FRIEDMAN'S PREDICTIVIST INSTRUMENTALISM—A MODIFICATION

Bruce J. Caldwell

In an article titled "A Critique of Friedman's Methodological Instrumentalism" (1980) as well as in my book, *Beyond Positivism* (1982, henceforth *BP*), I argued that Milton Friedman's methodology could be reconstructed as a variant of the philosophical position known as instrumentalism. In this paper, I review the origins of my views in the work of Ernest Nagel. Next, an error in my presentation is corrected and a modification is offered. I argue that Friedman can still be viewed as an instrumentalist, but that his is a predictivist rather than a noncognitivist variant of the doctrine. Where appropriate, I will compare my views with those of the other participants in the symposium.

I. ORIGIN OF MY IDEAS ON INSTRUMENTALISM

My point of departure was "The Cognitive Status of Theories," chapter 6 of Ernest Nagel's magisterial *The Structure of Science* (1961). Nagel argued that

Research in the History of Economic Thought and Methodology
Volume 10, pages 119-128.

ISBN: 0-55938-501-4

both realism and instrumentalism could be presented as reasonable positions, that "opposition between these views is a conflict over preferred modes of speech" (p. 152). This stance appealed to me at the time I was writing, though I suspect that many would not accept it today.

Nagel defines an instrumentalist as one who:

> maintains that theories are primarily logical instruments for organizing our experience and for ordering experimental laws. Although some theories are more effective than others for attaining these ends, theories are not statements, and belong to a different category of linguistic expressions than do statements. For theories function as rules or principles in accordance with which empirical materials are analyzed or inferences drawn, rather than as premises from which factual conclusions are deduced; and they cannot therefore be usefully characterized as either true or false, or even as probably true or probably false. However, those who adopt this position do not always agree in their answers to the question whether physical reality is to be assigned to such theoretical entities as atoms (p. 118).

Nagel contrasts instrumentalism with realism. Realists view theories as literally true or false, even though in practice the best that one may be able to do is to establish them as more or less probable. Realists also believe that the objects ostensibly postulated by theories actually exist.

Nagel also discusses the strengths and limitations of instrumentalism. There are two major strengths. First, instrumentalism accurately describes the way that theories are often actually used by scientists: as instruments for some practical purpose. Second, it avoids numerous difficulties associated with other views. For example, it is not necessary to worry about the literal truth or falsity of theories, which is particularly liberating for theorists. (This point was made by Giedymin, cited in *BP*, p. 52.) Or, as Nagel mentions, theories are often formulated using ideal, limiting concepts (perfect vacuum, perfect competition) which are for the most part not descriptive of anything observable. When viewed as statements, these must be viewed as false. The instrumentalist can easily justify the use of such concepts because they make the theory simpler. "Despite the fact that a theory may employ simplifying concepts, it will in general be preferred to another theory using more 'realistic' notions if the former answers to the purposes of a given inquiry and can be handled more conveniently than the latter" (p. 132).

The limitations are three. First, just because theories are often *used* as instruments does not imply that terms like "true" and "false" cannot be used to characterize them. Next, when theories are used as premises in scientific explanations, they are statements about which it makes sense to ask about truth and falsity. Finally, scientists who claim to be instrumentalists often speak like realists—they talk as if they believe that theoretical entities actually exist, for example.

II. MY INTERPRETATION OF FRIEDMAN AS AN INSTRUMENTALIST

In *BP*, I mentioned the instrumentalist-realist debate concerning whether theoretical terms make real reference, but I did not follow up on it because no economic methodologist had ever discussed the question of reference in that particular context. Though at the time I was writing (the late 1970s) it seemed clear that realism was becoming increasingly important in the philosophy of science, I could not see its relevance for economics. That is probably why I found Nagel's agnostic treatment of the issue so attractive.

Of all the writings on economic methodology reviewed in *BP*, Friedman's work was the most difficult to interpret because of his paucity of references to philosophers. In an article published in 1973, Stanley Wong argued that Friedman could best be characterized as an instrumentalist. After reading Nagel, it seemed to me that this was, indeed, the best way to interpret the Chicago economist's position. This view was further supported by Larry Boland's (1979) addition to the literature. My contributions in *BP* and in my (1980) *Southern Economic Journal* piece were two. First, I labeled Friedman's position "*methodological* instrumentalism," which was meant to emphasize that Friedman was never interested in the *philosophical* questions which surrounded the instrumentalism-realism debate. (Thus, I agree completely with Dan Hammond's point that Friedman's context was very different from those of his later interpreters.) Next, I offered a critique of Friedman's methodological position.

Fairly soon after the publication of my article and book, it was pointed out to me in correspondence from Boland and Dan Hausman that I had made a mistake in characterizing Friedman as an instrumentalist. As was shown above, instrumentalists believe that theories, because they are instruments, *cannot be usefully characterized as true or false.* Friedman's most noteworthy claim is that the realism of a theory's assumptions should not matter in our assessment of the theory's adequacy, that the ability to predict and simplicity are the only appropriate criteria of theory appraisal. In my discussion of Friedman, I equated "realism" with "truth-value." I then argued that Friedman is an instrumentalist. But this is a mistake. If it is correct to equate realism with truth-value, then Friedman is saying that the assumptions of economic theory *can* be characterized as true or false: *namely, they are* **false.** However, their truth or falsity ("realism") *does not matter, because only predictive adequacy matters.* My error was to equate Friedman's claim that *truth and falsity do not matter* with the instrumentalist claim that theories *are not true or false.*

How important was the error to the rest of my argument? In one sense, it was an egregious mistake, since one of the announced goals of my early work was to clarify the confusion I had found in the methodological literature. But the error did very little damage to my own critique of Friedman. A cursory examination of the arguments in *BP* bears this contention out.

My first move was to point out two standard philosophical arguments against instrumentalism. The first is that explanation should share equal billing with prediction as a goal of science. Logical empiricists had asserted the logical symmetry of explanation and prediction. Their opponents in the 1960s argued that often scientists are able to explain phenomena (like evolution, or the characteristics which suicides might share) without being able to predict them. On the basis of such counterexamples, the symmetry thesis was denied and it was claimed that the goals of science should include both explanation and prediction.

This argument, because it insists that science explains as well as predicts, shares common ground with the one presented by Tony Lawson in his symposium contribution. Lawson's argument is more ambitious, however, since it would seem to leave out the possibility of prediction in any but a closed system. Since the systems studied by most scientists (economists included) are open rather than closed, explanation takes precedence over prediction as the goal of science. Lawson completes his argument by offering a specific realist account of what constitutes an adequate scientific explanation.

The second argument is that theories *can* be usefully characterized as true or false. This second argument *does not* work against Friedman, since he admits that the assumptions of economic theories are true or false. (As noted above, he says that they are mostly false, but that *their falsity does not matter.*) With some modification, though, even this second argument can be made to work against Friedman. Most *philosophers* who believe that theories are true or false also believe that the truth or falsity of a theory's assumptions *does matter*, and matters very much. Indeed, this is one reason why philosophers find Friedman's position so bizarre. (Since his position is not viewed as bizarre by many economists, this leads philosophers to draw the obvious inference about economists-in-general, as well.)

Having presented the philosophical case against instrumentalism, I then made the key observation that any adequate critique of Friedman would have to go beyond the philosophical disputes and deal with his position as it relates to the practice of economics.[1] Two further arguments were then presented. The first was a challenge of Friedman's claim that prediction is the *only* goal of *economic* science. The second was a demonstration that Friedman did not adhere *in his own work* to his strictures concerning simplicity. Neither argument concerned the truth value of statements.

III. FRIEDMAN AS A PREDICTIVIST INSTRUMENTALIST

It may still be true that Friedman is best characterized as an instrumentalist. But it is also clear that the standard categories do not fit him very well. Some new categories will be proposed in an attempt to clarify the situation.

Let us begin by defining two variants of instrumentalism. They differ according to their starting points. *Noncognitive instrumentalism* begins from a descriptive statement about the cognitive status of theories (theories are instruments, and therefore cannot be viewed as true or false). *Predictivist instrumentalism* starts out from a normative statement concerning the proper goal of science (the only goal of science is the development of theories which are good instruments for prediction).[2] An instrumentalist could hold either view, or both together. (It will turn out that Friedman can be read as affirming predictivist instrumentalism but denying noncognitive instrumentalism.) Let us examine each of the forms of instrumentalism more closely to see how they fit in economics.

A. Noncognitive Instrumentalism and Economics

The entire issue of whether noncognitive instrumentalism offers a suitable methodology for economics depends on whether one believes that theories are statements (in which case they can be characterized as true or false) or rules of procedure, inference tickets, or instruments (in which case they cannot be characterized as true or false).

Most economists have never explicitly considered this issue. However, economists sometimes say things which might be construed as supportive of noncognitive instrumentalism. Consider the assertion, "My theory is just an instrument for some purpose; I do not think of it as true or false." One who makes such a claim may well be a conscious advocate of noncognitive instrumentalism. But there are other possibilities. For example, an economist might make the statement in justifying his failure to reject the useful but false theories so commonly encountered in the discipline. Alternatively, one might refer to a theory as an instrument in an attempt to be a careful and modest researcher. After all, it takes considerable cheek to assert that one has a true theory. It seems more modest, and perhaps even more scientific, to claim that one's theory is only an instrument.

Both Dan Hausman (1989) and Uskali Mäki (1989) have argued that among economic methodologists, Fritz Machlup comes closest to endorsing this version of instrumentalism.[3] Machlup referred to theories as "inference tickets" and "rules of procedure" (Machlup, 1955, p. 16), terms which a noncognitive instrumentalist certainly would use. He expropriated these ideas from philosophers in an attempt to walk a middle road between the extremes of (what he called) ultraempiricism and a priorism. But this was only one part of his methodological thought. Machlup also opposed the popular positivism of his day, preferring a subjectivist approach that leaned heavily on the work of such writers as Max Weber and Alfred Schütz. This led him to make some recommendations which are difficult to square with noncognitive instrumentalism: for example, that the assumptions of economic theory be

"understandable" (1955, p. 17). Machlup was exposed to a wide variety of methodological views in his days in Vienna. As a result, his own approach was an eclectic and idiosyncratic blend of a number of possibly incompatible positions. As such, the noncognitive instrumentalist model is insufficiently rich to capture the totality of Machlup's methodological views.

B. Friedman's Predictivist Instrumentalism and Economics

Predictivist instrumentalism states that the *only* goal of science is the development of theories which are good instruments for prediction. Given this end, the best attributes a theory can possess are predictive adequacy and simplicity.

In its pure form, this doctrine is agnostic concerning the key issue facing noncognitive instrumentalists, the cognitive status of theories. For the predictivist instrumentalist, theories may be viewed *either* as statements or as instruments. But in *his* version of predictivist instrumentalism, Friedman is *not* agnostic. For Friedman, theories are statements which can be considered true or false. Thus, Friedman affirms predictivist instrumentalism but denies noncognitive instrumentalism.

One final element must be added to get a full statement of Friedman's methodological position. If theories contain statements which can be true or false, should we seek true theories, or false theories, or does it not matter? Friedman's answer is that the "realism of assumptions" (their truth-value) does not matter.

Friedman's predictivist instrumentalism can therefore be stated as follows:

The only goal of science is the development of theories which are good instruments for prediction. Given this end, the best attributes a theory can possess are predictive adequacy and simplicity. The "realism of assumptions" (their truth-value) does not matter. Indeed, many of the "best" theories in economics have assumptions which are false.

Friedman's predictivist instrumentalism is much better than is noncognitive instrumentalism for describing the views of economists. (Of course, this may be due simply to the influence that Friedman's essay has had on the profession.) Predictivist instrumentalism accomodates the fact that economists often view the discovery of predictively adequate theories as their sole goal. Similarly, when economists use assumptions which are unrealistic, they are more likely to say that the use of unrealistic assumptions does not matter than they are to say that the assumptions are neither true nor false.

The claim that prediction is the *only* goal of science is the most controversial part of this position. If accepted, it would make the search for true, explanatory theories at best incidental to the search for the best predictor. Realist philosophers of science would reject Friedman's position on these grounds.

Within economics, I think that fewer economists would so quickly reject this claim, primarily because Friedman's position has entered the rhetoric of the profession. But our *practice* is not consistent with it. Predictive adequacy is valued highly by economists, but it is not the *only* goal of economics. Friedman's predictivist instrumentalism at best describes only a part of what economists do.

It can be finally noted that, *even if* one were to accept the proposition that prediction is the sole goal of science, this does not imply that the truth and falsity of assumptions does not matter, nor that the simpler theory is the better theory.

Dan Hausman (1989) shows that the former claim need not hold. Say a theory which once predicted well suddenly starts predicting poorly. How might we go about trying to figure out what went wrong? Hausman answers that an efficient way is to inquire about the truth or falsity of some of its assumptions. Thus, *for instrumental reasons* (that is, in order to find the best predictor), we may well be interested in the truth and falsity of assumptions.

Though there are problems with defining simplicity, I argued in *BP* that the simpler theory need not be the one that predicts best. The example is the Phillips Curve. The stationary, "simpler" Phillips Curve does less well at predicting the events of the 1970's than does the more "realistic" Friedman-Phelps apparatus. That Friedman developed this apparatus, and even referred to it in his Nobel address as an example of scientific progress in economics, is one of the sweeter ironies of this tale.

IV. SOME CONCLUDING REMARKS

In conclusion, I will briefly mention some points of contact between my framework and the far richer engine of analysis proposed by Mäki. Because of the complexity and detail of Mäki's system, it may not be apparent that the two frameworks are broadly consistent with one another. But they are. An ontological, referential, representational, and veristic nonrealist is the same thing as a noncognitive instrumentalist: both think that their theories do not refer to or represent anything existing in the world. In describing Friedman's position, I argued that he denied noncognitive instrumentalism. This is equivalent to Mäki's claim that Friedman is a semantical realist in all the dimensions mentioned above. I pointed out that for predictivist instrumentalists like Friedman, predictive adequacy and simplicity are important attributes of a theory. This is equivalent to Mäki's claim that Friedman values certain realistic attributes of neoclassical theory ("is capable of predicting well") as well as certain unrealistic attributes ("is simple").

Our accounts differ in two respects. In my presentation, I equate "realism of assumptions" with "truth-value." Mäki presents a much richer and more general interpretation in which "realisticness" *can* refer to truth-value, but also to other characteristics of representations. Second, I argued that false assumptions do not matter to Friedman (what matters is predictive adequacy), whereas Mäki claims that Friedman views false assumptions *as a virtue*.

For a predictivist instrumentalist who also denies noncognitive instrumentalism (like Friedman does), there are actually *three* alternatives. These can be stated in the form of imperatives:

1. Seek true theories which predict well!
2. Seek false theories which predict well!
3. Seek theories which predict well, their truth or falsity does not matter!

Mäki attributes version number two to Friedman, whereas I attribute version number three to him.

Both interpretations find support in Friedman's text. Interestingly, our disagreement may be due to differences in our respective interpretative frameworks. Most realists think that we *should* seek theories which are true, that is, they endorse imperative number one. As a realist, Mäki's interpretative eye was drawn to those passages in Friedman's essay in which the opposite claim is made. Because I focused on Friedman's predictivist instrumentalism, I was drawn to those passages in which the importance of prediction was emphasized. In addition, in *BP* I always tried to present whatever position was under examination *in its best possible light* before submitting it to criticism. This is relevant because I believe version number two to be a straw man. It implies that we should choose a theory we know to be false over one we believe to be true if both predict well. It also implies that we should seek out false theories to substitute for all theories we believe to be true. Version three thus seems to be the more defensible one.

It must be added that *neither* framework attempts to explain *why* Friedman viewed prediction as so important and "unrealistic assumptions" as inconsequential. Dan Hammond suggests an answer to these questions. Friedman is a Marshallian, a practical, applied economist who wants to solve problems in the real world. But he also recognizes that the tools available to him (and for Friedman, the only tools are those of neoclassical theory) have assumptions which are problematical. Friedman's response is to argue that it is not necessary to worry about these problematical, "unrealistic" assumptions. *Were Friedman to stop here,* his position would be indistinguishable from the Walrasians, who also use theories with false assumptions. This is precisely why prediction is so important for Friedman. **Prediction is his link to the real world.** It is what separates his views from those of Walrasians like Lange and Lerner or, for that matter, Arrow and Debreu. Hammond's great contribution is to get us to step back from the philosophical categories to get a handle on the issues which actually motivated Friedman in his work.

ACKNOWLEDGMENT

I would like to thank Warren Samuels and Dan Hausman for their insightful comments on the paper. Remaining errors are my own.

NOTES

1. Given the disagreement that existed in the philosophy of science, it was crucial to go beyond that discipline in developing criticisms of methodological positions in economics. This point was emphasized early on in *BP*:

> A study of economic methodology from a philosophy of science perspective may help one to clarify, unify, categorize, and explicate debates in the former field. But it will not provide ultimate grounds for arbitrating among well-developed and well-argued alternative positions. (p. 3).

The sorts of additional questions which were asked included: Is this position practicable in economics? Does it accurately portray any current practice? Does its proponent's work reflect its usage? Is it internally consistent? What would be the benefits and costs of its adoption?

2. The term "noncognitive instrumentalism" is borrowed from an article by Sidney Morgenbesser (1969), though I use it in a slightly different way. Dan Hausman alerted me to the importance of Morgenbesser's piece. I discovered, after completing the paper, that Alan Coddington had earlier (1979) used the term "predictionism" to refer to what I have labeled Friedman's predictivism. Coddington makes a number of important points, but unfortunately, the article has gone unappreciated.

3. Actually, Hausman argues that while Machlup is the closest thing we have to a noncognitive instrumentalist in economics, he nonetheless badly misapplied the doctrine. Machlup used it to insulate the "theoretical" claims of neoclassical economics from testing. Logical empiricists had used it to argue that it was possible to still accept theories which had theoretical claims which were untestable.

REFERENCES

Boland, Lawrence. 1979. "A Critique of Friedman's Critics." *Journal of Economic Literature* 17:503-522.

Caldwell, Bruce. 1980. "A Critique of Friedman's Methodological Instrumentalism." *Southern Economic Journal* 47:366-374.

________. 1982. *Beyond Positivism.* London: Allen and Unwin.

Coddington, Alan. 1979. "Friedman's Contribution to Methodological Controversy." *British Review of Economic Issues* 2:1-13.

Friedman, Milton. 1953. "The Methodology of Positive Economics." Pp. 3-43 in *Essays in Positive Economics.* Chicago: University of Chicago Press.

Hausman, Daniel. 1989. "Economic Methodology in a Nutshell." *Journal of Economic Perspectives* 3:115-127.

Machlup, Fritz. 1955. "The Problem of Verification in Economics." *Southern Economic Journal* 22:1-21.

Mäki, Uskali. 1988. "How To Combine Rhetoric and Realism in the Methodology of Economics." *Economics and Philosophy* 4:89-109.

________. 1989. "On the Problem of Realism in Economics." *Ricerche Economiche* 43: 176-198.

Morgenbesser, Sidney. 1969. "The Realist-Instrumentalist Controversy." Pp. 200-218 in *Philosophy, Science and Method: Essays in Honor of Ernest Nagel,* edited by S. Morgenbesser, P. Suppes, and M. White. New York: St. Martin's Press.

Nagel, Ernest. 1961. *The Structure of Science.* New York: Harcourt Brace.

Wong, Stanley. 1973. "The F-Twist and the Methodology of Paul Samuelson." *American Economic Review* 62:312-325.