

## A Critique of Friedman's Critics

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Milton Friedman's essay 'The methodology of positive economics' [1953] is considered authoritative by almost every textbook writer who wishes to discuss the methodology of economics. Nevertheless, virtually all the journal articles that have been written about that essay have been very critical. This is a rather unusual situation. The critics condemn Friedman's essay, but virtually all the textbooks praise it. Why should honest textbook writers ignore the critics? It will be argued here that the reason is quite clear. *Every* critic of Friedman's essay has been wrong. The fundamental reason why all of the critics are wrong is that their criticisms are not based on a clear, correct or even fair understanding of his essay. Friedman simply does not make the mistakes he is accused of making. His methodological position is both logically sound and unambiguously based on a coherent philosophy of science – instrumentalism.

In order to defend Friedman from his critics, I shall outline some necessary background knowledge – a clear understanding of the nature of logic and the philosophy of instrumentalism – and then present a reader's guide to his essay. Based on this background knowledge and the reader's guide, I shall survey and comment upon the major critics of Friedman's methodology. I shall conclude with a suggestion as to how a fair criticism would proceed.

### THE USEFULNESS OF LOGIC

#### ***Modus ponens*: logic's only useful property**

Aristotle was probably the first to systemize the principles of logic; most of them were common knowledge in his time. Logic has not changed much since then, although some presentations lead one to think that our logic is different. Modern writers too often discuss logic as if it had nothing to do with truth. But such a view of logic is an error. In Aristotle's view logic was the study of the principles of true and *successful* argument.<sup>1</sup>

Recognizing that arguments consist only of individual statements joined together with an 'and' or an 'or', Aristotle was concerned with determining what kinds of statements are admissible into logical arguments. He posited some rules that are in effect necessary conditions for the admissibility of statements into a logical argument. These rules, which later became known as the axioms or canons of logic, cannot be used to justify an argument; they can only be used to criticize or reject an argument on the grounds of inadmissibility.<sup>2</sup>

The only purpose for requiring arguments to be logical is to connect the truth of the premises or assumptions to the truth of the conclusions. Merely joining together a set of admissible statements does not necessarily form a logical argument; the only criterion for whether an admissible argument is logical is whether it is a sufficient argument in favor of its conclusions in the following sense. *If* your argument is logical, then whenever *all* of your assumptions (or premises) are true *all* of your conclusions will be true as well.

To prove that an argument is logical, one must be able to demonstrate its sufficiency. Whenever one establishes the logical sufficiency of a formal (or abstract) argument, one can use that formal

argument as a part of a larger empirical (or contingent) argument that is *in favor* of the truth of any particular conclusion of the formal argument.<sup>3</sup> That is to say, whenever you offer an empirical argument in favor of some proposition, you are purporting both that the form of the argument is logically valid *and* that your assumptions are true. In this sense, logical validity is a necessary (but not sufficient) condition for an empirical argument to be true.

Using a formal argument in favor of the truth of any of its conclusions by arguing from the truth of its assumptions is said to be using the argument in the affirmative mode – or, more formally, in *modus ponens*. The ability to use any argument successfully in *modus ponens* is the primary necessary condition for the argument's logical validity or consistency (or, for short, its 'logicality'). However, this is not the only necessary condition for an argument's logicality. Whenever *modus ponens* is assured for a given argument, that argument can always be used in a denial or criticism of the truth of its assumptions. Specifically, *if* your argument is logical, then any time *any one* conclusion is false *not all* of your assumptions can be true (i.e. at least one assumption must be false).<sup>4</sup> Using this mode of argument against the truth of one's assumptions by arguing from the falsity of a conclusion is called *modus tollens*. Whenever one successfully criticizes an argument by using *modus tollens*, one can conclude that either an assumption is false *or* the argument is not logical (or both).

### **Beyond *modus ponens***

In order to distinguish *modus ponens* from its corollary *modus tollens*, not only must we explicitly refer to truth and falsity, but we must also specify the direction of the argument. Heuristically speaking, *modus ponens* 'passes' the truth *forward* from the assumptions to the conclusions.<sup>5</sup> *Modus tollens*, on the other hand, 'passes' the falsity *backward* from the conclusions to one or more of the assumptions.<sup>6</sup> The important point here, which I shall argue is implicitly recognized by Friedman in his essay, is that if one changes the direction (forward or backward) of either valid mode of using a logical argument, then the logicality of one's argument ceases to be useful or methodologically significant. Specifically, any use of *modus ponens* in *reverse* is an example of what logic textbooks call 'the Fallacy of Affirming the Consequent'. Similarly, any use of *modus tollens* in *reverse* is an example of what is called 'the Fallacy of Denying the Antecedent'. It is especially important to note that truth cannot be 'passed' backward nor can falsity be 'passed' forward.<sup>7</sup>

The major point to be emphasized here is that while the truth of assumptions and conclusions is connected in the use of a logical argument in *modus ponens*, the truth of the same assumptions and conclusions is not connected if they are used in *reverse modus ponens*. Similarly, their falsity is not connected when used in *reverse modus tollens*.

I think an explicit recognition of the two *reverse* modes of argument is essential for a clear understanding of Friedman's essay. Any methodological criticism which presumes that any formal argument that can be used in *modus tollens* can also be validly used in *reverse modus ponens* involves a serious methodological error. Recognition of this methodological error, an error which Friedman successfully avoids, is essential for an appreciation of his rejection of the necessity of testing (as I will show in the third section).

### Objectives of an argument: necessity vs sufficiency

Finally, there is another aspect of the logicity of an argument that is reflected in Friedman's essay. It has to do with the 'necessity' and the 'sufficiency' of statements or groups of statements. In some cases one is more concerned with the sufficiency of an argument; in other cases one is more concerned with the necessity of its assumptions. To illustrate, consider the following *extreme* dichotomization. There are basically two different affirmative types of argument: the conjunctive and the disjunctive.

*Conjunctive type of argument:* Because statement  $A_1$  is true, and  $A_2$  is true, and  $A_3$  is true, and " , one can conclude that the statement  $C_1$  is true.

Axiomatic consumer theory might be an example of such an argument where the  $A$ s include statements about the utility function and the existence of maximization is the conclusion. On the other hand,

*Disjunctive type of argument:* Because statement  $R_1$  is true, or  $R_2$  is true, or  $R_3$  is true, or " , one can conclude that the statement  $C_2$  is true.

A politician's reasons for why he or she is the best candidate might be an example of this type of argument. These two ways of arguing can be most clearly distinguished in terms of what is required for a *successful refutation* of each type of argument. The conjunctive type of argument is the easiest to refute or criticize. Ideally, a pure conjunctive argument consists of assumptions *each of which is offered as a necessary condition*. It is the conjunction of *all* of them that is *just* sufficient for the conclusion to follow. If any one of the assumptions were false, then the sufficiency of the argument would be lost. To refute a pure conjunctive argument, one needs only to refute *one* assumption. The disjunctive argument, on the other hand, is very difficult to refute. Because in the extreme case such an argument, in effect, offers every assumption as a *solitarily sufficient condition* for the conclusion to follow, none of the assumptions are necessary. If someone were to refute only *one* of the assumptions, the argument would not be lost. In order to defeat a pure disjunctive argument, one must refute *every* assumption – clearly a monumental task.<sup>8</sup>

## 'INSTRUMENTALISM' AND THE RELATIONSHIP BETWEEN LOGIC, TRUTH AND THEORIES

### The problem of induction

The discussion so far has not worried about how one knows the truth of the assumptions (or conclusions). Unfortunately, logic is of little help in determining the truth of a statement. Logic can only help by 'passing' along known truths. This limitation of traditional logic leads to a consideration of the so-called *problem of induction*: the problem of finding a *form* of logical argument where (a) its conclusion is a *general* statement, such as one of the true 'laws' of economics (or nature), or its conclusion is the choice of the true theory (or model) from among various competitors; and (b) its assumptions include *only* singular statements of *particulars* (such as observation reports). With an argument of this form one is said to be arguing inductively from

the truth of particulars to the truth of generals. (On the other hand, a deductive form of argument proceeds from the truth of generals to the truth of particulars.) If one could solve the problem of induction, the true 'laws' or general theories of economics could then be said to be induced logically from the particulars. But not only must one solve the problem of induction, one must also acquire access to all the particulars needed for the application of the solution. Any 'solution' that requires an infinity of particulars is at best impractical and at worst an illusion. The requirement of an infinity of true particulars in order to provide the needed true assumptions for the application of *modus ponens* means in effect that such an inductive argument would not carry the force of *modus ponens*.

One might ask, just what determines whether or not a form of argument is logical? But I have already discussed this question above. As noted in the first section, the criterion or necessary condition for any logical argument is that it must be capable of fulfilling the promise of *modus ponens*. However, as far as anyone knows *modus ponens* is assured only by a 'deductive' form of argument.

### **'Inductivism'**

One can identify (at least) three different views of the relationship between logic, truth and theories. The 'inductivists' say that theories can be true and all true theories (or assumptions) are the result of applying inductive logic to observations. 'Conventionalists' deny that a theory can be inductively proven, and they furthermore consider it improper to discuss the truth status of a theory. 'Instrumentalists', such as Friedman, are only concerned with the usefulness of the conclusions derived from any theory. Unlike conventionalists, instrumentalists may allow that theories or assumptions can be true but argue that it does not matter with regard to the usefulness of the conclusions.

A clear understanding of inductivism, I think, is essential for the appreciation of every modern methodological point of view. Even when economists only argue deductively (that is, by using *modus ponens* and including assumptions that are necessarily in the form of general statements), it might still be asked, how do they know that the 'laws' or other general statements used are true? The inductivist philosophers have always taken the position that there is a way to prove the truth of the needed general statements (as conclusions) using only assumptions of the form of singular statements (e.g. observations). Such inductivists often think the only problem is to specify which kinds of singular statements will do the job, that is, those which are unambiguously true and capable of forming a sufficient argument for the truth of a given statement or conclusion.

What kinds of statements must economists rely on? Clearly, biased personal reports will not do even if their conjunction could be made to be sufficient. For this reason inductivist philosophers and many well-known economists (following John Neville Keynes) distinguish between 'positive' statements, which can be unambiguously true, and 'normative' ones, which cannot. Singular positive statements would supposedly work because they can be objectively true. But normative statements are necessarily subjective, hence they would not carry the same logical guarantee of unambiguous truth.

Contrary to the hopes of the inductivists, even though one can distinguish between positive and normative statements, there is no inductive logic that will guarantee the sufficiency of any finite set of singular statements. There is no type of argument that will validly proceed from assumptions that are singular to conclusions that are general statements. Specifically, there is no conjunction of

a *finite* number of true singular statements from which unambiguously true general statements will validly follow with the assurance of *modus ponens*. Thus, distinguishing between positive and normative statements (as most economists do today) will not by itself solve the problem of induction;<sup>9</sup> and for this reason Friedman tries to go *beyond* this distinction.

### **The ‘conventionalist’ alternative to inductivism**

Since no one has yet solved the problem of induction, one is always required to assume the truth of his or her premises or assumptions. In response to the failure to solve the problem of induction, some philosophers and economists go as far as to avoid using the word ‘truth’ at all. They may, however, attempt to determine the ‘validity’ of a theory or argument, since logic can (at least) help in that determination. Too often, many economists who are unaware of these methodological problems create much confusion by using the word ‘validity’ when they mean ‘truth’ [e.g. see Friedman 1953, pp. 10ff.]. Their formal alternative to avoiding the word ‘truth’ is to take the position that ‘truth’ is a matter of convention; philosophers who take such a position are thus called ‘conventionalists’. They view theories as being convenient catalogues or ‘filing systems’ for positive reports. Of course, catalogues cannot be properly called true or false. They are to be judged or compared only by criteria of convenience such as simplicity or degrees of approximation or closeness of ‘fit’, etc.

Conventionalism forms the foundation for most methodological discussions in economics today (e.g. which criterion is best, simplicity or generality?). It is also the primary source of methodological problems because its usual application is built upon a fundamental contradiction. Conventionalists presume that it is possible to discuss logical validity without reference to truth or falsity. Yet, as noted above, the fundamental aspect of logic that defines ‘validity’ (namely, the assurance of *modus ponens* or *modus tollens*) requires an explicit recognition of (a concept of) truth or falsity.<sup>10</sup> Conventionalism does not offer a solution to the problem of induction; it only offers a way to avoid discussing such philosophical obstacles. Although Friedman accepts and employs several conventionalist concepts, to his credit he constructs a methodological approach that goes beyond the sterile philosophy of conventionalism.

### **Instrumentalism and the usefulness of logic**

For the purposes of discussing Friedman’s point of view, one can consider any theory to be an argument in favor of some given propositions or towards specific predictions. As such a theory can be considered to consist only of a conjunction of assumption statements, that is, statements, each of which is *assumed* (or asserted) to be true. In order for the argument to be sufficient it must be a deductive argument, which means that at least some of the assumptions must be in the form of general statements. But, without an inductive logic, this latter requirement seems to raise in a modified form the methodological problems discussed above. When can one assume a theory is true? It is such difficulties that Friedman’s essay attempts to overcome.

So long as a theory does its intended job, there is no apparent need to argue in its favor (or in favor of any of its constituent parts). 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. This view of the *role* of theories is called ‘instrumentalism’. It

says that theories are convenient and useful ways of (logically) generating what have turned out to be true (or successful) predictions or conclusions. Instrumentalism is the primary methodological point of view expressed in Friedman's essay.

For those economists who see the object of science as finding the *one* true theory of the economy, their task cannot be simple. However, if the object of building or choosing theories (or models of theories) is only to have a theory or model that provides true predictions or conclusions, *a priori* truth of the assumptions is not required *if* it is already known that the conclusions are true or acceptable by some conventionalist criterion.<sup>11</sup> Thus, theories do not have to be considered true statements about the nature of the world, but only convenient ways of systematically generating the already known 'true' conclusions.

In this manner instrumentalists offer an alternative to the conventionalist's response to the problem of induction. Instrumentalists consider the truth status of theories, hypotheses or assumptions to be irrelevant for any practical purposes so long as the conclusions logically derived from them are successful. Although conventionalists may argue about the nature or the possibility of determining the truth status of theories, instrumentalists simply do not care. Some instrumentalists may personally care or even believe in the powers of induction, but such concern or belief is considered to be separate from their view of the role of theories in science.

For the instrumentalists, who think they have solved the problem of induction by ignoring truth, *modus ponens* will necessarily be seen to be irrelevant. This is because they do not begin their analysis with a search for the true assumptions but rather for true or useful (i.e. successful) conclusions. *Modus tollens* is likewise irrelevant because its use can only begin with false conclusions. This also means that like the pure disjunctive argument, the instrumentalist's argument is concerned more with the sufficiency of any assumptions than with their necessity. This is because any analysis of the sufficiency of a set of assumptions begins by assuming the conclusion is true and then asks what set of assumptions will do the logical job of yielding that conclusion. Furthermore, any valid or fair criticism of an instrumentalist can only be about the sufficiency of his or her argument. The only direct refutation allowable is one that shows that a theory is insufficient, that is, inapplicable. Failing that, the critic must alternatively provide his or her own sufficient argument, which does the same job.

By identifying three distinct philosophical views of theories, I am not trying to suggest that one must choose one (that would merely be reintroducing the problem of induction at a new level). Few writers have ever thought it necessary to adhere to just one view. Most writers on methodology in economics make some use of each view. For this reason it is sometimes necessary to sort out these views in order to make sense of methodological essays. I hope to show that even a superficial understanding of these philosophical views will help form a clear understanding of Friedman's 1953 essay.

## **A READER'S GUIDE TO FRIEDMAN'S ESSAY**

### **An overview**

Friedman's 1953 essay is rather long and rambling. However, he does manage to state his position regarding all of the issues I have discussed so far. Because the essay is long, it is hard to focus on its exact purpose, but I think it can best be understood as an instrumentalist's argument for instru-

mentalism. As such it tries to give a series of sufficient reasons for the acceptance of instrumentalism. And furthermore, it can be fairly judged only on the basis of the adequacy or sufficiency of each reason for that purpose. We are told that the essay's motivation is to give us a way to overcome obstacles to the construction of a 'distinct positive science' centering on the problem of 'how to decide whether a suggested hypothesis or theory should be tentatively accepted as part of the "body of systemized knowledge [of] "what is"' [p. 3]. The 'distinct positive science', we are told, is essential for a policy science [pp. 5–7]. This methodological decision problem is, in fact, an inductivist's problem.<sup>12</sup> Implicitly Friedman recognizes that we do not have an inductive logic [p. 9], and he offers what he considers to be an acceptable alternative. Basically Friedman's solution (to the problem of induction) is that our acceptance of a hypothesis for the purposes of policy application should be made a matter of 'judgement'. Judgements, he says, cannot be made *a priori* in the absence of a true inductive science.

#### **'Positive vs. normative economics': the problem of induction in instrumentalist terms**

In the introduction Friedman expresses his interest in the problem of induction and then, in Section I, he restates the problem in instrumentalist terms. He says the task of positive economics is to

provide a system of generalizations that can be used to make correct predictions about the consequences of any change in circumstances. Its performance is to be judged by the precision, scope, and conformity with experience of the predictions it yields. [p. 4]

The inductivist's distinction between positive and normative statements is the most important part of inductivism that is retained by Friedman. And he brings with that distinction the inductivist's claim that normative economics depends on positive economics, but positive economics does not necessarily depend on the normative [p. 5]. In this light he notes that even methodological judgements about policy are also positive statements to be accepted on the basis of empirical evidence [pp. 6–7].

#### **'Positive economics': conventionalist criteria used with an instrumentalist purpose**

Friedman begins Section II with a mild version of conventionalism by saying that a theory (i.e. a set of assumptions) can be viewed as a language whose

function is to serve as a filing system for organizing empirical material " and the criteria by which it is to be judged are those appropriate to a filing system. [p. 7]

But his viewing a theory as a language has its limitations. I would think that a distinguishing feature of all languages is that they are intended to be both consistent and complete (e.g. there should be nothing that cannot be named or completely described); and this would preclude empirical applications as the theory would, in effect, yield only tautologies. To avoid this he adopts the now popular opinion that we must add 'substantive hypotheses' [p. 8]. But here he again raises an inductivist's problem: how do we choose the substantive hypotheses? Friedman answers that positive statements ('factual evidence') can determine acceptance. He clearly indicates that he does understand the fundamentals of logic by implicitly using *modus tollens*. He

says that a ‘hypothesis is rejected if its predictions are contradicted’ [p. 9]. But what about *modus ponens*? Well, that is considered inapplicable because there is no inductive logic. Friedman, using the word ‘validity’ when he means ‘not inconsistent with facts’ (which happens to be a necessary condition of true hypotheses), says:

The validity of a hypothesis in this sense is not by itself a sufficient criterion for choosing among alternative hypotheses. Observed facts are necessarily finite in number; possible hypotheses, infinite. [p. 9]

In other words, one cannot directly solve the problem of induction.

All this means that the main task of a positive economics is left unfulfilled. At this point Friedman says that we need additional criteria (beyond consistency with the facts) if we are going to be able to choose [p. 9]. Here he poses the problem of choosing between *competing* hypotheses or theories, *all* of which have already been shown to be consistent with available positive evidence (that is, none of them have been shown to be false using *modus tollens*). The criteria with which he claims there is ‘general agreement’ are the ‘simplicity’ and the ‘fruitfulness’ of the substantive hypotheses [p. 10].<sup>13</sup> However, these are not considered to be abstract philosophical (i.e. conventionalist) criteria but rather they, too, are empirically based, hence can be expressed in instrumentalist terms: ‘simpler’ means requires less empirical ‘initial knowledge’ (the word ‘initial’ refers here to the process of generating predictions with something like *modus ponens*). ‘More fruitful’ means more applicable and more precise [p. 10]. The possibility of a tradeoff is not discussed.

Friedman explicitly rejects the necessity of requiring the ‘testing’ of substantive hypotheses before they are used simply because it is not possible. But here it should be noted that his rejection of testing is partly a consequence of his use of the word ‘testing’. Throughout his essay ‘testing’ always means ‘testing for truth (in some sense)’. It never means ‘testing in order to reject’ as most of his critics seem to presume. That is, for Friedman a *successful* test is one which shows a statement (e.g. an assumption, hypothesis or theory) to be true; and, of course, a minimum condition for a successful test is that the statement not be inconsistent with empirical evidence [see pp. 33–4].<sup>14</sup>

Appreciating the success orientation of Friedman’s view is essential to an understanding of his methodological judgements. For Friedman, an instrumentalist, hypotheses are chosen because they are successful in yielding true predictions. In other words, hypotheses and theories are viewed as instruments for successful predictions. It is his assumption that there has been a prior application of *modus tollens* (by evolution, see [p. 22]), which eliminates unsuccessful hypotheses (ones that yield false predictions), and which allows one to face only the problem of choosing between successful hypotheses. *In this sense*, his concentrating on successful predictions precludes any further application of *modus tollens*. And similarly, any possible falsity of the assumptions is thereby considered irrelevant. Such a consideration is merely an appreciation of the logical limitations of what I above called *reverse modus tollens*. And since he has thus assumed that we are dealing exclusively with successful predictions (i.e. true conclusions), nothing would be gained by applying *modus ponens* either. This is a straightforward appreciation of the limitations of what I called *reverse modus ponens*. Knowing for sure that the hypotheses (or assumptions) are true is essential for a practical application of *modus ponens*, but such knowledge, he implies, is precluded by the absence of an inductive logic [pp. 12–14].



By focusing only on successful hypotheses, Friedman correctly reaches the conclusion that the application of the criterion of ‘simplicity’ is relevant. He says there is virtue in a simple hypothesis *if* its application requires less empirical information. One reason a simple hypothesis can require less information, Friedman says, is that it is descriptively false [pp. 14–15]. (For example, a linear function requires fewer observations for a fit than does a quadratic function.) This raises the question of ‘unrealistic’ descriptions versus ‘necessary’ abstractions. Friedman explicitly recognizes that some economists (presumably, followers of Lionel Robbins) hold a view contrary to his. For them the ‘significance’ of a theory is considered to be a direct result of the descriptive ‘realism’ of the assumptions. But Friedman claims that

the relation between the significance of a theory and the ‘realism’ of its ‘assumptions’ is almost the opposite. “ Truly important and significant hypotheses will be found to have ‘assumptions’ that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions (in this sense). [p. 14]

Clearly, this latter judgement is based on the additional criteria of importance and significance that presume a purpose for theorizing: namely, that theories are only constructed to be instruments of policy. Those economists who do not see policy application as the only purpose of theorizing can clearly argue with that judgement. But nevertheless, in terms of the economy of information, his conclusion is still correct with respect to choosing between *successful* hypotheses that are used as policy instruments.

#### **‘Realism of assumptions’ vs. the convenience of instrumentalist methodology**

In his Section III, Friedman continues to view successful ‘testing’ to be ‘confirming’, and for this reason he concludes that testing of assumptions is irrelevant for true conclusions (since *modus ponens* cannot be used in reverse). Having rejected the necessity of testing for the truth of assumptions, Friedman examines the question of the relevance of the falsity of assumptions for the various uses of theories. That is, what if one could show that an assumption is false? Does it matter? Friedman argues again [p. 18] that the falsity of the assumptions does not matter *if the conclusions are true*. He correctly says: one can say there must be an assumption that is false *whenever* some particular conclusion is false (*modus tollens*), but one cannot say any assumptions are true *because* any conclusion is true (*reverse modus ponens*, again) [p. 19].

This leads Friedman to discuss the possibility that a false assumption might be applied as part of an explanation of some observed phenomenon. Here he introduces his famous version of the ‘as if’ theory of explanation. He says that as long as the observed phenomenon can be considered to be a logical conclusion from the argument containing the false assumption in question, the use of that assumption should be acceptable. In particular, if we are trying to explain the *effect* of the assumed behavior of some individuals (e.g. the demand curve derived with the assumption of maximizing behavior), *so long as the effect is in fact observed and it would be the effect if they were in fact to behave as we assume*, we can use our behavioral assumption even when the assumption is false. That is, we can continue to claim the observed effect of the individuals’ (unknown but assumed) behavior is *as if* they behaved as we assume. Note carefully, the individuals’ *behavior* is not claimed to be *as if* they behaved as we assume, but rather it is the *effect* of their behavior that is claimed to be *as if* they behaved according to our assumption.

Failure to distinguish between the effect and the behavior itself has led many critics to misread Friedman's view. His view does not violate any logical principles in this matter.

So far the choice between competing hypotheses or assumptions has been discussed with regard to currently available observations, that is, to existing evidence. But a more interesting question is the usefulness of any hypothesis in the future; past success will not guarantee future success. This presents a problem for the methodological conclusions that Friedman has, for the most part, presented correctly up to this point. He offers some weak arguments to deal with this problem. The first is an adaptation of a Social-Darwinist view that repeated success in the face of competition temporarily implies satisfaction of 'the conditions for survival' [p. 22]. Unfortunately, he does not indicate whether these are necessary conditions, which they must be if his argument is to be complete. He adopts another Social-Darwinist view, which claims that past success of our theory is relative to other competitors, thereby claiming a revealed superiority of our theory. This unfortunately presumes either that the other theories have not survived as well or that the comparative advantage cannot change. The former presupposition, however, would be ruled out by his prior commitment to discussing the problem of choosing between successful theories [p. 23]. The latter merely begs the question. Finally he unnecessarily adds the false conventionalist theory of confirmation that says the absence of refutation supports the (future) truth of a statement [pp. 22-3].

#### **The 'positive aspects of assumptions' are positive aspects of instrumentalist methodology**

If assumptions do not need to be true, why would one bother worrying about them? Or, in other words, what role do assumptions play? Friedman says their role is positive [p. 23]. Assumptions: (a) are useful as an 'economical mode' of expressing and determining the state of the 'givens' of a theory – that is, the relevant facts – in order to provide an empirical basis for the predictions; (b) 'facilitate an indirect test' of a hypothesis of a theory by consideration of other hypotheses that are also implied; and (c) are a 'convenient means of specifying the condition under which the theory is expected' to be applicable.

Friedman is not very careful about distinguishing between assumptions, hypotheses and theories, and to make matters worse, in his Section IV he introduces the concept of a model. This can present some difficulty for the careful reader. Inductivist methodology posits significant differences between assumptions, hypotheses, theories and some other things that are called 'laws'. The inductivist's distinctions are based on an alleged difference in the levels of inductive proofs of their truth. Assumptions are the least established and laws are the most. Without committing oneself to this inductivist tradition, one can easily see hypotheses as intermediate conjunctions formed by using only part of the assumptions of a theory. For example, the theory of the consumer entails certain hypotheses about the slope of the demand curve, but the assumptions of the theory of the consumer are only part of our market theory of prices. Moreover, the assumptions and hypotheses of consumer theory are independent of the theory of the firm.

Discussing models raises totally new issues. A model of a theory is a conventionalist concept. As Friedman correctly puts it, 'the model is the logical embodiment of the half-truth' [p. 25]. Models in his sense correspond to the concept of models used in engineering. When one builds a model of something, one must simplify in order to emphasize the essential or significant features. Such simplification can always be seen to involve extra assumptions about the irrelevance of certain empirical considerations. These extra assumptions are usually descriptively false.

Most simplifying assumptions are designed to exclude certain real-world complications or variables. Such exclusion also reduces the need for information concerning those variables when one wishes to apply the model. In this sense, assumptions are economical in terms of the amount of prior information required for empirical application.

Friedman notes that the problem of choosing models can be seen as a problem of explaining when the model is applicable. To solve the latter version of this problem, he says that to any model of a theory or hypothesis one must add ‘rules for using the model’ [p. 25]. These required rules, however, are not mechanical. He says that ‘no matter how successful [one is in explicitly stating the rules] “there inevitably will remain room for judgement in applying the rules’ [p. 25]. Unfortunately, the ‘capacity to judge’ cannot be *taught*, as each case is different (another instance of the problem of induction). However, it can be *learned*, ‘but only by experience and exposure in the “right” scientific atmosphere’ [p. 25] (this is a version of conventionalism). This seems to bring us back to the inductive problem that his version of instrumentalism was intended to solve.

In spite of all the discussion about ‘assumptions’, Friedman cautions us not to put too much emphasis on that word. By saying there are problems concerning judgements about the applicability of certain assumptions of particular hypotheses or theories, we are not to be misled into thinking there is some special meaning to the term ‘assumption’. The assumptions of one hypothesis may be the conclusions of a (logically) prior set of assumptions. In other words, when one says a statement is an assumption, one is not referring to any intrinsic property. A statement is called an assumption because that is how one chooses to use it. There is nothing that prevents one from attempting to explain the assumed ‘truth’ of one’s assumption by considering it to be a conclusion of another argument, which consists of yet another set of assumptions.<sup>15</sup> Moreover, the popular notion of a ‘crucial assumption’ is likewise relative to the particular model in which it is being used.

In the last part of his Section IV, Friedman faces an alleged problem that may be created by the dismissal of the testability (i.e. confirmability) of assumptions. The set of conclusions of any argument must contain the assumptions themselves. In some cases, within some subsets of assumptions and conclusions of a given theory there is interchangeability. In these cases dismissing testability of assumptions can seem to mean that the testability of some conclusions has been dismissed as well. Recall, however, that testing for Friedman still means confirming. Thus, if one considers the testing of an assumption one can, in effect, be seen to be considering merely the confirming of one of the conclusions. Friedman’s emphasis on true (successful) conclusions is seen to be playing a role here, too. Of course, there are other conclusions besides the assumptions themselves. However, someone may propose a set of assumptions only because *one* of the (true or observed) conclusions of interest is a logical consequence of that set. If one bothers to use the proposed assumptions to derive other conclusions from these assumptions, one can try to confirm the additional conclusions. In this sense, the assumptions used to derive one conclusion or hypothesis can be used to ‘indirectly test’ the conclusion of interest. Nevertheless, logic does not permit one to see the confirmation of the secondary conclusion as a direct confirmation of the conclusion of interest. The significance of such an indirect test is also a matter of judgement [p. 18].

### **‘Economic issues’ or some examples of instrumentalist successes**

Finally, in his Section V, Friedman applies his methodological judgements to some specific examples, but here he does not raise any new questions of methodology. His objective seems to be merely to provide a demonstration of the success of instrumentalist methodology with several illustrations. Note that such a line of argument is quite consistent with instrumentalism and its compatibility with the disjunctive form of argument.

### **THE CRITICS**

Friedman’s paper elicited a long series of critiques, none of which dealt with every aspect of his essay. The primary motivation for all of the critics seems to be that they disagree with particular things Friedman said. I will argue here that the basis for each of the critiques is a misunderstanding and hence each involves a false accusation.

#### **Testability vs. refutability: Koopmans**

Most misunderstandings are the result of Friedman’s ‘Introduction’, where he seems to be saying that he is about to give another contribution to the traditional discussion about the methodology of inductivism and conventionalism. Such a discussion would usually be about issues such as the verifiability or refutability of truly scientific theories. What Friedman actually gives is an alternative to that type of discussion. Unfortunately, most critics miss this point.

In regard to the traditional discussion, Tjalling Koopmans says that the object of our attempts to develop or analyze the ‘postulational structure of economic theory’ is to obtain ‘those implications that are verifiable or otherwise interesting’ [Koopmans 1957, p. 133]. In this light, Koopmans says that one must distinguish between the logical structure of a theory and the ‘interpretation’ of its terms. He says that the logical structure’s validity is considered to be independent of the interpretations (Koopmans is using the term ‘validity’ correctly, but it does not correspond to Friedman’s usage). He says, ‘from the point of view of the logic of the reasoning, the interpretations are detachable. Only the logical contents of the postulates matter’ [p. 233]. When any argument is logically valid, no interpretation can lead to a contradiction. (This is one interpretation of *modus ponens*.) One way to view the testing of an argument is to see a test as one interpretation of the terms such that a conjunction of the argument and the specific interpretation in question forms an empirical proposition about the real world, which does or does not correspond to our observations.

Koopmans also says a ‘distinction needs to be made here between *explanatory* and *normative* analysis’ [p. 134]. Here Koopmans explicitly equates *positive* with *explanatory*. He adds that

these two types of analysis do not necessarily differ in the interpretations placed on the terms. They differ *only* in the motivation of the search for conclusions. “ In explanatory analysis, what one looks for in a conclusion or prediction is the possibility of testing, that is, of verification or refutation by observation. Of course, the interpretations of the terms used in the postulates form the connecting link through which observation is brought to bear on the statements that represent conclusions. Verification, or absence of refutation, lends support to the set of postulates taken as a whole. [ibid., emphasis added]

Now Friedman clearly does not agree with this distinction since he argues that how one views the parts of a theory depends on its use and that a theory cannot be analyzed independently of its use. Also, Koopmans' statement seems to suggest that priority should be given to testing conclusions. Friedman need not agree. Since Friedman's analysis begins with *successful* conclusions, testing is precluded because it is automatically implied by the usefulness and the logicality of the explanation.

Starting with a different concept of theorizing – that is, that theories are directly analyzable independently of their uses – Koopmans proceeds to criticize Friedman by restating Lionel Robbins' methodological position [Robbins 1935]. The basic concern for Koopmans (but not Friedman) is the sources of the basic premises or assumptions of economic theory. For the followers of Robbins, the assumptions of economic analysis are promulgated and used *because* they are (obviously) true. The truth of the assumptions is never in doubt. The only complaint Koopmans brings against Robbins is that his assumptions were a bit vague – a problem that Koopmans thinks can be solved with the use of sophisticated mathematics. The primary virtue of Koopmans' work is that it does try to solve that problem. Implicitly, both Robbins and Koopmans see the process of economic theorizing as merely the task of applying exclusively *modus ponens* and *modus tollens*. In particular, the sole purpose of developing a theory is so that one can 'pass' the obvious truth of the assumptions on to some conclusions.

Koopmans seems to object to Friedman's dismissal of the problem of clarifying the truth of the premises – the problem that Koopmans wishes to solve. Friedman's view is that (*a priori*) 'realism' of assumptions does not matter (i.e. *modus ponens* is not applicable). The source of the disagreement is Koopmans' confusion of *explanatory* with *positive*. Koopmans is an inductivist, who defines successful explanation as being logically based on observably true premises, that is, ones that are in turn (inductively) based on observation. Friedman does not consider assumptions or theories to be the embodiment of truth but only as instruments for the generation of useful (because successful) predictions. Thus, for Friedman *positive* is not equivalent to *explanatory* because he does not use *modus ponens*. Explanation in Koopmans' sense is irrelevant in Friedman's instrumentalism.

In order to criticize Friedman's argument against the concern for the 'realism' of assumptions, Koopmans offers an *interpretation* of his own theory of the logical structure of Friedman's view. Koopmans says:

Since any statement is implied by itself, one *could* interpret Professor Friedman's position to mean that the validity or usefulness of any set of postulates depends on observations that confirm or at least fail to contradict (although they could have) *all* their implications, immediate and derived. [1957, p. 138, first emphasis added]

He then goes on to claim that this interpretation of Friedman's argument leads to some objectionable conclusions and thus claims to destroy Friedman's argument. The details of this line of argument do not matter here, since Koopmans' argument itself can be shown to be irrelevant and thus of no logical value.

Koopmans' interpretation contradicts Friedman's purpose (that *some* conclusions be successful – not necessarily *all*). Remember that Friedman is only concerned with the *sufficiency* of a theory or set of assumptions. He would allow any theory to be even more than 'just' sufficient<sup>16</sup> so long as it is sufficient for the successful predictions at issue. On the other hand, Koopmans'

interpretation falsely presumes a concern for *necessity*. In other words, Koopmans' theory of Friedman's view is itself void because (by his own rules) at least one of its assumptions is false. Or, also by Koopmans' own rules (*modus tollens*), his own theory of Friedman's view must be considered refuted, since the false assumption is also one of the conclusions. His theory is not 'realistic' even though some of his conclusions may be. There is nothing in the application of *modus tollens* to a specific interpretation (which necessarily involves additional assumptions – e.g. rules of correspondence) that would require the rejection of Friedman's view itself.<sup>17</sup>

### **Necessity of verifying assumptions: Rotwein**

Some economists would accept the obviousness of the premises of economic theory. In this group would fall the self-proclaimed 'empiricists'. The basis of their philosophy is the view that the truth of one's conclusions (or predictions) rests *solely* (and firmly) on the demonstrable truth of the premises; and the prescription that one *must* so justify every claim for the truth of one's conclusions or predictions. Needless to say, empiricists do not see a problem of induction. Friedman clearly does, and in this sense he is not an orthodox empiricist (even though the term 'positive' usually means 'empirical'). According to Eugene Rotwein, Friedman criticizes their view by claiming that it represents 'a form of naive and misguided empiricism' [Rotwein 1959, p. 555]. Actually, Rotwein sees his criticism as a family dispute amongst empiricists. What is questioned is

Friedman's contention "that the 'validity' of a 'theory' is to be tested *solely* by its 'predictions' with respect to a given class of phenomena, or that the question of whether or to what extent the assumptions of the 'theory' are 'unreal' (i.e. falsify reality) is of no relevance to such a test. [p. 556]

(Note that Friedman was not discussing the 'validity of theories' but rather the validity of 'hypotheses' used in a model of a theory.)<sup>18</sup>

Now it seems to me there is 'good' and 'bad' naivety. Good naivety is exemplified by the little boy in Andersen's story 'The Emperor's New Clothes'. Good naivety exposes the dishonesty or ignorance of others. Friedman simply refuses to join in the pretense that there is an inductive logic, one that would serve as a foundation for Rotwein's verificationist-empiricism. Rotwein attempts to twist the meaning of 'validity' into a matter of probabilities so that he can use something like *modus ponens* [p. 558]. But *modus ponens* will not work with statements whose truth status is a matter of probabilities, and thus Friedman is correct in rejecting this approach to empiricism. Rotwein's arguments are on a far weaker foundation than are Friedman's. It is, in fact, Rotwein's view that is naive, since it is based on an unfounded belief that science is the embodiment of truths based (inductively) on true observations, which are beyond doubt, or on true hypotheses, which can be inductively proven.

### **Testability as refutability: Bear and Orr and Melitz**

Some sophisticated and friendly critics of Friedman's methodology choose to criticize only certain aspects while accepting others. This can lead to criticisms that are necessarily invalid. For example, Donald Bear and Daniel Orr dismiss Friedman's instrumentalism, yet they recommend what they call his 'as if' principle [Bear and Orr 1967]. They recommend 'as if' because they too accept the view that the problem of induction is still unsolved. They are correct in appreciating that the principle is an adequate means of dealing with the problem of induction.

That it is possible to accept one part of Friedman's methodology while rejecting another does not necessarily create a contradiction. The appreciation of such a possibility is facilitated by recalling that each part of Friedman's argument is designed to be sufficient. In this vein, Bear and Orr claim that Friedman's arguments against the necessity of testing and against the necessity of 'realism' of assumptions are both wrong. Bear and Orr (agreeing with Jack Melitz) say that Friedman erred by 'confounding " abstractness and unrealism' [1967, p. 188, fn. 3]. And they further claim, 'all commentators except Friedman seem to agree that the testing of the whole theory (and not just the predictions of theory) is a constructive activity' [p. 194, fn. 15].

These criticisms are somewhat misleading because Friedman's concept of testing (sc. verifying) does not correspond to theirs. It is not always clear what various writers mean by 'testing', mostly because its meaning is too often taken for granted. One can identify implicitly three distinct meanings as used by the authors under consideration. Where Friedman sees testing only in terms of verification or 'confirmation', Bear and Orr adopt Karl Popper's view that a successful test is a refutation [Bear and Orr 1967, pp. 189ff.]. But Melitz sees testing as confirmation or disconfirmation [Melitz 1965, pp. 48ff.]. Unfortunately, one can only arrive at these distinctions by inference. Bear and Orr present, in one section, the logic of refuting theories, followed by a lengthy discussion of tests and the logic of testing. Melitz is more difficult to read. The word 'testing', which figures prominently in the article's title, never appears anywhere in the introduction. Melitz never does directly discuss his own concept of testing.

In both critiques, the logic of their criticisms is an allegation of an inconsistency between *their* concepts of testing and Friedman's rejection of the necessity of testing assumptions. The logic of their critiques may be valid, but in each case it presumes a rejection of instrumentalism. But instrumentalism, I argue, is an absolutely essential part of Friedman's point of view. Consequently, contrary to the critics' views, the alleged inconsistency does not exist *within* Friedman's instrumentalist methodology.

As was argued above, Friedman's concept of testing is quite consistent with his instrumentalism and *his* judgements about testing. Viewed from the standpoint of Friedman's concept of testing, Melitz and Bear and Orr present criticisms that are thus logically inadequate. This situation shows, I think, that one cannot understand the particular methodological judgements of Friedman unless one accepts or at least understands his instrumentalism.

Their suggestion that Friedman's view is based on an error of logic is simply wrong. And furthermore, it is unfair to make that suggestion only on the basis of an inconsistency between *their* concept of testing and his judgements, which were based on *his* concept. There is no reason why Friedman's view should be expected to be consistent with their view of what constitutes science or of what others think testability or testing really is.

### Errors of omission: De Alessi

Another even more friendly criticism is offered by Louis De Alessi. He meekly criticizes Friedman for seeing only *two* attributes of theories – namely, a theory can be viewed as a language and as a set of substantive hypotheses. On the other hand, De Alessi seems to think Friedman should have included a set of rules of correspondence or rules of interpretation. His criticism of Friedman is in the spirit that such rules of interpretation are necessary for a positive theory. He says, ‘Unfortunately, Friedman’s analysis has proved to be amenable to quite contradictory interpretations’ [De Alessi 1965, p. 477]. But as I said before, this is not necessarily a criticism for an instrumentalist who has rejected further applications of *modus tollens*.

De Alessi later raises another minor criticism [De Alessi 1971]. He says Friedman leaves room for error by telling us that some assumptions and conclusions are ‘interchangeable’. De Alessi correctly notes that such ‘reversibility’ of an argument may imply that the argument is tautological. When an argument is tautological, it cannot also be empirical, that is, positive. The logic of De Alessi’s argument is correct. However, it is not clear that with Friedman’s use of ‘interchangeable’ he was indicating ‘reversibility’ of (entire) arguments. The only point Friedman was attempting to make was that the status of being an ‘assumption’ is not necessarily automatic. In any case, just because some of the conditions and assumptions are interchangeable does not necessarily mean that the theory as a whole is tautological. If Friedman were viewing assumptions as ‘necessary’ conditions, then the problem that De Alessi raises would be more serious. But Friedman’s instrumentalism does not require such a role for assumptions.

Both of De Alessi’s criticisms are founded on the view that *modus tollens* can be applied to Friedman’s view. In particular, it is the view that was asserted by Koopmans, namely that if *any interpretation* of a view (or argument) is considered false then the view itself must be false. But this presumes that the assumptions were necessary conditions. As I have said, that is not the case with instrumentalism. Hence De Alessi’s criticisms are irrelevant, even though one might find merit in the details of his argument.

### The ‘F-Twist’: Samuelson

The most celebrated criticism of Friedman’s methodology was presented by Paul Samuelson [1963] in his discussion of a 1963 paper by Ernest Nagel.<sup>19</sup> Samuelson explicitly attributes the following proposition to Friedman.

A theory is vindicable if (some of) its consequences are empirically valid to a useful degree of approximation; the (empirical) unrealism of the theory ‘itself’, or of its ‘assumptions’, is quite irrelevant to its validity and worth. [Samuelson 1963, p. 232]

Samuelson calls this the ‘F-Twist’. And about this he says, it is

fundamentally wrong in thinking that unrealism in the sense of factual inaccuracy even to a tolerable degree of approximation is anything but a demerit for a theory or hypothesis (or set of hypotheses). [p. 233]

However, Samuelson admits that his representation of Friedman’s view may be ‘inaccurate’ (that is supposedly why he called it the ‘F-Twist’ rather than the ‘Friedman-Twist’). Nevertheless, Samuelson is willing to apply his potentially false assumption about Friedman to explain (should



one say describe?) Friedman's view. His justification for using a false assumption is Friedman's own allegedly valid 'as if' principle. Samuelson argues in this way on the basis of the theory that if he can discredit or otherwise refute Friedman's view by using Friedman's view, then followers of Friedman's methodology must concede defeat.

Samuelson's argument goes as follows. First he says:

The motivation for the F-Twist, critics say, is to help the case for (1) the perfectly competitive laissez faire model of economics, " and (2), but of lesser moment, the 'maximization-of-profit' hypotheses [p. 233].

Then he says:

If Dr Friedman tells us this was not so; if his psychoanalyst assures us that his testimony in this case is not vitiated by subconscious motivations; " – still it would seem a fair use of the F-Twist itself to say: 'Our theory about the origin and purpose of the F-Twist may be "unrealistic" " but what of that? The consequence of our theory agrees with the fact that Chicagoans use the methodology to explain away objections to their assertions.' [p. 233]

Samuelson admits that there is an element of 'cheap humor' in this line of argument. But nevertheless, it is an attempt to criticize Friedman by using Friedman's own methodology.

I will argue here that Samuelson does not appear to understand the 'as if' principle. I argued above that when using the 'as if' principle, one must distinguish between the empirical *truth* of a behavioral assumption and the *validity of using* that assumption, and I noted that the latter does not imply the former.

Perhaps Samuelson is correct in attributing a pattern of behavior to the followers of Friedman and that such a pattern can be shown to follow logically from his assumption concerning their motivation, but the 'as if' principle still does not warrant the empirical claim that his assumption about Friedman's or his followers' motivation is true. More important, the 'as if' principle is validly used *only* when explaining *true* conclusions. That is, one cannot validly use such an 'as if' argument as a critical device similar to *modus tollens*. If the implications of using Samuelson's false assumption are undesirable, one cannot pass the undesirableness back to the assumption. Furthermore, there are infinitely many false arguments that can imply any given (true) conclusion. The question is whether Samuelson's assumption is necessary for his conclusion. Of course, it is not, and that is because Samuelson is imitating Friedman's mode of argument using sufficient assumptions.

The mode of argument in which Friedman accepts the 'as if' principle is neither a case of *modus ponens* nor one of *modus tollens*. Yet when Samuelson proceeds to give a serious criticism of the 'as if' principle, he assumes that both of them apply. But even worse, by Samuelson's own mode of argument, his assumption that attributes the F-Twist to Friedman is false and his attempts to apply this by means of *modus ponens* are thus invalid.

## ON CRITICIZING INSTRUMENTALISM

It would seem to me that it is pointless (and illogical) to criticize someone's view with an argument that gives different meanings to the essential terms.<sup>20</sup> Yet this is just what most of the critics do. Similarly, using assumptions that are allowed to be false while relying on *modus*

*ponens*, as Samuelson does, is also pointless. Any effective criticism must deal properly with Friedman's instrumentalism. Presenting a criticism that ignores his instrumentalism will always lead to irrelevant critiques such as those of Koopmans, Rotwein and De Alessi. None of these critics seems willing to straightforwardly criticize instrumentalism.

Instrumentalism presents certain obstacles to every critic. When instrumentalists argue by offering a long series of reasons, each of which is sufficient for their conclusions, it puts the entire onus on the critic to refute each and every reason. Friedman makes this all the more difficult by giving us, likewise, an instrumentalist argument in support of instrumentalism itself. Thus, refuting or otherwise successfully criticizing only some of Friedman's reasons will never defeat his view. Since Friedman never explicitly claims that his argument is intended to be a logically sufficient defense of instrumentalism, one cannot expect to gain even by refuting its 'sufficiency'. Yet it would be fair to do so, since 'sufficiency' is the only logical idea that instrumentalism uses. Such a refutation, however, is unlikely, since it would seem to require a solution to the problem of induction.

Finally, and most importantly, I think it essential to realize that instrumentalism is solely concerned with (immediate) practical success. In this light, one should ask, 'What are the criteria of success? Who decides what they are?' Questions of this type, I think, must also be dealt with before one can ever begin – constructively or destructively – to criticize effectively the instrumentalism that constitutes the foundation of Friedman's methodology.

What then must one do to form an effective but fair and logical critique of Friedman's methodology? Whatever one does, one cannot violate the axioms of logic. It does not matter to instrumentalists if others have different definitions of the words 'validity', 'testing', 'hypothesis', 'assumptions', etc. When criticizing an argument in which reasons are offered as sufficient conditions, it should be recognized that *modus tollens* is useless. And when *modus tollens* is useless, there is no way one can directly criticize.

Since, as I have argued here, the internal construction of Friedman's instrumentalism is logically sound, in any effective criticism of his view the only issue possibly at stake is the truth or falsity of instrumentalism itself. But no one has been able to criticize or refute instrumentalism. That no one has yet refuted it does not prove that instrumentalism is universally correct. To claim that it does is to argue (invalidly) from *reverse modus ponens*. Again, this is a matter of logic.

Any effective criticism of instrumentalism must at least explain the absence of refutations. There are, I think, three possible ways any given argument may avoid refutations. First, as a matter of logical form, an argument may merely be irrefutable.<sup>21</sup> Second, if an argument is of a logical form that is conceivably refutable, it may simply be that it is true, hence no one will ever find refutations because they will never exist. Third, the absence of refutations may not be the result of an intrinsic property of the argument itself, but the consequence of how one deals with all potential refutations. That is, the defense may be either circular or infinitely regressive.<sup>22</sup>

As a matter of logic alone, instrumentalism need not be irrefutable. So, as an argument about how one should treat economic analysis, either instrumentalism is true or its proponents have been supporting it with a circularity or an infinite regress. And thus the first question is, is instrumentalism true? Repeated successes (or failed refutations) of instrumentalism are logically equivalent to repeated successful predictions or true conclusions. We still cannot conclude logically that the assumptions, that is, the bases of instrumentalism itself, are true. They could very well be false, and in the future someone may be able to find a refutation.

It has been argued in this paper that Friedman's essay is an instrumentalist defense of instrumentalism. That may be interpreted to mean that Friedman's methodology is based on an infinite regress, but if it is then at least it is not internally inconsistent or otherwise illogical. His success is still open to question. The repeated attempts to refute Friedman's methodology have failed, I think, because instrumentalism is its own defense and its *only* defense.

## NOTES

- 1 However, he also explained how one can win an argument by cheating – for example, by concealing the direction of the argument – see Kneale and Kneale 1962, p. 33.
- 2 Specifically, Aristotle said that in order for an argument to be logical, *the premises must not violate any of the following axioms*: first is the *axiom of identity*, viz different statements cannot use different definitions of the same words; second is the *axiom of the excluded middle*, viz statements that cannot be true or false, or can be something else, are prohibited; and finally, the *axiom of non-contradiction*, viz statements cannot be allowed to be both true and false. Thus, any argument that contains such prohibited statements cannot qualify as a *logical* argument.
- 3 Previously proven mathematical theorems are the major source of the formal proofs used in economics.
- 4 These logical conditions are not independent of the axioms of logic. Each condition presumes that the statements of the argument are admissible. For example, each condition presumes that if a statement is not true it must be false.
- 5 I say 'heuristically' because otherwise it is quite incorrect to consider 'truth' to be something that can be passed around. Properly speaking, 'truth' is a property of statements only; that is, there is no 'truth' without a statement that is true. And the verb 'to pass' suggests the passage of time as well as the involvement of direction, but the intention is to avoid the time aspects. The verb 'to connect' preserves the timelessness, but it does not suggest direction.
- 6 But usually when there are many assumptions, one does not know which assumption 'caused' the false conclusion.
- 7 To illustrate, since this may seem counterintuitive to someone unfamiliar with formal logic, let us consider a simple example of an argument, the statements of which individually do not violate the axioms of logic. Let the assumptions be:
  - A<sub>1</sub>: 'All males have negatively sloped demand curves.'
  - A<sub>2</sub>: 'Only males have negatively sloped demand curves.'
  - A<sub>3</sub>: 'All my demand curves are negatively sloped.'

And let the conclusion that would follow as a matter of logic alone be:

C: 'I am a male.'

Now let us say we do not know whether the assumptions are true or false. But let us say we know that the conclusion is true. Does knowing that the conclusion of a logical argument is true enable us to say that we also know that any of the assumptions are true? Unfortunately not. As the above illustrative argument demonstrates, even if the conclusion is true all the assumptions can be false! In other words, although one's argument is logical, one still cannot use its logicity to assert that the assumptions are true on the basis of a known true conclusion. Note also that this example shows that the falsity of any assumption is not necessarily 'passed' on to the individual conclusions.

- 8 This is even more important if we distinguish between the two different purposes for building arguments. A disjunctive argument might be used by pure politicians who wish to convince us to vote for them or their policies. A conjunctive argument might be the objective of pure theorists who offer their arguments

as tests of their understanding of the world or the economy. If the theorists' understanding of the world is correct, they should be able to explain or predict certain relevant phenomena; the assumptions used will represent their understanding (for example, the so-called 'laws' of economics, physics, etc.). If a prediction turns out wrong, with the use of *modus tollens* one can say there is something wrong with their understanding of the world. Pure politicians, contrarily, may not care *why* someone votes for them or their policies so long as the vote is in their favor. *Success* is the politicians' primary objective.

- 9 Few economists today are serious inductivists; yet most follow Friedman's lead by stressing the importance of distinguishing between normative and positive statements. It might be argued that for some economists the use of this distinction is merely an unexamined inductivist ritual.
- 10 Truth substitutes, such as probabilities, will not do. Stochastic models, in which the assumptions are in the form of probability distribution statements, usually cannot provide the logical force of either *modus ponens* or *modus tollens*. This point was stressed by early econometricians, but is usually ignored in most econometrics textbooks [see Boland 1989, Chapter 7].
- 11 This was seen above as the limitation of *reverse modus ponens* in the illustrative argument about males and negatively sloped demand curves.
- 12 Which would easily be solved if we only had an inductive logic.
- 13 Note here, although Friedman uses conventionalist criteria, it is for a different purpose. For a conventionalist the criteria are used as truth status substitutes; in conventionalism one finds that theories are either better or worse. In this sense, Friedman can be seen to pose the problem of choosing among theories already classified as 'better' in his sense (successful predictions).
- 14 I stress, this is the view Friedman used in *his essay*. In correspondence Professor Friedman has indicated to me his more general views of testing in which success might be either a confirmation or a disconfirmation. But he still would question the meaningfulness of 'testing in order to reject'.  
Although Friedman seldom used the word 'truth', it should be noted that throughout he consistently uses the word 'validity' (by which he always means at least 'not inconsistent with the available facts') in the same sense that 'truth' plays in *modus ponens* seemingly while also recognizing that *modus ponens* is assured only when applied to 'truth' in the absolute or universal sense (i.e. without exceptions). Technically speaking his use of the word 'validity' may lead one to the incorrect identification of 'truth' with 'logical validity'. In this regard, applications of Friedman's methodology are often confused with orthodox conventionalism. This confusion can be avoided by remembering that 'validity' is a necessary (but not sufficient) condition of empirical 'truth' – hence, validity and truth are not identical – and by recognizing that we can believe our theory is true, even though we know we cannot prove that it is true.
- 15 For example, the assumption of a negatively sloped demand curve may be an assumption for the market determination of price, but it is the conclusion of the theory of the consumer.
- 16 That is, Friedman might argue that 'Occam's Razor' need not be used, as it is a pure intellectual exercise which serves no useful purpose.
- 17 Specifically, with an argument consisting of a conjunction of many interdependent assumptions, a false conclusion does not necessarily implicate any particular assumption but only the conjunction of all of them.
- 18 Nor does he say 'solely'.
- 19 Nagel's paper [1963] is often alleged to be a criticism of Friedman's essay. But Nagel's paper only tries to show that some of Friedman's definitions may not be universally accepted. Furthermore, a close reading will show that Nagel explicitly agrees with Friedman's methodological position. It is for this latter reason that Samuelson responds to Nagel by offering a criticism of Friedman's position. Also, it might be noted that Stanley Wong's paper [1973] is likewise not very critical of Friedman's methodology, although Wong, like Nagel, does note that Friedman's methodology is an example of instrumentalism.
- 20 Such an argument would at least involve a violation of the axiom of identity.

- 21 Statements of the form 'there will be a revolution' can never be proven false *even if they are false*. And tautological statements are true by virtue of their logical form alone, hence they cannot be refuted simply because one cannot conceive how they might be false.
- 22 For example, if one were to argue that revolutions are never successful, and one supported this with the evidence that every revolution has failed, the revolutionary might respond by saying that those were not 'genuine' revolutions.

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