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Methodology: A Comment on Frazer and Boland, II

By KEVIN D. HOOVER*

In a recent issue of this *Review* (1983), William Frazer and Lawrence Boland present Milton Friedman's methodology as instrumentalism. My purpose is not to question Frazer and Boland's interpretation of Friedman; rather it is to question their accompanying assertion that instrumentalism is a sound methodology for short-run, practical policy purposes.

Boland has set out his views on instrumentalism in greater detail elsewhere (1979; 1980; 1981; 1982, ch. 9). Since Frazer and Boland's "Essay" draws heavily on the earlier works, I will refer to them as well in order to fill in the implicit details of the argument. The larger purpose of the "Essay" is to examine the relationship between the methodologies of Friedman and of Karl Popper. Similarly, the larger purpose of Boland's earlier article (1979) is to defend Friedman's views from what Boland regards as misguided criticism and to provide a properly drawn "fundamental critique" of Friedman's instrumentalism. While it is important to recognize Frazer and Boland's larger purposes, I am not directly concerned with them, but rather with their analysis of instrumentalism itself. Most of what they say about instrumentalism refers to Friedman. I must assume, however, that what they say about Friedman *as an instrumentalist* applies to their understanding of instrumentalism generally.

According to Frazer and Boland, Friedman's essay (1953) calls "attention to the great relevance of positive economics (empirical study) for normative economics ('what ought to be')..." They continue, "...the question was which policy should be selected. Such question of how we get from where we are to the policy goal was thus seen as an

empirical question, as one of selecting the most useful theory among available competitors" (p. 130). The promise of instrumentalism to Frazer and Boland is that it provides an effective method for answering this question. It does so by dissolving or ignoring the problem of induction and is as a method "...free from logical errors" (p. 131). Boland puts the case even more strongly: "Friedman's methodological position is both logically sound and based on a coherent philosophy of science—Instrumentalism" (1979, p. 503). (Compare Boland 1979, p. 521; 1980, p. 1557; 1982, p. 151.)

Boland qualifies his support of instrumentalism in an important respect: "Instrumentalism is always limited to short-run practical problems. If one is looking for a more universal understanding of the workings of the economy—that is, a true theory of economics—then instrumentalism will never do, since it ignores the truth of theories" (1982, pp. 151–52). (Compare 1979, p. 521; Frazer and Boland, pp. 131, 141–42.) My contention is that, even for short-run, practical policy problems, instrumentalism is not a sound, effective methodology.

I. The Logic of Instrumentalism

The problem of induction is how can we validly infer a general proposition from any finite number of particular instances of it. There is no widely accepted solution to it.

Boland (1979, 1982) sets forth his position with the help of a classical distinction between two sorts of inference: *modus ponens* (A implies B ; A is true; therefore, B is true) and *modus tollens* (A implies B ; B is false; therefore, A is false). Each has an associated fallacy: *affirming the consequent* (B is true; therefore, A is true); and *denying the antecedent* (A is false; therefore, B is false). *Modus ponens* argues from *sufficiency* (A is a sufficient condition for B); *modus tollens* from *necessity* (B is a necessary condition

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for *A*). For *modus ponens* to be useful we must have criteria for the truth or acceptability of *A*; for *modus tollens* for the falsity or unacceptability of *B*. If *A* is a theory which includes some general statements, we would require induction to establish its truth; if *B* is a set of particular conclusions, Boland seems to think determining its truth to be less problematic.

In Boland's view, the instrumentalist proceeds in three steps (1979, pp. 509–12; 1982, pp. 143–48). First, because the instrumentalist believes that there is no inductive logic, he never uses *modus ponens*. Second, he reasons by *modus tollens* to rule out any theories whose consequences are contradicted by any particular facts accepted as true. Having narrowed the universe of theories down to those whose known consequences are true, he has no further use of *modus tollens*—it is merely a prefilter. Finally, he uses conventionalist criteria (for Friedman simplicity and fruitfulness) to select a theory to use for a particular occasion. This theory is free from logical error. It is *alogical*; the first two steps ensure that logic is barely relevant to it. We do not believe that we can establish true antecedents; therefore, we cannot use *modus ponens*. We restrict our attention to consequences known already to be true; therefore, we have no use for *modus tollens*.

But if we consider only consequences already known to be true, what do theories *do*? Boland's answer is that for the instrumentalist they are convenient generators of those consequences. They use "something like *modus ponens*" to do their work (Boland 1979, p. 511). Presumably, it is not *modus ponens* only because the instrumentalist considers the truth of the antecedents to be irrelevant (Boland 1979, p. 509; 1982, p. 142). It cannot be emphasized strongly enough that Frazer and Boland's claim to logicity for instrumentalism is successful *only if* we restrict our attention to already successful predictions—to consequences known to be true. In this case, however, theories as generators are not needed—at best, they are a convenient shorthand, neat summaries of particular facts already known.

II. The Irrelevance of Instrumentalism

So far Frazer and Boland's exposition cannot be faulted. But a crucial flaw enters with the claim that instrumentalism is a sound or useful methodology, in the sense that a practical economist would be warranted in adopting it for short-run policy purposes. This claim rests on an equivocation about predictions. Boland writes: "For Friedman, an instrumentalist, hypotheses are chosen because they *are successful* in yielding true predictions" (1979, p. 511, emphasis added). The present tense in this quotation masks the fact that the instrumentalist can choose a hypothesis only because it *has been successful*, while the whole interest in predictions arises from the hope that they *will be successful*. This ambiguity has important consequences for Boland's argument. He writes:

So long as a theory does its intended job, there is no apparent need to argue in its favor.... For some policy-oriented economists, the intended job is the generation of true or successful predictions. In this case a theory's predictive success is always a sufficient argument in its favor.

[1979, p. 508; 1982, p. 144]

The last sentence may appear to commit the fallacy of affirming the consequent. This is not so, however, if predictive success refers to *past* predictions already known to be successful. In that case, it merely states that the theory has passed the *modus tollens* prefiltering process (step two).

Unfortunately a (perhaps infinite) number of theories will pass the prefiltering process. As policy-oriented economists we want to predict what *will* turn out to be true. We want not past but future success at prediction. For this we must push beyond the class of consequences already known to be true.

Boland recognizes that past success provides no guarantee of future success (1979, p. 513). Frazer and Boland nevertheless believe instrumentalism is sound for short-run, practical policy problems. And their view of the

scope of the shorter run is generous:

...[P]hilosophically, Friedman's views on methodology are instrumentalist in the shorter run where policymakers reside (including in the philosophical context less than a quarter in duration, cycles, trends, and long swings). The span of time is in contradistinction to an infinitely distant future where some ultimately true theory may reside, as found in the imagination of some philosophers. [1983, p. 141]

Normative ends, policy choices, in such a short run are still about the future. Predictions about the consequences of policies cannot belong to the class of the already known to be successful. The only way a theory can be *logically* helpful in generating predictions in this as yet unexamined class of consequences is if it is supposed to be true. And this leads us once again right up to the problem of induction: what grounds are there for supposing a theory to be true?

III. Some Replies Anticipated

Frazer and Boland might argue that instrumentalism has no need for induction; as Boland puts it, "...instrumentalism is its own defense and its *only* defense" (1979, p. 522). The instrumentalist makes no assumption about the truth of his theory and, hence, has no need for justification. He is therefore free, as a practical matter, to use his theory until it fails him and will no longer pass the prefilter. But in this case, anyone is free to use almost any arbitrary theory. Even past predictive failure is no hindrance, so long as the theory asserts that the future is different from the past (see Nelson Goodman, 1965).

Even as a short-run, practical matter, we seek advice about the unknown future and, therefore, need to distinguish one generator of predictions from another. When theories advise incompatible actions, we must choose between them. For example, if one theory said hold *M1* growth to zero in order to reduce inflation and the other said let it grow at 20 percent to reach the same goal, we could not do both.

Frazer and Boland (pp. 130, 142) recognize that such practical choices have to be made. They argue, however, that instrumentalism is particularly at home with such problems. But how to discriminate between theories? Boland answers: "From the stand point of instrumentalism, the only prescription is to choose the theory which is most useful" (1982, p. 148). Unfortunately for the instrumentalist, when we restrict ourselves to conclusions already known to be true, no theory is especially useful; and when we seek advice about the future, a *true* theory is ideally suited to give useful advice.

Against this Boland claims that a theory does not have to be true to be successful (1980, p. 1556; 1982, p. 151). It is enough that the observed consequences be *as if* the theory were true (Boland 1979, p. 513). But this is not enough. Useful predictions are generated by (something like) *modus ponens*. *Modus ponens* in the class of consequences not already known to be true requires not only that the antecedents be supposed to be true, but that they be in fact true. When one has success in prediction, it is *accident* not *logic* which leads to the success, unless the antecedents are either true or related to the truth in such a way that their falsehood is irrelevant (for example, as an approximation).

The inevitable counterexamples are, first, Newton's theory (Frazer and Boland, p. 142)—it is known to be false, but is still practically useful—and, second, the television repairman who believes that faulty transistors are caused by the death of little green men but has still managed to fix the set (Boland 1980, p. 1556; 1982, p. 145). Such examples do not begin to meet the objections.

Newton's theory is easily understood as an approximation. By approximation I mean that all the falsifying evidence falls outside a well-defined set of boundaries or levels of accuracy (for example, Newton's theory has only been falsified at velocities approaching the speed of light).¹

¹Boland's (1981) remarks on Herbert Simon's appeal to approximation do not apply here. Simon argues that

The green man theory might be isomorphic to a true theory—names do not matter. Even if it is not, the example has no force, since Boland considers only the case in which the television set in fact works after the repairman has looked at it, rather than the case in which we must choose who shall repair it. If the set works after it has been repaired, we have no need for any inference or method. We only need inference when the set is yet to be fixed, and here instrumentalism is no help at all.

Most logicians distinguish logical validity (a formal property of an argument) from soundness (a substantive property). An argument with false antecedents may be valid without being sound.² My position is that in the only cases in which instrumentalist arguments are sound, they are also jejune. The only sense in which restricting our attention to short-run, practical problems might make instrumentalism more logically warranted is if “practical problems” is construed to mean “past problems.” For really practical problems this is useless.

Finally, a disclaimer. To point out that the problem of induction cannot be dismissed

when assumptions are approximately correct, conclusions will be approximately correct. I define a theory to be an approximation when its limits of accuracy are either specified within the theory or known because it is “observationally nested” within a better theory (see W. H. Newton-Smith, 1981).

²The argument, All scientists are wise; Economists are scientists; therefore, All economists are wise, is logically valid. If the premises were true, the conclusion would follow. But it is unsound.

even for short-run, practical policy problems is not to offer a solution to it. I have not advocated conventionalism or any other methodology. I have merely pointed out that instrumentalism, while logically flawless in dealing with any problem to which we already have the answer, is not a logically sound or efficacious methodology for any problem that matters.

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