Methodology: A Comment on Frazer and Boland, I

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Milton Friedman has been taken by some to be a follower of the logical positivists and by others a disciple of the philosopher Karl Popper, but until recently not enough has been said by way of support for such claims to make it possible to evaluate their substance. Recently, however, Laurence Boland (1979) has suggested that Friedman should rather be considered an “instrumentalist,” meaning by this that for Friedman “theories are convenient and useful ways of (logically) generating what have turned out to be true (or successful) predictions or conclusions” (pp. 508–09) and Boland has given us a substantive analysis of his position. More recently Boland has combined forces with William Frazer (1983) to try to demonstrate that while Friedman is an instrumental thinker, and instrumentalism is a point of view against which Popper has argued vigorously, still Friedman’s instrumentalism is in some sense compatible with Popper’s falsificationist doctrine. These efforts are certainly in the right direction. A major reason there has been so little return on such a vast amount of discussion about Friedman’s methodology is that the many participants in the debate have focussed on one or another aspect of Friedman’s formulations without trying to determine what general point of view underlay his approach. Boland has dug deeper, and with Frazer has set out to show one of the fruits of this approach. The effort is therefore to be applauded for what it tries to do. But for reasons that will be given in this comment, Frazer and Boland wind up with a quite distorted picture.

First, a word about the genesis of the term “instrumentalism” as used by Frazer and Boland. They rely on Popper (1965, ch. 6) who has traced this way of thinking to the English philosopher George Berkeley. Popper includes within the term such philosophers of science as Ernst Mach, Heinrich Hertz, Richard von Mises, Moritz Schlick, and others. Their instrumentalism, in Popper’s view, grew out of the problem of induction in the philosophy of science and was a way of rescuing the integrity of science while bypassing a troublesome problem, namely, that no one had found a way to prove deductively the truth of theories in the empirical sciences. The price paid for the rescue, as Popper sees it, is that empirical science was given a lower status than other realms of knowing (by making the sciences mere technologies), and this seems to be at least one of the reasons why Popper has rejected it. But Popper himself has not solved the induction problem so that he, in turn, pays a price for the rejection of instrumentalism, the price being the elimination of a substantial part of the empirical or observational element from science.1 As he puts it, the alternative to induction is conjectural knowledge, whose objectivity lies in its being formulated so as to be open to rational criticism.2

There is, however, another type of “instrumentalism” which was developed by the pragmatic American philosopher, John Dewey (1938), who strongly influenced one of Friedman’s mentors, Wesley Mitchell.3 As

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1So far as Popper is concerned, those who do the major work in science could be deaf, dumb and blind, so long as they had a few assistants with unimpaired senses to try to falsify the theories that scientists come up with. Facts are said to be the ultimate arbiter of the fate of a theory (Paul Schilpp, 1974, p. 971), but progress in science rests on the stage reached in critical discussion. Note that Popper (1972) claims he has resolved the induction problem, but Boland (1982) argues convincingly that he is mistaken in this.

2Popper (1972, pp. 9, 16, and passim).

3There are important instrumental components in both Mitchell’s and Friedman’s approach to economics though both deviate from instrumentalism in very different ways. These differences are considered in our (1983) paper.
has often been observed, Dewey’s philosophy very well reflects the practical American environment, and having this orientation, it is not much concerned with the “rationalistic,” nor with deductive proof. As a consequence, the very attempt to find a deductive proof of induction Dewey considered a will-o’-the-wisp arising out of a “quest for certainty” which is unattainable in this world (see Joseph Ratner, 1939, pp. 275–91). To Dewey the problem of induction is a pseudo-problem dreamed up by philosophers and best left alone. What is needed, he believed, is a theory of inquiry, based upon detailed and careful observation in the first instance, which shows how logical forms are generated in the process of inquiry, everyday as well as scientific, and how the scientist, as well as the layman, judges whether progress has been made when inquiry is undertaken.

Having approached science in this way, Dewey came up with notions which differ in some important respects from both Popper’s and those of the instrumentalists whom Popper criticizes, as well as having features in common with both. Like Popper and those whom he calls instrumentalists, Dewey puts the test of implications derived from theory at the very center of “science.”

But unlike Popper and those whom he calls instrumentalists, Dewey emphasized the observational basis of science (pp. 108–10); he did not recognize a “higher” sphere than the practical one of trying to achieve the ends of social existence (Ratner, pp. 945–49); and he rejected logical consistency as the one true God of the intellectual realm. Against this background some of the limitations of Boland’s analysis and of the Frazer-Boland analysis become apparent.

Looking at Boland’s original paper (1979) we note, first, that he deals with just one of Friedman’s essays (1953, first essay) and only one aspect of the thesis there developed. He does not try to explain, for example, why Friedman has involved himself with very extensive statistical investigations (Friedman, 1957; Friedman and Simon Kuznets, 1945; Friedman and Anna Schwartz, 1963; 1982) whereas most others have not; why Friedman is less concerned with an overarching logical consistency (one general explanation) (Friedman, 1974); why Friedman is less enthusiastic about the simultaneous-equations (structural) approach than many other economists (Friedman 1951); why Friedman is unconcerned not to have come up with an overall testable (or falsifiable) macro theory as other macroeconomists have tried to do (see John Wood, 1981); why Friedman’s approach to policy differs from that of others. As we show elsewhere (1983), all of these diverse strands can be accounted for as part of a general rationale once the nature of the basic point of view is properly identified. Second, Boland sees Friedman’s as a “practical” approach. But does practicalism necessarily go with instrumentalism? Surely not the instrumentalism of Berkeley, Mach, and the rest whom Popper points to as instrumentalists. Thus, while Boland in this instance has certainly put his finger on an important aspect of Friedman’s thinking, he has failed to connect it to his instrumentalism thesis. We need the Dewey-type of (pragmatic) instrumentalism to enable us to make the connection.

Third, to Boland, Friedman’s instrumentalism is built on a disjunctive type of argument (1979, p. 506), an approach, as he notes, which is more ap-

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4Dewey was even critical of economists for resting the validity of their theories on assumptions (Dewey used the term “premises”) and argued that they should instead attempt to test the implications (Dewey’s term was “consequences”) of the theory. See Dewey, pp. 504–06.

5In a letter (see Arthur Burns, 1952, p. 96), Mitchell noted that Dewey opened his eyes to the fact that systems of thought, from his grand-aunt’s theology to Marshall, might be tolerably self-consistent but that this was not an adequate test of “truth.”

6Though he certainly carries it too far. He—alone and in combination with Frazer—makes it appear as if science is no concern whatsoever to Friedman. This is not true. It seems rather that Friedman passionately wanted to contribute to economic science, because he felt, as Dewey did, that to be useful, economics had to be scientific.

7Pragmatism and instrumentalism do not necessarily go together. Thus, Frank Knight gave assent to the pragmatic view of William James (1935, p. 97) but was scathingly critical of Dewey’s instrumentalism (1947, essay three).
appropriate for politicians trying to win elections (p. 506, fn. 8) than scientists searching for truth.8 This is inconsistent with the implication in Frazer-Boland to the effect that Friedman was influenced by Popper, for it seems to us that had Friedman been influenced even in a general way by Popper, he would have amended his practice in order to try to become a “true” (Popperian) scientist. Of course, this would not necessarily have occurred were Friedman’s major concern technology rather than science, but all of the evidence points in the other direction. Not only does Friedman again and again talk about “economic science,”9 but in the very article with which Boland is concerned he uses Galileo as a model, not a well-known engineer.

It is worth pausing at this point to remind ourselves of what it is that Friedman and Popper are trying to do, each in his own work. Friedman wants usable predictions of the consequences of changes in the economic environment: specific answers to concrete problems, such as the effect of a tax on the demand for some commodity; the consequences for prices of a sudden jump in the rate of growth of the money stock. Facts—generalizations of related facts—and measurement are important here; but arguments based upon plausible generalizations only hinder the process of clarifying what policymakers have to argue and decide about. The ends of positive economic science are achieved when one of many possible explanations of events can be shown to hold up better under a wide variety of circumstances. One may then have some confidence in it as a basis for positive prescription (see Friedman and Schwartz, 1963, p. 676). Theories give more or less precise answers to the problem at hand; can explain more or fewer events; may have been checked over a more or less extensive base of cases; and, accordingly, are more or less useful.

By contrast, for Popper, “Theories are true or false and not merely [useful] instruments” (1972, p. 80). Deep down he is concerned with a striving after true knowledge (Schilpp, p. 1022; Popper, 1972, p. 697). Truth here has to do with propositions which have so far resisted refutation by facts and which thus give us some tentative ground for thinking we have moved in the direction of the target class—admittedly ideal and unattainable—of all true statements (Popper, 1972, pp. 57 ff.). Popper’s work has been an extended attempt to formulate a logic for science as the pursuit of truth which avoids the pitfalls of Inductivism and Conventionalism (terms well explained by Boland, 1979). The process is one of identifying all the consequences of a conjecture, both those which can be tentatively accepted as true and those which could be false. By severe testing of its vulnerable parts—testing those strong claims for which matters could most easily be otherwise (Schilpp, p. 978)—and failing to reject them we pare down the falsity content and add to the tentative truth content. Thus truthlikeness is “a purely logical affair” and “we test for truth, by eliminating falsehood” (Popper, 1972, pp. 30, 81).

Friedman, too, rejects Inductivism and Conventionalism, as Boland (1979) makes clear.10 Not surprisingly, Popper’s theory of method issues in a methodological rule that is virtually indistinguishable from Friedman’s. As if echoing Popper’s point that science can only be “a competitive comparison of falsity contents” (1972, p. 81), Friedman, too, writes: “The greater the range of evidence that, if observed, would contradict a theory, the more precise are its predictions and the better a theory it is provided it is not, in fact, contradicted” (1974, p. 134). This agreement, however, is quite superficial. For Popper a rule for judging good scientific practice is a by-product of his philosophical

8In fact, as we read Boland, the politician is in a higher realm than Friedman because, according to Boland, the politician uses disjunctive reasoning where not all, but at least one, premise or assumption must be true for the argument to stand up logically; in Friedman’s instrumental approach, however, according to Boland, none of the assumptions need be true.

9As Mitchell did—see, for example, Friedman (1953, pp. 282–83.)

10In an earlier article (1970), Boland argued that Friedman was a conventionalist but he has apparently changed his mind.
concern, namely, what would a theory of the growth of true knowledge have to look like to avoid Inductivism and Conventionalism. Friedman for his part is mainly a consumer of methodological insights. A good theory, for him, is first and last a *useable* theory, if it happens also to conform to certain Popperian criteria of how good science is done that is an incidental bonus picked up along the way. One would be hard put to argue that Friedman’s economic practice and its results have been driven by anything remotely resembling Popper’s concern with a logical analysis of the growth of true knowledge. It is precisely by ignoring the different nature of the concerns of Popper the philosopher and Friedman the positive economic scientist that we end up with distortions such as those we alluded to earlier in Frazer-Boland. We deal here with five of these.

Notice, first, that Friedman has not only argued that “The theorist *starts with* some set of observed and related facts, as full and comprehensive as possible” (1953, p. 282, italics added), but in his labors for the National Bureau he has very conscientiously put this tenet to work. This, however, is the very antithesis of Popper’s notion of how knowledge is acquired. Popper starts with hypotheses. Friedman’s way could be called positivistic, since it has its roots in the idea that knowledge is acquired through the senses, but it is also an important element in Dewey’s instrumental philosophy. Friedman, of course, was not ready merely to observe; he insists that observation be done in such a way that it has theoretical relevance. But he has tried to steer a middle course between empiricism and rationalism, just as Dewey did. Popper, however, on his own admission is not very far from the rationalists. He even calls himself a critical rationalist.

Second, as Frazer-Boland observe, and as Boland elsewhere argues at length (1979, Sec. 3), Friedman is not engaged in testing to falsify; he is busy trying to find explanations of successful outcomes (predictions that work; empirical regularities that he knows to hold). At the level of his basic concerns, in other words, he is busy with confirming success, not with refutation. This has to do with his starting with observation and with his interest in useable predictions. It is precisely what we would expect a Deweyan to be busy with. But it is not, as Frazer-Boland admit, the Popperian way. Popper enters only in so far as Friedman’s criteria for choosing among possible explanations of success are, as we have seen, in agreement with his. The difference between testing for truth and comparing theories as instruments of prediction stands.

Third, to bring Friedman closer to Popper, Frazer-Boland argue that Popper’s way of thinking, like Friedman’s, is oriented around problems. But there is a difference in the way the term “problem” is used in the two cases. The kind of problems with which Friedman is concerned, as Frazer-Boland show, are practical social problems. This is Dewey’s way. The major problems Popper is concerned with are cosmological, as shown, for example, in his views about the origins of modern science (1965, ch. 5). Starting problems for Popper may be practical problems (what can be done…?); but unless these lead to theoretical problems (*why* are…?) they are not “problems of explanation,” which are the problems that really interest him (1972, p. 263). It seems to us, therefore, that there is also a very large difference here.

Fourth, Frazer-Boland present Friedman as a “short run” instrumentalist. What they seem to mean by this is that Friedman is not concerned at all with economic science, but rather with economic technology (i.e., de-

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11 This is not to say that any sort of theory will do for Friedman. He has a strong preference for theory that fits with the unifying themes of conventional price theory.

12 Note, for example, Friedman’s observation that rationalism on the one side and empiricism on the other have produced real and valuable improvements in the formal language available for describing economic interrelationships and in our detailed knowledge of the phenomenon to be explained by economic theory. But they have left something of a vacuum in the equally vital intermediate area of theories or hypotheses that have implications about important phenomena susceptible of contradiction through observation. [1952, p. 457]

13 This quest is at the very heart of Dewey’s philosophy, particularly as it relates to social inquiry (see, for example, Dewey, ch. 24, particularly pp. 503–12).
VICES that work in the short run) (see Boland, 1982, p. 152). This points back to our third reservation about Boland’s 1979 paper, where we observed that Friedman has insisted that he is concerned with economic science, a claim which, from his Deweyan perspective, is true. Only it happens to be something quite distinct from Popper’s science as the growth of true knowledge.

Finally, to make Friedman a Popperian it is not enough to force Friedman over to Popper; Popper also must be moved over to Friedman. To accomplish this task, Frazer-Boland give us the following quotation from Popper:

...For instrumental purposes of practical application of theory may continue to be used even after its refutation, within the limits of its applicability: an astronomer who believes that Newton’s theory has turned out to be false will not hesitate to apply its formalism within the limits of its applicability.  [1965, p. 113]

Using these notions for the purposes at hand raises a whole host of questions. For one thing, while technological methods involving falsified theory may work, scientific theory is more reliable. Even Friedman wants explanatory rationales for what works. Technological devices have use only because they save thought or computation. It surely cannot be said that Friedman prefers instruments, if he does so at all, because they save on computation. Nor does a study of his counterfactual analysis with Schwartz in the Monetary History, for example, suggest a lazy mind.

There are more subtle difficulties, too, in applying to Friedman the argument suggested by the above quotation from Popper. We say this because the way in which Frazer-Boland use the quotation makes it appear that they are implicitly postulating a tradeoff between immediate practical usefulness and long-run advance in economic science, and that they see Friedman choosing the former while most other economists opt for the latter. Our problem with this postulated tradeoff is, first, that a comparison of Friedman’s work in positive economics with that of others does not bear it out. Second, there is little evidence that Friedman actually recommends such a choice. It seems to us rather that Friedman, like Mitchell and Dewey, opts for the practical—that is, predictive power—partly because he feels that the order of scientific advance is the identification of empirical regularities first, with explanations following. Friedman, as we have noted, recommends that the theorist start “with some set of observed and related facts, as full and comprehensive as possible” (1953, p. 282), and in most of his own work in positive economics he actually follows this order. Furthermore, Friedman seems to hold the implicit belief that philosophers’ ideals of science might never be attained in economics and we may therefore have to settle for something else. He tells us, for example (1953, p. 38), that it would be desirable to have a more general theory of monopoly and competition than Marshall’s which would do for economics what the theory of friction has done for physics; but so long as we do not have one we had better make the most of what we have. This “second best” choice, however, is not one in favor of short-run practical gains over long-run scientific progress. On the contrary, it amounts to an acceptance of the realities for what they are, a major tenet of a Deweyan way of thinking. Considering how often methodologists have chided economists for not living up to some ideal such as Popper’s,14 and how little effect this has had, the Deweyan position on this score, which Friedman seems implicitly to hold, can be said to have a good deal to recommend it. The gist of all this is that Frazer and Boland’s attempt to make a short-run Popperian out of Friedman misses the mark by a wide margin.15

14 Some methodologists (for example, Blaug and Hutchison in Spiro Latsis, 1976) keep scolding economists for not living up to Popper’s ideal. (Blaug refers to Lakatos’ ideal which in this respect is related to Popper’s.) Is not the major reason for this failing that Popper’s ideal is utopian?

15 Friedman is reported by Frazer-Boland to have said that his methodological position is consistent with Popper’s. We have suggested the limited, almost incidental sense in which this is true. We do not see how Friedman could subscribe to Frazer-Boland’s analysis of the connection.
With one observation of Frazer-Boland we do agree. They tell us that “Friedman’s early orientation in methods had no immediate background in established economics” (p. 129). It follows that the orientation must have come for the most part from outside established economics. We have tried to show that it could not have come from Popper, nor even those whom Popper calls instrumentalists.16 We believe that it came from the way of thinking that prevails in the American environment and that is so well reflected in Dewey’s pragmatic instrumentalism.17 In this comment, we have had room enough only to suggest why we have doubts about Frazer and Boland’s interpretation. In a more extended study we undertake the constructive task of trying to show how an understanding of some Deweyan aspects of Friedman’s way of thinking helps shed light on his approach to the methodology of positive economics.

16Frazer-Boland say that Friedman was inspired by Popper (p. 135) and they have a list of distinctive traits which they consider characteristic of Friedman’s approach (pp. 135–39). But they fail to make any connection between these traits and either Popper’s way of thinking or that of the Popperian type of instrumentalists.

17There was also, of course, a Chicagoan input, which lies outside our concerns here. Surprisingly, however, aspects of a Deweyan type of instrumentalism were not uncommon even in the Chicago economics department when Friedman studied there. For some references, see our earlier paper, fn. 5.

REFERENCES


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