

Revised final draft

## **Boland on Friedman's Methodology: A Summation**

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In this short paper I wish to discuss the methodology of criticizing Friedman's methodology. Twenty years ago, most methodologists felt that Friedman's methodology was dead and buried after Samuelson presented his now famous "F-twist" joke. Methodologists tend to talk only to other methodologists (partly because no one else thinks they have anything to say). For methodologists, the issue of Friedman's methodology was closed. Unfortunately for them, there is a very large proportion of the economics profession which still thinks Friedman's methodology is alive and well. If Friedman's methodology were dead, it would not be so widely practiced. When I wrote my 1979 paper on Friedman's methodology essay, I wrote it with these facts in mind rather than with the self-serving myopia of many methodologists.

Methodologists must face the facts and explain the continued widespread acceptance of Friedman's methodology. My 1979 explanation proceeded from the presumption that the acceptance of Friedman's methodology is so widespread that by now it would be unlikely for anyone to find that this methodology is internally inconsistent. What was more likely was that some of the existing critiques of Friedman's essay were flawed. But most important, I think anything worth talking and writing so much about is worth criticizing well.

Many writers have responded to my 1979 paper. With few exceptions, the purposes of these responses have not been clear. Obviously, most of these writers have very strong objections to Friedman's economics and thus feel duty-bound to attack Friedman at every occasion. People have been attacking Friedman and his methodology for over thirty years and, as I continue to claim, nothing much has been accomplished. In the 1979 paper my intention was to deliver the message that if one wishes to criticize Friedman's methodology, one is more likely to succeed when the criticism is based on one's clear-headed understanding of the aim and purpose of Friedman's essay than on one's ideological biases.

The recent attacks on my 1979 paper give one the impression that the newest critics are more interested in attacking the messenger than coming to grips with the fact that Friedman's methodology still lives in the hearts and minds of many, if not most, mainstream economists. The most recent attack on the messenger reads like a case written by Crown Prosecutor, providing what is purported to be indisputable evidence that I must be hopelessly incompetent when it comes to reading the printed word and to discussing the fine points of Aristotelian logic<sup>1</sup>. Since the most recent attack [Dennis, 1986] was published in this **Journal**, I am taking this opportunity to present my defense<sup>2</sup>. The readers of this **Journal** are invited to join my jury.

### THE EVIDENCE

My learned colleague from Manitoba has charged me with (1) not understanding the essence of instrumentalism, (2) not representing Friedman's essay correctly, (3) not understanding the history of logic, (4) making grievous logical errors, and above all, (5) not understanding the essence of "modern formal logic". I plead Not Guilty to all charges.

My learned friend begins his prosecution with an impressive display of serious concern for proper terminology. It would, of course, be wise for any prosecutor to make clear to a jury which includes readers of this **Journal** that he understands the essence of Dewey's use of the term "instrumentalism" since these readers are by and large very familiar with the writings of John Dewey. Furthermore, if he can do this with a lot of puffery and indignation showing, all the better.

But, while this may be a very clever rhetorical stance, I still do not understand what a discussion of Dewey's instrumentalism has to do with my 1979 essay. Obviously, I carefully defined how I was to use the term "instrumentalism" in my article and I adhered to my definition throughout – in a most consistent fashion. Nowhere in my article did I mention Dewey or Dewey's instrumentalism. Nowhere have I said that Friedman's instrumentalism was in any way related to Dewey's instrumentalism.

Perhaps my learned friend presumes that if Dewey was the first to use the term "instrumentalism" then no one else should use that term in any other way. If we accept this presumption and this type of rhetoric, then it should also be true that whenever my learned friend uses the term "logic" he should always mean Aristotelian logic since Aristotle came before all the modern formalist logicians. More on this later. I ask the jury to ignore my learned colleague's untimely outburst on the essence of instrumentalism since it was introduced only to deaden your senses concerning fair play and justice.

Also, I ask the jury to ignore my learned friend's extensive arguments for his claim that the "text of [Friedman's] essay scarcely supports the interpretation Boland tries to place on it". This is not an argument I have to defend. Professor Friedman has told me and several others that he completely accepts my 1979 interpretation of his essay. Given such expert testimony, there is nothing more for me to do. My learned friend will have to figure out how to bring Milton to court to face cross-examination.

My learned friend claims I said "it is difficult to find the 'focus' on the 'exact purpose' of Friedman's essay" [Dennis, p. 653] and thereby claims I said Friedman's essay needed clarification. This claim is both misleading and unfair. What I actually said was, "Because the essay is long, it is hard to focus on its exact purpose" [p. 509]. I am not going to spend much time trying to understand why my learned friend would resort to such poor scholarship. Unfortunately, his footnote 3 goes even further by suggesting that Friedman believes his famous essay needed clarification. But, I cannot see how Professor Friedman's explicit statement that I correctly represented his essay implies it needed clarification. Surely, by correctly stating something one does not always perform an act of clarification.

Let us now turn to the methodology of my learned colleague's attack on my 1979 article. Why, it might be asked, has my learned colleague carried on for almost three pages trying to show that I do not understand the essence of Aristotle's logic? Surprisingly, he provides an answer to this question. He claims he is offering evidence that I "lack a command of elementary principles of formal logic" [Dennis, p. 650]. And thus he wants you to conclude, he says, that my "discussion of hypothesis-testing is [... no ...] less suspect". I urge you not to be fooled. Again, my learned colleague is trying to cloud your vision – the nature of Aristotle's logic played no role in my 1979 article. Since he thinks my few references to Aristotle are very revealing, let me be more specific.

Let the record of evidence show the following. In my 1979 article I mentioned Aristotle in two paragraphs and one footnote. The point of the first mention was that we should follow Aristotle and not separate questions of truth from logical validity (as many modern formalist logicians are wont to do). The second mention (including the footnote) was to explain that "Aristotle was concerned with determining what kinds of statements are admissible into logical arguments". Nowhere in my references to Aristotle did I connect him to the more modern terminology about how logic is used (viz. *modus ponens* and *modus tollens*). Yet, my learned colleague would have you believe that I was claiming that Aristotle lectured about such things as *modus ponens* and "the conditional form of statement and argument". Nowhere did I do any such thing. This kind of accusation and misrepresentation of my paper is both unfair and misleading.

My learned colleague from Manitoba would have you believe that, unlike him, I was unable to read the well-known history of logic by William and Martha Kneale [1962]. The irony is that this book is precisely the one I used as a reference to write the sections on *modus ponens* as well as my later discussion of the “material conditional”. Moreover, it was on the basis of this book that in my first mention of Aristotle’s logic I said that “Logic has not changed much since [Aristotle’s time], although some presentations lead one to think that our logic is different.” My very next sentence was, “Modern writers too often discuss logic as if it had nothing to do with truth.” Now let me be absolutely clear here. The logic that Aristotle discussed has nothing whatsoever to do with “modern formal logic”. The logic that Aristotle discussed, and the one which I reviewed in my 1979 article, is the one ordinary people use everyday. Ordinary logic has not changed since Aristotle’s day. Ordinary individuals never find a need to use “modern formal logic” in their everyday arguments. I have never heard of a lawyer using “modern formal logic” to present his case to a judge or jury. Despite my learned colleague’s obvious attempt to mislead the jury into thinking that I was totally ignorant of modern formalist logic when I claimed logic has not changed much, he lets the cat out of the bag by admitting that “true connoisseurs of formal logic will be aware of recent attempts to modernize Aristotelian logic ... but so far these efforts have had little success”. I think he must concede that I have won this point.

To press his personal attack, my learned Manitoban colleague refers to my 1982 book and accuses me of making a logical error on page 138. He says there I “reduce the material conditional ... to a conjunction” (Dennis, pp. 648-9). My learned friend is wrong. I have not done what he says. But more importantly, he has taken my discussion out of context. The question raised in my book is whether material conditionals are admissible into logical arguments about empirical facts. Even more important, in my book the discussion is about how and why modern formal logic is misleading. By taking truth tables for granted, formalist logicians claim that the statement “if P then Q” must be considered true whenever P is false (see for example, Dennis, p. 646). My learned friend claims that I must accept this interpretation of conditional statements since “it has worked well in formal logic and is the only interpretation ... in use by formal logicians” (Dennis, p. 646). I do not care how many “formal logicians” accept this nonsense, I certainly do not accept it nor do I feel any need to accommodate these formalists. Somehow, I feel my friend would not accept Friedman’s argument that we should all accept neoclassical economics simply on Friedman’s claim that it has worked well in the minds of many economists.

My Manitoban friend would have you believe that my article is riddled with logical errors. He gives only one example taken from a footnote – namely, my illustration of how a logically valid argument can be misused. Now despite the fact that this example has nothing to do with Aristotle, he persists in claiming it shows how ignorant I am in not realizing that the example is impossible “in traditional Aristotelian terms”. Since I never claimed it was possible in “Aristotelian terms”, what does his discussion supposedly establish? Well, in this case he says “modern formal logic” comes to the rescue by showing that my example is not logically valid by the rules of “modern formal logic”. This is not only unfair, it is quite bizarre. Since he has referred to my 1982 book, I know he is aware that I explicitly reject what he calls “modern formal logic” as well as all such self-serving exercises in formalism-for-formalism’s-sake.

This brings us then to the keystone of my learned colleague’s case against me – my alleged lack of an understanding of “modern formal logic”. Let me state my position for the record. I am very much opposed to unnecessary and self-serving formalism. For the record, it should be noted that formalism in mathematics was primarily an early 20th century phenomena where the methodological program of formalism was promoted as the means of formalizing the proof procedure (see Meschkowski, 1965, Chapter 10). The foundation of this early program of

formalism was to be mathematical logic or what my learned friend calls “modern formal logic”. Formalism in economics has also been active for several decades although its growth was greatest in the 1970s. The excessive formalism of recent mathematical economics is of immediate concern to many economists today (see Grubel and Boland, 1986). Much of mainstream economics has been taken over by formalists who are quite willing to assume anything to make their models formally complete (see also Boland, 1986). Realism and relevance are virtually of no concern in the many journals which devote most of their space to mathematical economics.

It is all too easy for us to see that “modern formal logic” is to ordinary logic what mathematical economics is to ordinary economics. Despite all the resources devoted to the self-serving games of mathematical economics, hardly anything useful has been learned over the last fifty years of its development. Mathematical economists will likely counter that mathematical economics gave us linear programming and input/output analysis but these examples are misleading. These techniques have their origins in attempts to solve practical problems rather than in the fortunate outcomes of a few mathematical economists entertaining themselves by assuming whatever happens to meet their fancy. On the basis of its track record, the practice of formalism-for-formalism’s-sake whether in mathematical economics or in formal logic has not proven to be a very useful methodological exercise.

#### THE SUMMATION

I have presented enough evidence to summarize my defense. I am not guilty of misrepresenting instrumentalism. I am not guilty of the logical errors my learned colleague attributed to me. I am not guilty of misusing Aristotelian logic. However, I am most definitely guilty of not respecting the wishes of modern formalists, be they logicians, mathematicians, or mathematical economists.

Much of my learned colleague’s paper presents his formalist critique of Friedman’s essay with specific reference to Friedman’s views of assumptions, predictions and hypothesis testing. I have made no attempt in this paper (or elsewhere, for that matter) to *defend* Milton Friedman or his essay. Nor have I tried to defend the well-known philosopher of science, Karl Popper, who also received some scorn from my learned Manitoban friend. To the contrary, neither Friedman nor Popper need any help from me.

My learned colleague from Manitoba has presented a long and intense paper that purports to put me and Professor Friedman in our place – supposedly somewhere among the “crackpots” and “quacks”. I ask the jury to ignore these youthful excesses of intensity and focus instead on the logic of his case. His paper begins and ends with a discussion of Dewey’s instrumentalism. At the beginning, his discussion is intended to lead the audience astray by claiming the term instrumentalism used throughout my 1979 paper is not the instrumentalism that Dewey discussed. Of course, I accept this claim since I never said Friedman’s instrumentalism was the same as Dewey’s. At the end, his discussion is intended to meet a challenge I presented in my 1979 paper. Namely, given that Friedman’s instrumentalist methodology is the centerpiece of his famous methodological essay, an effective criticism of that essay must be internal and thus somehow deal with Friedman’s instrumentalism. Unfortunately, my learned friend does not deal with Friedman’s instrumentalist methodology but instead with Dewey’s instrumentalism. Since my friend has already argued that Friedman’s instrumentalism is definitely not Dewey’s, he cannot now use Dewey’s version to form an internal criticism.

Most of my learned colleague’s criticisms of my views of testing would have you think that I have only superficially considered the question of hypothesis testing in economics. He wishes you to believe my views of testing are thus suspect. To do so, however, he conceals some relevant

information from your view. He never deals with the dozen or so of my widely available papers where I discuss testing in economics (Boland 1968, 1969, 1970, 1971, 1974, 1975, 1977a, 1977b, 1977c, 1977d, 1981, 1985). Instead of dealing with my published views of testing, he attempts to discredit my alleged expertise in logic in order to argue on the basis of “guilt by association”.

The bulk of my learned friend’s critique of my views of logic rests on two things – my opening reference to Aristotle and my innocent attempt to assist those readers “unfamiliar with formal logic”. My use of the word “formal” seems to have violated a cardinal rule. Of course, I nowhere presented a formalist version of logic nor did I suggest anywhere that I had. Moreover, I have never claimed to be an expert on questions of logic yet everything my learned colleague says seems directed at proving to you that I am not an expert. Now, I cheerfully admit my lack of expertise in logic. But, my saying that I am not an expert should not be interpreted as my signing away any right to think for myself. I am certainly not going to be convinced by formalist logicians, formalist economists or mathematicians who argue that I should reject Aristotle’s logic in favor of demonstrably empty formalism.

#### Footnotes

1. Actually, there is much more low-level name calling than is appropriate for a scholarly journal, but given the obviously strong feelings involved in discussions of Friedman’s methodology, such a level of discussion is not unexpected. Certainly, most readers will agree, vitriolics and name-calling will never be considered a good substitute for a logically sound argument or criticism.

2. The article in question [Dennis, 1986] is the most vitriolic criticism I have ever read in a leading journal. Normally I would not respond to such an attack but since this **Journal** is widely read, I feel honor bound to respond.

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