mill clearly stated that there are economic laws that are true, but for him this did not mean that we can deduce with certainty the actual state of affairs from these basic laws, i.e., he did not use the logical scheme $p \implies q$ if $p$: Economic laws are tendency (or ceteris paribus) laws and they might not directly apply to reality, because “so many influences are operating at once” (p. 14) that it is very likely that we have overlooked some disturbing causes. It is for this reason that Mill didn’t regard “inconsistency evidence as a signal that theory has to be rejected. To Mill, certainly in economics, disconfirmation meant that pertinent disturbing causes had not been taken into account” (p. 141). Moreover, for Mill, it is necessary to know the specifics of a certain situation before we can apply the basic laws of economics. What I hope that the above observations on Mill convey is that when we are concerned with the question how economic theory relates to reality, the truth of the basic economic laws is not really a central issue. Economic laws are true in some abstract world, but not in the real world. For Friedman, economic propositions are hypotheses which also need not be true in the real world. What is of key importance is that for both Friedman and Mill disconfirming evidence simply suggests that something has to be done.4

If we look at The Theory of the Consumption Function it is clear that Friedman starts with an outline of his Permanent Income Hypothesis (PIH). As the hypothesis contains theoretical notions it is not possible to “derive it” from the data (see also section 2 above). Some implications of PIH are spelled out and it is shown that three important global empirical regularities can be explained by PIH (P. 195-203). Most of the rest of the monograph is devoted to showing the consistency of PIH with existing evidence. The way in which this is done is the following. There are many factors that play a role in PIH and when keeping the other factors constant it is possible to deduce the implications of each factor taking on different values. (Thus, the implications hold ceteris paribus.) When observed differences coincide with the differences so deduced, Friedman concludes that the difference in value of this factor causes the observed difference even in cases where the other factors clearly are not constant. As there are many different ceteris paribus implications that can be derived from PIH, it is difficult to see what evidence Friedman would consider as disconfirming the hypothesis. Mill’s tendency laws cannot be disconfirmed by empirical evidence either. From this, I conclude that as long as we stick to a hypothesis, because there is no evidence that we want to consider as disconfirming evidence, it does not seem to be too important whether a hypothesis is considered to be necessarily true or not.
Hirsch and de Marchi try to make a case that Friedman basically starts with observation and reasons "upwards" to arrive at hypotheses and theoretical notions (a bottom-up approach) and that Mill reasons "downwards" from true laws to expected observations (a top-down approach). The contrast between Friedman and Mill is, however, not as sharp as Hirsch and de Marchi suggest. Above, we have seen that Mill does not deduce particular observations from true laws. For him, economics is an "inexact" science (see also Hausman, 1981). On the other hand, Friedman also uses "top-down" arguments as the above remarks on The Theory of the Consumption Function suggest. What Mill and Friedman have in common is the view that there is no deductive link from theory to applied work. Theory yields only some tendency laws and in order to apply them we need knowledge of the specific situation. They also agree that applied work has to be embedded in theory in order to make it into fruitful economics. (Note the observations made in the previous section on the need of a mechanism to support the hypothesized causal relation between money and income). They put different emphasis, however, on the relative importance of theoretical analysis and detailed analysis of the statistics. They also differ on the usefulness of applied work for the development of theoretical insights. However, even this difference should be put to the extreme, because Friedman often takes traditional theory (price theory, utility theory) as beyond any doubt (see also p. 48-9).

5. The Role of Predictions in Friedman's Positive Economics

In the introduction, I have mentioned that Hirsch and de Marchi devote a special chapter to the predictions Friedman actually made in his more popular work. They come up with a small list of predictions (p. 253-4), which includes a phrase as "The most likely pattern for the year [ahead, 1970] is a mild recession". The obvious question is what is the status of statements like this. The answer is far from obvious, however, and in the chapter you can see that Hirsch and de Marchi have struggled with it.

Friedman has stated at several places that "the only relevant test" is whether a theory predicts accurately or not. Above I have argued that Hirsch and de Marchi convincingly show that what Friedman has in mind when using the term "test" should not be interpreted in a falsificationist way. However, if this is the case, the only conclusion one can draw from a disconfirmed prediction is that "some important factor has been left out". Prediction, in this interpretation, does not seem to be too serious a test.

Looking at the prediction quoted above, it is clear that Friedman does not predict any actual state of affairs; he only says that some state of affairs is more likely than another. The reader is reminded of the fact that, according to Mill, we cannot predict the actual occurrence of a certain state, but only a tendency for that state to occur. So, it seems that Friedman and Mill do not disagree fundamentally on the nature of prediction either. They both regard a prediction as a statement about a future state of affairs based on the best available evidence.

The conclusion is legitimate that unlike claims made in his Methodological Essay and unlike his popular writings suggest, prediction does not have a special role in Friedman's positive economics. This conflict is noted by Hirsch and de Marchi who conclude that "it seems that in his popular writings -the ones in which we also encounter predictions- Friedman overstates his claim for the empirical evidence, stretching it to suggest unique causation, which elsewhere he denies is possible." (p. 266).

6. Conclusion

Milton Friedman; Economics in Theory and Practice is a book that is written by two authors who have creatively struggled with their material. Fortunately, they have not tried to polish the loose ends of their analysis away. Different aspects of Friedman's mode of inquiry are brought to the fore so that the reader is presented with a heterogeneous picture. Because of this unorthodox set-up of the book, there are some points which are controversial (as some observations in sections 2-5 suggest). This can also be viewed in a more positive way, however, namely as an adequate representation of the controversial material they have discussed. On the whole, the book stimulates re-thinking the methods Friedman employed in his positive economics and the ones he advocated in his Methodological Essay.

Notes
1. Unless otherwise noted, all page references refer to the book under review.
2. In this review, I will not make a sharp distinction between a logical "implication" and a material "causation".
3. I do not want to suggest that Friedman and Schwartz made a logical mistake. On the contrary, their analysis was not of a logical (read: deductive) nature, but instead of an empirical nature. Also, there might be good (non-standard logical) reasons to make an argument as the one mentioned here (see also Janssen and Tan, 1991), if put in an appropriate manner.
4. The similarities between Friedman and Mill are worked out in more detail in Janssen and Tan (1992).
5. It is important to note here that these implications often contradict each other when the ceteris paribus clause is neglected.
References


Princeton UP.

Princeton UP, Princeton.

*Philosophy of Science* 48, pp. 363-85.
