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A Critique of Friedman's Methodological Instrumentalism*

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I. Introduction

Milton Friedman's 1953 classic, "The Methodology of Positive Economics," is probably the best-known piece of methodological writing in the discipline [9]. It is also a marketing masterpiece. Never before has one short article on methodology been able to generate so much controversy. It was reviewed often, usually negatively. Yet ironically, the methodological prescriptions advanced in his essay have been accepted by many working economists. And this has happened without Friedman ever having directly responded to his critics!

The most recent contribution to the secondary literature is Lawrence Boland's piece, "A Critique of Friedman's Critics" [4, 503–22]. Boland argues that Friedman's methodology is best understood as a variant of the philosophical position known as instrumentalism, and that if Friedman is so interpreted, many critiques of his position existent in the economic literature miss their mark. While these points are well-taken, Boland states in his conclusion that "no one has been able to criticize or refute instrumentalism" [4, 521]. Such a statement leaves the reader with the impression that Friedman's position is not only untouched, but perhaps even vindicated. It is the purpose of this paper to challenge such a conclusion by critiquing Friedman's methodological instrumentalism.

In the next section, some brief comments on definitional and interpretive issues are followed by a restatement of Friedman's position. Sections III, IV, and V contain the core of the criticisms against Friedman's methodological instrumentalism, from both philosophical and methodological perspectives. A conclusion emphasizes the importance of this debate for the critical methodological question of how theory choice is effected in economics.

II. The Restatement of Friedman's Position in Philosophical Terms

Some Definitional and Interpretive Issues

To understand what follows, the philosophical positions of instrumentalism and conventionalism must first be defined.

* I would like to thank Professor Lawrence Boland for many helpful comments.

Instrumentalists claim that theories are best viewed as *nothing more* than instruments. So viewed, theories are neither true nor false (instruments are not true or false), but only more or less adequate, given a particular problem. Just as a hammer is an adequate instrument for certain tasks, and not for others, theories are evaluated for their adequacy, which is usually measured by predictive power.

The conventionalist view stresses the organizational function of theories: theory construction is undertaken to organize a complex of facts into a coherent whole. In the words of philosopher Joseph Agassi, theories are "mathematical systems which serve as pigeon-holes within which to store empirical information" [2, 29]. In this view, theories are again neither true or false, but are posited for a time as being true by convention, given consensus within a community of scholars. The primary conventionalist criterion of theory choice is simplicity: the simpler theory organizes the facts better. But only revolutionary conventionalists try to find ever simpler theories; conservative conventionalists attempt to preserve existent theories by building onto them ever more elaborate (critics would label them ad hoc) peripheral systems.[1]

As Boland emphasizes [4, 509–16], Friedman's methodology combines elements of both instrumentalism and conventionalism. However, Friedman's most controversial statements, that the purpose of science is prediction and that the "realism" of assumptions does not matter, are instrumentalist.

Two more observations are necessary before proceeding. First, it is clear that Friedman was not aware that he was advancing an instrumentalist position when he wrote his 1953 article. This poses no serious problems: Friedman, after all, should be the last to object to a reviewer treating him "as if" he were an instrumentalist. More to the point, in private correspondence with Boland, Friedman states that Boland's representation of him as an instrumentalist is "entirely correct."[2]

The second observation concerns Boland's characterization of instrumentalism, which this author feels is incomplete. Boland isolates the strictly methodological implications of the instrumentalist view: that theories and the theoretical terms contained in them are only instruments; that it is therefore meaningless or irrelevant to speak of them as either true or false; and that theories and their constituent parts can only be judged for their adequacy, which is usually measured by how well their implications (predictions) are confirmed by the data. Boland further characterizes instrumentalism as a methodological response to the problem of induction, and contrasts it favorably with inductivism and conventionalism [4, 506–509].

Within the philosophy of science, instrumentalism is much more than a response to the problem of induction. It is, for example, one side in the debate over the ontological status of the entities referred to by theories and theoretical terms. In that debate, instrumentalism is contrasted with realism: realists claim that theories and theoretical terms should make real references, instrumentalists deny it. Where one stands in such a debate determines one's perception of the role, status and function of theories and theoretical terms in science [28, 29–36]. Philosophers Joseph Agassi and Imre Lakatos claim that one's position on such issues even affects the way the history of science is written [2; 17, 91–136].

1. Popper defines and discusses instrumentalism in two works [24, section 12; 25, 97–119, 215–50]; a number of philosophers have addressed conventionalism [2, section 8; 17, 94–96; 24, sections 19, 20, 30, 46]. Latsis offers an interesting interpretation of Friedman as a conventionalist [18]; Stanley Wong, a student of Boland, was the first to label Friedman an instrumentalist [31, 314].

2. I would like to thank Professor Boland for sharing this information with me.

In his attacks against "realistic assumptions" Friedman was not advancing an argument concerning the ontological status of theoretical entities: he was concerned with methodology, not epistemology. For this reason, Friedman should be viewed as a "methodological instrumentalist," to emphasize that though his analysis is consistent with the methodological implications of instrumentalism, he never dealt with the epistemological issues associated with that philosophical position.

The Restatement of Friedman's Position

The salient features of Friedman's methodology may be summarized as follows:

1. The goal of science is to discover hypotheses that predict well. In Friedman's words, "The ultimate goal of a positive science is the development of a 'theory' or 'hypothesis' that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed" [9, 7].

2. Assumptions are not a locus of testing for theories; their "realism" does not matter. If Friedman is an instrumentalist, "realism" refers to truth-value. Realism is then unimportant because theories are not true or false, but only instruments.

Much confusion in the debates on Friedman's position is due to a sloppy usage by all participants of such key concepts as realism, testability, degree of confirmation, and truth. "Realistic" and its opposite are perhaps the most notorious in this group of ill-defined terms; philosopher Ernest Nagel [22, 214–15] scores Friedman for using at least three senses of the word unrealistic in his essay. No work has been done on how the word realistic has been used by economists, and that task will not be attempted here. Realistic is probably best defined as meaning "understandable," "intuitively plausible," or "reasonable" in the commonsense usages of those words. Testability refers to the *capability* of an assumption or hypothesis to be subjected to test; degree of confirmation refers to the extent to which an assumption or hypothesis is confirmed or disconfirmed by testing, hence, it focuses on the *results* of such testing. Finally, we may follow Popper's reading of Tarski in defining truth as "correspondence to facts" relative to a given background knowledge. In this way, essentialism is avoided [25, 97–119, 215–50].

In much of the literature, the concept "realistic" is conflated with the concepts testable, highly confirmed and true.[3] A few examples show that these are not interchangeable terms. If a Roman philosopher postulated a heliocentric theory of the solar system, he would be propounding a theory which was both untestable and unrealistic (given the state of knowledge at the time) yet true (given the present state of knowledge). Conversely, a theory can be realistic, testable, perfectly confirmed, and false: one could theorize that air pressure increases at high altitudes, and test it by throwing a ball in the air, with its dropping back to earth counting as a confirming instance. Descartes' vortex theory of planetary motion, which fit the data and had analogues in the earthly phenomenon of eddies, was testable, somewhat confirmed, more realistic than action at a distance, and false. The point is made: a realistic theory need not be testable, a testable theory need not be confirmed, and even a perfectly

3. In reply to a comment by Robert Piron, Eugene Rotwein states that if assumptions are to have explanatory power, they should be realistic or true [26, 666–68]. Jack Melitz uses the terms realistic, highly confirmed and true interchangeably in his article on Friedman and Machlup [20, 37–39]. D. V. T. Bear and Daniel Orr state that Friedman's position comes down to the proposition, "the truth of the assumptions is largely irrelevant" [3, 188].

confirmed theory need not be true.[4] Confusing these terms makes the evaluation of Friedman's position all the more difficult.

3. Simplicity is a methodological virtue; the most significant theories explain "much by little" [9, 14].

4. In most cases, a number of hypotheses will meet the criterion of predictive adequacy. In such cases, additional criteria of theory choice (which are "to some extent . . . arbitrary") must be invoked to choose among them. These include simplicity and fruitfulness, and to a lesser degree, logical completeness and consistency [9, 9–10].[5]

The first two propositions comprise the core of Friedman's methodological instrumentalism; they will be the focus of the critique which follows.

III. The Philosophical Rejection of Instrumentalism

The Goal of Science

Philosophers of science since the 1940's have been unanimous in their rejection of the notion that the only goal of science is prediction. Even such positivist philosophers as Carl Hempel have claimed that explanation, not prediction, is the goal of science; it was Hempel who with Paul Oppenheim developed the covering law models of scientific explanation [12; 13]. More recent models of the structure and nature of explanation in science admit to even broader definitions of the concept than did the covering law models.[6] Once one takes the position that explanation is the goal of science, the instrumentalist view of theories and theoretical terms is considerably weakened. If science seeks theories that have explanatory as well as predictive powers, then theories that merely predict well may not be satisfactory, and the view that theories are nothing more than instruments for prediction must be rejected.

The Truth-Value of Theories

Even if it is admitted that science seeks theories that explain as well as predict, the instrumentalist has a second line of defense. It is well known that for any set of empirical data, an infinite number of mutually incompatible theories exist that can explain the evidence at hand. Even a moderate skeptic must therefore despair over the possibilities of ever finding the "true" theory. Instrumentalism allows one to circumvent the entire issue by claiming that

4. Jim Murphy provides the air pressure example, and Rowan and Eckberg discuss Descartes's vortex theory [21, 473; 27, 224–34].

5. Friedman, like most writers in methodology, goes beyond predictive adequacy and simplicity in defining criteria of theory-choice; for this reason, categories like instrumentalism and conventionalism must be viewed as extreme versions of how one may view theories and their selection. In a recent paper, Vincent Tarascio and I enumerate a number of empirical and non-empirical criteria of theory-choice [29]. Using that more general schema, one can characterize particular methodological viewpoints as instrumentalist, conventionalist, falsificationist, etc., depending on which criteria of theory-choice are emphasized.

6. The relevant literature on explanation is extensively documented by Caldwell [5; 6]. A point of clarification: Hempel and Oppenheim state that "an explanation is not fully adequate unless its explanans, if taken account of in time, could have served as a basis for predicting the phenomena under question" [13, 138]. This is *not* the same as stating that explanation and prediction are equivalent; rather, it is a *logical* analysis of the structure of an explanation, as is emphasized in Hempel's later paper. That the covering law models cannot be used to justify Friedman's instrumentalism is clear once one notes that one of Hempel and Oppenheim's "conditions of adequacy," which all sound explanations must meet, states that "the sentences constituting the explanans must be true" [13, 138].

theories are only instruments and that as such it is meaningless to speak of them being either true or false; as Boland puts it, instrumentalists "think they have solved the problem of induction by ignoring truth" [4, 509].

But indeed, such an approach does *not* solve the problem. Instrumentalists fail to comprehend that though we may not know whether a theory is true or false, it in fact is true or false. An analogy from probability theory illustrates this point: when an estimate of a probability distribution is made, the estimate may be wrong, but the actual distribution exists if the population is finite. Even if we never know what the actual distribution is, we should still try to make the best estimate using all of the available information. In regards to theories, the philosophical realist (who is, in such discussions, the opposite of the instrumentalist) recognizes at all times that his theory may be wrong, but is still willing to accept that risk and seek for the true theory. The realist will support only those theories he believes may actually be true, and he will posit such theories for a time as being "true by convention." Realism thus contains elements of conventionalism. Instrumentalists refuse to take such a step, and philosopher Imre Lakatos views this as a gross error on their part.

> ... some conventionalists did not have sufficient logical education to realize that some propositions may be true whilst being unproven; and others false whilst having true consequences, and also some which are both false and approximately true. These people opted for 'instrumentalism': they came to regard theories as neither true nor false but merely as 'instruments' for prediction. Conventionalism, as here defined, is a philosophically sound position; instrumentalism is a degenerate version of it, based on a mere philosophical muddle caused by a lack of elementary logical competence [16, 95].

Whether instrumentalism arose due to "a lack of elementary logical competence" on the part of its supporters is doubtless an arguable point; however, most contemporary philosophers of science share with Lakatos misgivings about the adequacy of instrumentalism. Thus, Peter Achinstein states that if we are to consider a set of propositions T a theory, then the person who holds it "does not know that T is true, although he believes that T is true or that it is plausible to think that it is" [1, 122]. Karl Popper also rejects instrumentalism, primarily because it forces scientists to abandon the search for truth. When one seeks for truth, it does not mean that one is searching for the "essential nature" of things: that view Popper labels essentialism, and he rejects it because the scientific enterprise is constantly seeking fuller rather than ultimate explanations. Instrumentalism is untenable because it does not urge scientists to practice a critical methodology; it is satisfied with high correlation and does not push the scientist to seek out fuller explanations [24, 103–14]. Other philosophers who reject instrumentalism include most contemporary positivists, Grover Maxwell, and P. K. Feyerabend; the notable exception is Stephen Toulmin [7; 19; 28, 27–37, 128–35; 30].

Boland's defense of Friedman rests on the claim that his critics did not deal with that economist's instrumentalism. The arguments above indicate that instrumentalism finds few supporters in contemporary philosophy of science. This point is important in its own right, but also is crucial to the evaluation of instrumentalism on purely methodological grounds, as is shown next.

IV. The Methodological Critique of Instrumentalism

Instrumentalism could still be retained if it was shown to be a superior position on strictly methodological grounds. Methodological positions can only be evaluated *given* a certain view of the nature and purpose of science. If the goal of science is to find theories that are predictively adequate, instrumentalism is a viable methodological stance. However, if the goal of science is the discovery of true explanatory theories, instrumentalism fails.

If we assume that contemporary philosophers of science are right, and that their view is the better characterization of the scientific enterprise, the question may be asked: Will instrumentalist methodology aid us in our quest for true, explanatory theories? Clearly not. First, the instrumentalist preoccupation with predictive adequacy forces scientists to prefer statistical correlation over causal explanation if the former provides better predictions.[7] Next, as Bear and Orr show, false antecedents can generate true consequences; if we wish our theories to be true as well as predictively adequate, prediction must be supplemented with other criteria of theory-appraisal [3, 188–91]. Third, as argued by Popper and repeated by Bear and Orr, acceptance of instrumentalism rules out disconfirmation in science: a theory that is neither true or false can be found inadequate, but not disconfirmed [25, 113–14; 3, 189–192]. Finally, the poor predictive record of the economics profession challenges the belief that even discovering theories which are predictively adequate will be a simple task [29].

These arguments are not new; all have been made by critics of Friedman. The point is that they are appropriate criticisms of Friedman only if one asserts that the goal of science is to seek for true, explanatory theories.

Philosopher Jerzy Giedymin has highlighted the methodological advantages of instrumentalism. If that position is interpreted as saying that theories need not be eliminated for their unrealism (or because they do not meet some other criteria excepting predictive adequacy), instrumentalism becomes a liberal methodological position which encourages theoretical pluralism [11, 178–80].

As is emphasized in an early article by Klappholz and Agassi, however, Friedman did not interpret instrumentalism in this way. Instead of arguing for a liberal methodology which encourages pluralism, Friedman claimed that realism was a vice and not a virtue and that theories which exhibited greater realism should not even be considered theoretical advances. By this wholesale elimination of possible competitors to neoclassical theory, "Friedman adopts a position which impedes criticism in general" [14, 66]. Even had Friedman taken a more liberal stance, instrumentalism is sufficient but not necessary for one to support theoretical pluralism: Kuhn, Lakatos and Feyerabend all call for tolerance in the assessment of new theories without invoking instrumentalism. It seems, then, that a strictly methodological defense of instrumentalism in unsuccessful, given a non-instrumentalist view of the nature and goals of science.

V. A Recent Example of Inconsistency in Friedman's Position

Defenders of Friedman might claim that the epistemological and methodological criticisms above do not defeat Friedman's position because that economist's methodology goes beyond simple instrumentalism. Specifically, Friedman adds additional criteria of theory choice (the most important of which is simplicity) to predictive adequacy for the appraisal of theories. Such a defense would claim that Friedman's position must be evaluated as a totality, and not for its purely instrumentalist aspects.

7. Charles Nelson's study shows that statistical data can be used to generate adequate predictions of the behavior of aggregates in the U. S. economy [23].

I have argued with Vincent Tarascio elsewhere that the arbitrary selection of only certain criteria of theory choice to supplement predictive adequacy is suspect, since the selection of such criteria is not always independent of the characteristics of one's favored theory [29, 991–95]. For example, proponents of well-established theories might view logical consistency and theoretical support as important criteria of theory choice, while advocates of newer theories might select realism or fruitfulness as their preferred criteria. But it is sufficient to show that even Friedman himself is not always consistent in his choice of supplemental criteria of theory selection.

In his Nobel lecture, Friedman reviews some of the changes in thinking which have occurred in the profession in the last several decades on the relationship between inflation and unemployment. He views those changes as illustrative of "the classical process for the revision of a scientific hypothesis" since they were occasioned "primarily by the scientific response to experience that contradicted a tentatively accepted hypothesis" [10, 453]. There have been three stages in the profession's analysis of the unemployment-inflation relationship. The first consisted of the simple acceptance of a stable, downward-sloping Phillips Curve; the second involved the introduction of a long run, vertical Phillips Curve (whose position corresponds to the natural rate of unemployment) together with a body of short run downward sloping curves whose levels correspond to different sets of inflationary expectations. In the third stage, which we are now entering, economists concern themselves with explaining the apparent positive relationship between inflation and unemployment. Friedman believes that analytic progress in the second stage leaned heavily on pioneering work done in the areas of expectation formation and information and contract theory; he expects that similar progress in the third stage will rely on the investigations by Arrow, Buchanan, Tullock and others who have applied economic analysis to political behavior [10, 454–60].

It is Friedman's conviction that the changes described above were brought on by the failure of earlier hypotheses to offer predictions which were consistent with the empirical evidence. This is undoubtedly true and indicates the important role of predictive adequacy in this branch of economics; but in not one place does he state that the most significant theories "explain much by little." Instead, he praises the newer theories for their "richness," and for their ability to "rationalize a far broader range of experience" [10, 470]. But doesn't the concept of costly information, the inclusion of stochastically changing inflationary expectations, and the existence of long run implicit or explicit contracts in goods and labor markets, doesn't all of that fly in the face of Friedman's earlier methodological dictums? And what about the holistic attempt (which characterizes the third stage) to include political institutions in an economic analysis; is there any way to defend that addition as simpler, more economical, or less concerned with realism? Clearly, Friedman's invocation of simplicity is much more useful if one is defending the quantity theory of money or the profit maximization assumption than if one is discussing recent developments in Phillips Curve analysis.

VI. Conclusion

The arguments above show that instrumentalism can and has been criticized on both epistemological and methodological grounds; as such, Boland's statement that "no one has been able to criticize or refute instrumentalism" [4, 521] must be rejected. In addition, I argued that Friedman himself has not always followed the dictates of his own professed methodology.

Instrumentalists and their opponents hold starkly contrasting views of the scientific enterprise, of the nature of explanation in science, and of the status and function of scientific theories. Instrumentalism is the more skeptical view: theories are no more than instruments, scientists are urged to be content with correlation, and to abandon any attempts at explanation. Opponents of instrumentalism demand more from their science; they believe that the search for theories which are explanatory, plausible and possibly true is a worthwhile scientific activity. Opponents of instrumentalism are not naively optimistic that such search will always end in success. Indeed, much of contemporary philosophy of science is concerned with whether rational theory choice is even possible in science [8; 15; 16]. But the invocation to try is ever-present.

Choice between these two visions of science is ultimately a personal matter, and may crucially depend upon whether the discipline involved is in a skeptical or optimistic period. For this reason, no ultimate, axiomatic refutation of instrumentalism is possible, or even desirable.[8]

One point, however, is clear. The question of how scientists choose among competing theories has been much debated recently in the philosophy of science, and such discussions are now having an impact on economic methodology [18; 29]. The selection of predictive adequacy, supplemented by a few other arbitrarily chosen criteria, as the penultimate and invariant guarantors of rational theory choice does little to advance or illuminate these discussions. Perhaps the most damaging claim against Friedman's own particular brand of instrumentalism is not that it is incorrect, or even implausible, but that it must be viewed, in the light of more recent work, as anachronistic.

8. Boland seems to think that opponents of instrumentalism have failed because they have not delivered an axiomatic refutation of instrumentalism [4, 521–22]. But waiting for such an axiomatic refutation is like waiting for Godot! Consensus among philosophers does not *refute* instrumentalism, it only raises questions about the legitimacy and adequacy of that position and the instrumentalist must respond to these charges. My invocation of *consensus* among philosophers of science (that instrumentalism is an inadequate position) caused Professor Boland to quip that, just as his paper involves an instrumentalist defense of instrumentalism, mine is essentially "a conventionalist defense of conventionalism."

On a separate but related point, it should be stressed that my attack on instrumentalism does not deny its value in situations when economists are only interested in generating predictions. Because economists, like other scientists, are ultimately concerned with *explaining* economic phenomena, however, Friedman's instrumentalism must be regarded as a "special case" rather than as *the* methodology of positive economic science, as he claims.

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