WHAT IS REALLY WRONG WITH MILTON FRIEDMAN'S METHODOLOGY OF ECONOMICS

STEVEN RAPPAPORT
DeAnza College

INTRODUCTION

Since its appearance in 1953 a rather extensive literature has arisen in response to Milton Friedman's article "The Methodology of Positive Economics." However, to date no consensus has emerged as to what Friedman's methodological views are. And, partly as a result of this, there is little agreement on the merits and defects of Friedman's position. My purpose here is twofold. First, I want to offer an interpretation of Friedman's methodology which, in several important respects, is different than any so far advanced. Secondly, though my sympathies are largely with Friedman's critics rather than his defenders, Friedman's position seldom receives careful, precise statement by the critics. And too often the criticism focuses on minor, peripheral issues. I hope to bring out more adequately than has been done hitherto what is really wrong with Friedman's views on the methodology of economics.

One might legitimately wonder what the point is of yet another contribution to the inconclusive literature generated by Friedman's 1953 article. The answer lies in the fact that Friedman's article, as well as the literature responding to it, attempts to deal with an issue

Reason Papers No. 11 (Spring 1986) 33-62.

of the first importance for neoclassical microeconomics, which remains today the dominant approach to microeconomics in a variety of countries, including the United States. The essence of the neoclassical approach is the assumption that agents of interest to economists—households, business firms, government bureaus, and so on—are optimizers. That is, they maximize or minimize something—utility, profit, the bureau's budget, etc.—perhaps subject to constraints. The neoclassical approach manifests itself in virtually all the *specific* theories or models regularly presented in textbook treatments of microeconomic theory. These models include among their assumptions or axioms that economic agents of some type—firms in a perfectly competitive product market, a firm that is a monopsony buyer of labor, and so on—are optimizers. However, going as far back as Thorstein Veblen, a number of economists and noneconomists have criticized the neoclassical models on the basis that their assumptions, and especially the assumptions that agents of various types are optimizers, are *unrealistic.* In his 1953 article Friedman attempted to parry once and for all this type of criticism of neoclassical models.[1] Given the state of the existing literature on Friedman's article, what justifies spending further effort on it is its rather novel attempt to lay to rest the persistent criticism of the neoclassical approach to microeconomics just described.

FRIEDMAN'S METHODOLOGY OF ECONOMICS

Friedman's paper, "The Methodology of Positive Economics," begins by citing an alleged threefold distinction between positive economics, normative economics, and the art of economics. Aside from some brief remarks about normative economics at the outset, Friedman's article is entirely concerned with positive economics. Viewing positive economics as a product rather than a process or activity, we can say that positive economics is supposed to contain only so-called descriptive statements and no value judgments. Among the formulations of positive economics two especially important types are hypotheses and theories. It is with these that Friedman is particularly concerned. He says:

> This paper is concerned with certain methodological problems that arise in constructing the "distinct positive science" Keynes called for—in particular, the problem of how to decide whether a suggested hypothesis or theory should be tentatively accepted as part of the "body of systemized knowledge concerning what is."[2]

Unfortunately, Friedman's use of the term "hypothesis" is ambiguous. Sometimes he uses it to refer to a single general statement such as "A substantial increase in the quantity of money within a relatively short period is accompanied by a substantial rise in prices."[3]

But at other times he uses "hypothesis" to refer to a *theory*, i.e., a whole set of statements which can be organized into a deductive system.[4] In this paper I will use "hypothesis" to refer to a single statement, never an entire theory.

Before describing the criteria for the acceptability of economic theories and hypotheses Friedman sets out, it is necessary to discuss his conception of the process of *testing* a scientific theory or hypothesis. Friedman tells us the following about the process of testing:

> Empirical evidence is vital at two different, though closely related, stages: in constructing hypotheses and in testing their validity. Full and comprehensive evidence on the phenomena to be generalized or "explained" by a hypothesis, besides its obvious value in suggesting new hypotheses, is needed to assure that a hypothesis explains what it sets out to explain—that its implications for such phenomena are not contradicted in advance by experience that has already been observed. Given that the hypothesis is consistent with the evidence at hand, its further testing involves deducing from it new facts capable of being observed but not previously known and checking these deduced facts against additional empirical evidence.[5]

Let H represent an economic theory or hypothesis. The passage quoted suggests that testing H at time t is deducing from H one or more statements—call them "evidence statements"—about observable phenomena, and then determining the truth-values of these inferred statements.[6] In Friedman's view the truth-values of the inferred evidence statements are not known or justifiably believed prior to testing at time t. Alternatively, if one or more evidence statements are deduced from H at time t and their truth-values have not been ascertained at or before t, then Friedman counts these evidence statements as *predictions* of H.[7] And the evidence statements involved in a test of H must in Friedman's view be predictions of H. This seems clearly implied by the last sentence of the quoted passage.

Two further matters concerning Friedman's views of testing deserve comment. If H is tested at time t and the inferred evidence statements all turn out to be true, then the test of H at t is successful; but if one or more of the evidence statements turns out on investigation to be false, the test of H at time t is unsuccessful. In Friedman's view, if H has been tested one or more times at or before t and each test has been successful, then H is *confirmed* at time t by the body of evidence statements involved in the tests.[8] This is how Friedman uses the notion of confirmation. Note that to say that a hypothesis H is confirmed in this sense by a body of evidence statements E, is *not* to say E affords grounds or reason, though ones that are less than deductively conclusive, for thinking H is true. It is merely to say H is not *refuted* by E. Inductivists hold that the fact that a hypothesis has been tested (and always successfully) provides good but less-than-conclusive grounds for thinking the hypothesis is true. And they often express this by saying the hypothesis is confirmed by the evidence statements involved in the tests.[9] Friedman's use of "confirmed" should be kept distinct from this inductivist use. Friedman's notion of confirmation is much closer to Popper's notion or corroboration.[10]

Friedman thinks that no matter how many successful tests an economic theory or hypothesis H has had, H could still be false. That is, the fact that H is confirmed at any given time does not logically imply that H is true. Friedman commits himself to this when he says: "Observed facts are necessarily finite in number; possible hypotheses infinite. If there is one hypothesis that is consistant [sic] with the available evidence, there is always an infinite number that are."[11] In completely general terms, the claim Friedman is making here is that for any scientific theory or hypothesis T that is confirmed at a given time, the body of evidence statements confirming T is consistent with theories or hypotheses *other* than T, including ones incompatible with T. This of course is the widely accepted principle of the underdetermination of theory by evidence. It may also be expressed like this: for any theory T that is confirmed at any given time, the body of evidence statements confirming T does not logically imply T. Clearly Friedman's acceptance of the underdetermination principle commits him to saying the fact that an economic theory or hypothesis is confirmed does not logically imply that it is true.[12]

We can partially sum up the discussion of the last three paragraphs by saying Friedman's conception of testing is a variant of the hypothetico-deductive method of testing scientific hypotheses and theories. It is a variant I will call "simple hypothetico-deductivism." And the term "simple" is appropriate. For the view merely asserts that testing a hypothesis or theory consists in deducing one or more predictions from it, and then determining whether the predictions are true or false. The test is successful, the theory or hypothesis passes the test, if all the predictions turn out to be true; otherwise the theory fails the test. And this is all there is to testing a theory or hypothesis, whether in economics or any other nonformal science.

With Friedman's conception of testing in hand we can set out the chief epistemic rules for economics he presents.[13] Two of them are found in the following passage:

> As I shall argue at greater length below, the only relevant test of the *validity* of a hypothesis is comparison of its predictions with experience. The hypothesis is rejected if its predictions are contradicted ("frequently" or more often than predictions from an alternative hypothesis); it is accepted if its predictions are not contradicted; great

> confidence is attached to it if it has survived many opportunities for contradiction. Factual evidence can never "prove" a hypothesis; it can only fail to disprove it, which it what we generally mean when we say, somewhat inexactly, that the hypothesis has been "confirmed" by experience.[14]

Again let H be an economic theory or hypothesis. One epistemic rule for economics Friedman proposes in the quoted passage is this:

(R1) *H is acceptable at time t if H is confirmed at t.*

Recall that for Friedman to say H is confirmed is to say H has been tested one or more times (in the manner prescribed by simple hypothetico-deductivism), and all the tests have been successful. So, (R1) makes the fact that H has been tested, and always successfully, a *sufficient* condition for the acceptability of H. Another epistemic rule in the quoted passage is:

(R2) *H should be rejected at time t if (a) H has been tested on one or more occasions prior to t and on many of those occasions the test has been unsuccessful, or (b) at t the percentage of unsuccessful tests H has had is greater than the percentage of unsuccessful tests of some existing alternative to H.*

(R2) makes the satisfaction of condition (a) or (b) a sufficient condition for rejectability. But condition (a) of rule (R2) is vague or imprecise inasmuch as it speaks of *many* of the tests of H being unsuccessful. (The word Friedman actually uses in the quoted passage is "frequently"). Out of the total number of tests H has had, what specific number must be unsuccessful to enable us to say that many of H's tests have failed? Clearly there is no general answer. What is important about this imprecision of condition (a) of rule (R2) is that it has the result that (R2) does *not* make a *single* unsuccessful test sufficient for rejectability. Suppose at time t an economic theory H is well confirmed. Economists acting on rule (R1) accept H. Imagine that after time t, H is tested again but the test is unsuccessful. Rule (R2) does not require economists to now reject H. Should they continue to accept H, they will not violate (R2). For (R2) says many of H's tests must be unsuccessful for H to be worthy of rejection. And in the situation at hand most of H's tests have been successful; it is only one test that has failed. (For simplicity's sake I assume in the situation being envisaged that there is no alternative to H with a smaller percentage of unsuccessful tests.) In short, according to Friedman's methodology of economics, it is epistemically permissible for economists to continue to accept a theory or hypothesis in the face of a certain amount of adverse empirical evidence.

Since Friedman accepts the principle of the underdetermination of theory by evidence, he must allow that a situation can arise in which economists are confronted with two or more theories inconsistent with one another but equally confirmed. In such a situation Friedman's epistemic rule (R1) would obviously be powerless to enable economists to decide which of the theories to adopt. Friedman is aware of this and supplements rule (R1) with an additional epistemic rule to cover just the sort of situation we are envisaging. He says this: "The choice among alternative hypotheses equally consistent with the available evidence must to some extent be arbitrary, though there is general agreement that relevant criteria are suggested by the criteria 'simplicity' and 'fruitfulness,' themselves notions that defy completely objective specification."[15] Let H_1, $H_2, \ldots H_n$ be inconsistent or alternative hypotheses or theories. The passage quoted contains the following epistemic rule:

(R3) *If $H_1, H_2, \ldots H_n$ are equally confirmed at time t, then the simplest and most fruitful member of the group should be accepted at t.*

Some philosophers hold that in science simplicity is relevant *before* testing. Specifically, simplicity is to be appealed to in order to decide which of a number of competing hypotheses is to be subjected to empirical test.[16] This is not part of simplicity's role according to Friedman. On his rule (R3) simplicity is to be used along with fruitfulness to decide between competing theories that have already been tested and withstood the test. Friedman makes some remarks about the concepts of simplicity and fruitfulness used in (R3). What he says is very brief and sketchy and does not usefully contribute to the analysis of simplicity that philosophers of science have sought.[17] Economists who adopted (R3) would in a large measure have to rely on their intuitive or preanalytic understanding of simplicity and fruitfulness in acting on (R3) in particular situations.

So far nothing has been said about the aspect of Friedman's position in "The Methodology of Positive Economics" which usually receives the most attention. It appears in the following passage:

> The difficulty in the social sciences of getting new evidence for this class of phenomena and of judging its conformity with the implications of the hypothesis makes it tempting to suppose that other, more readily available, evidence is equally relevant to the validity of the hypothesis—to suppose that hypotheses have not only "implications" but "assumptions" and that the conformity of these "assumptions" to "reality" is a test of the validity of the hypothesis *different from* or *additional to* the test by implications. This widely held view is fundamentally wrong and productive of much mischief.[18]

In this passage Friedman considers the following pair of claims:

(01) *A hypothesis or theory in economics is acceptable only if its assumptions are realistic.*

(02) *The realism of the assumptions of an economic hypothesis or theory H is distinct from the truth of its predictions, i.e. the realism of the assumptions of H can be determined independently of ascertaining the truth-value of H's predictions.*

Friedman regards these two claims as mistaken and productive of much mischief—those who accept (01) and (02) constitute his opposition in "The Methodology of Positive Economics." Friedman makes a considerable effort to show that (01) and (02) are mistaken, an effort to be examined later on. For now I want to clarify (01) and (02) after relating Friedman's rejection of these two claims to the epistemic rule (R1) that he accepts.

The concepts of assumptions and realism in (01) and (02) need explanation. But whatever exactly the meaning of these two concepts, we can say (01) and (02) conflict with Friedman's rule (R1). (02) implies that the fact that an economic theory H is confirmed, is compatible with the assumptions of H being unrealistic. For (02) says the truth of H's predictions is one thing and the realism of H's assumptions another. And on Friedman's simple hypothetico-deductivist view of testing and confirmation, H being confirmed just consists in its predictions so far having turned out to be true. Suppose then H is confirmed but its assumptions are unrealistic. By epistemic rule (01) H is unacceptable, but by Friedman's rule (R1) H is acceptable. In sum, Friedman's acceptance of (R1), together with his simple hypothetico-deductivism, commits him to rejecting the conjunction of (01) and (02).

We need to clarify (01) and (02) in order to get a better idea of what Friedman takes his opposition to assert. It is convenient to first focus on the notion of assumptions used in (01) and (02). Friedman believes that one important kind of hypothesis found in economics is what I will call "as-if hypotheses." An as-if hypothesis takes the following form: __________ as if __________.

Some economic as-if hypotheses Friedman himself cites are as follows:[19]

(1) Business firms behave as if the managers have as their goal maximizing profits and have the knowledge needed to reach this goal (i.e, know the relevant total revenue and total cost functions, know how to calculate marginal revenue and marginal cost, etc.).

(2) American cigarette firms did not behave during World War II as if they were perfectly competitive firms.

(3) In situations involving risk individuals choose as if they were seeking to maximize their expected utility.

All three of these statements count as as-if hypotheses. Now Friedman counts what comes after the term "as-if" in an as-if hypothesis as the assumptions of the hypothesis. This is confirmed by the following passage:

> This implies that the distance traveled by a falling body in any specific time is given by the formula $s = \frac{1}{2}gt^2$, where s is the distance traveled in feet and t is the time in seconds. The application of this formula to a compact ball dropped from the roof of a building is equivalent to saying that a ball so dropped behaves as if it were falling in a vacuum. Testing this hypothesis by its assumptions presumably means measuring the actual air pressure and deciding whether it is close enough to zero.[20]

Friedman not only applies the term "assumptions" in connection with as-if hypotheses taken singly; he also talks about the assumptions of theories. Consider this passage:

> In speaking of the "crucial assumptions" of a theory, we are, I believe, trying to state the key elements of the abstract model. There are many different ways of describing the model completely—many different sets of "postulates" which both imply and are implied by the model as a whole. These are all logically equivalent: what are regarded as axioms or postulates from one point of view can be regarded as theorems from another, and conversely.[21]

Recall that an economic theory or model is a set of statements capable of being arranged into one or more deductive systems. In the quoted passage Friedman identifies the axioms of a deductive systematization of the statements in a theory as assumptions of the theory. Friedman notes in the passage that the set of statements belonging to a theory admits of different deductive systematizations. And this makes the question of whether a statement belonging to a theory is an assumption or not relative to a particular systematization of the theory. An axiom and therefore an assumption on one systemization may be a theorem and so not an assumption on a different systematization of the theory.

We now know what the term "assumptions" covers as used in (01) and (02). Unfortunately it is less clear how the notion of realism is used in these two claims. The fact is that "realistic" and its cognates are used in several different ways by Friedman. Consider the following passage:

> Euclidean geometry is an abstract model, logically complete and consistent. Its entities are precisely defined—a line is not a geometrical

> figure "much" longer than it is wide or deep; it is a figure whose width and depth are zero. It is obviously "unrealistic." There are no such things in "reality" as Euclidean points or lines or surfaces.[22]

This passage suggests the following:[23]

(D1) *A statement is unrealistic if and only if it contains one or more ideal object terms; it is realistic if and only if no such terms are used in the statement.*

But consider now this passage:

> 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). The reason is simple. A hypothesis is important if it "explains" much by little, that is, if it abstracts the common and crucial elements from the mass of complex and detailed circumstances surrounding the phenomena to be explained and permits valid predictions on the basis of them alone. To be important, therefore a hypothesis must be descriptively false in its assumptions; it takes account of, and accounts for, none of the many other attendant circumstances, since its very success shows them to be irrelevant for the phenomena to be explained.[24]

This passage suggests something like the following account of "realistic" and its antonym "unrealistic"—not the term "descriptively false" gets equated with "unrealistic":

(D2) *A statement is unrealistic (descriptively false) if and only if it does not afford a complete or exhaustive description of whatever it is about; a statement is realistic if and only if it does provide such a description.*

(D1) and (D2) are not at all equivalent. The statement "Reno is a city in Nevada" is unrealistic in the sense of (D2). The statement omits mention of the population of Reno and numerous other features of the city. But the statement is realistic in the sense of (D1), for it contains no ideal object terms. There is still a third meaning of "realistic" and "unrealistic" in Friedman's article. Section III of the article is entitled "Can a Hypothesis Be Tested By the Realism of Its Assumptions?" In the opening paragraph of section III Friedman says: "The application of this formula to a compact ball dropped from the roof of a building is equivalent to saying that a ball so dropped behaves as if it were falling in a vacuum. Testing this hypothesis by its assumptions presumably means measuring the air pressure and deciding whether it is close enough to zero."[25] The term "the realism of" does not appear just before "its assumptions" in the second sentence of this passage. But given the title of section III, we could insert the term without altering the meaning of the sentence. So, the passage quoted in effect says that determining the realism of the assumption "it (the ball) is falling in a vacuum" consists in finding out whether the air pressure is close to zero, that is, in finding whether the assumption is true or approximately true. The following account of the realism of a statement is suggested by all this:

(D3) *A statement is unrealistic if and only if it is neither true nor approximately true; it is realistic if and only if it is true or approximately true.*

It should be clear that (D3) does not determine the same concept of realism as does (D1) or (D2).

Which of the three uses of the concept of realism described above is employed in (01) and (02)? Friedman wishes to deny (01) and (02) in all three uses of the concept of realism. But only if the notion of realism in (01) and (02) is interpreted in the light of (D3) is Friedman's denial of the two claims of any interest. The first passage quoted in the preceding paragraph, together with the context from which it is drawn, indicates that Friedman believes (01) is false if the term "unrealistic" occurring in it is used in the sense of (D1). But this belief of Friedman's is not all controversial. It is generally admitted that scientific theories which contain ideal object terms may be acceptable. And (01) interpreted in light of (D1) quite unreasonably requires that economic theories lack such terms if they are to be acceptable. The second of the passages quoted in the previous paragraph shows that Friedman wishes to deny (01) if "unrealistic" is interpreted in the sense of (D2). But this too is hardly an interesting move on Friedman's part. (01) is patently false if it affirms that assumptions of hypotheses and theories in economics must afford an exhaustive description of what they are about in order to be acceptable. In section III of "The Methodology of Positive Economics" Friedman argues against (01) and (02). And his line of argument is directed at these two claims when the concept of realism is used in the sense of (D3). So Friedman wishes to reject (01) and (02) when the concept of realism in the two claims is interpreted in the sense of (D3). In this meaning of the notion of realism it is by no means obvious that Friedman is right in denying (01) and (02). Indeed, some of those who have discussed Friedman's methodological views have identified as his major error the denial of (01) when "realism" is taken in the sense of (D3).[26] It is when the concept of realism is used in the sense of (D3) in claims (01) and (02) that Friedman's denial of these claims is interesting and controversial. Accordingly I propose to focus on Friedman's rejection of (01) and (02) when "realistic" in (01) and "realism" in (02) mean "true or approximately true" and "truth or approximate truth" respectively.

It is worthwhile indicating how Friedman's methodological views as I have interpreted them enable him to answer the charge that neoclassical microeconomic models incorporate unrealistic assumptions. But before doing so it might be useful to summarize the main features of Friedman's position. The chief epistemic rules of Friedman's methodology of economics are (R1), (R2), and (R3). Each of these rules presupposes simple hypothetico-deductivism. The notions of test and confirmation are used in the formulation of the three rules, and these two concepts derive their sense from their relation to Friedman's variant of the hypothetico-deductive method. In addition, Friedman rejects the methodological position represented by (01) and (02). That is, Friedman denies that it is necessary for the acceptability of an economic theory or hypothesis that it have true or approximately true assumptions; moreover, he affirms that the *only* way to determine the truth or approximate truth of the assumptions of a theory or hypothesis is by ascertaining the truth-value of its predictions.

As indicated at the outset of this article, one of Friedman's main motives in his "The Methodology of Positive Economics" is to rebut the criticism that neoclassical microeconomics is unrealistic. Let us single out a particular example of this type of criticism of neoclassical theory, an example Friedman himself discusses. One important branch of neoclassical microeconimics is concerned with the behavior of business firms in hiring factors of production and the pricing of those factors. This branch of microeconomics is so-called marginal productivity theory or for short MPT. In 1946 Richard Lester published a paper criticizing MPT. Specific models in MPT—such as the model of the hiring policy of a firm in a competitive labor market—characteristically include as an assumption that a firm hires a quantity of a factor such as labor that maximizes firm profits. Lester attempted to challenge this, as well as certain other asserted statements of MPT, appealing to the results of a questionnaire he sent to 58 firms in the southern part of the United States. The managers of the firms responded by saying, among other things, that profits were not particularly important in their decisions about the quantity of labor they hired.[27] The implication of course is that the profit-maximizing assumption of specific models in MPT is unrealistic. Friedman's response to this criticism is that the assumption of profit maximizing does *not* imply anything about what firm managers *say* about their goals or other considerations entering into their hiring decisions.[28] In other words, Friedman claims that to say (1) firm managers will respond to Lester-type questionnaires by saying that their firms hire a quantity of labor that maximizes firm profits, is *not* a *prediction* of the specific models in MPT.[29]

How does this rebut the charge that the profit-maximizing assumption of MPT is unrealistic? Recall that on my interpretation Friedman holds that the realism of the assumptions of a theory cannot be determined except by ascertaining the truth-value of predictions of the theory. But this is just what Lester is trying to do. He is claiming that the profit-maximizing assumption of MPT is unrealistic on the basis that (1) above turned out to be false (as indicated by his questionnaire), when (1) is not a prediction of MPT at all. The way I have represented Friedman as answering Lester's criticism of MPT exemplifies the general pattern of Friedman's responses to charges that this or that assumption of neoclassical theory is unrealistic. Charges of this type are typically backed up by claiming some statement S other than the neoclassical assumption being challenged but allegedly bearing on the truth-value of the assumption, does not fit the observable phenomena. Friedman responds by saying statement S is not a prediction or implication of neoclassical theory at all. Given his view that realism of assumptions of a theory can only be determined by ascertaining the truth-value of predictions of the theory, the charge of lack of realism of the assumption in question collapses.

Friedman and Instrumentalism

A persistent theme in the literature on Friedman's methodological views is that he is an instrumentalist. I want to discuss three instrumentalist interpretations of Friedman. Two of them seem to me to be incorrect accounts of Friedman's position. And the other taken on its own presents a rather incomplete picture of Friedman's methodological views.

Those who regard Friedman as an instrumentalist do not attach the same meaning to "instrumentalist." Stanley Wong takes instrumentalism to be the view that scientific theories are not true or false descriptions of the real world, but just instruments for generating predictions about observable phenomena. Wong ascribes instrumentalism in this sense to Friedman. Wong says this:

> *Instrumentalism* is the thesis that theory in science is merely an instrument for prediction of observable reality. Accordingly, a theory cannot properly be called true or false.
>
> That Friedman is an instrumentalist is quite evident. The apparent ambiguities and inconsistencies in his essay can best be sorted out by considering his view as instrumentalism.[30]

Wong is mistaken in attributing to Friedman instrumentalism in his sense. Wong does not direct our attention to a single passage in Friedman's writings in which he says or implies that economic theories and hypotheses lack a truth-value. To be sure, Friedman does say that the goal or aim of the construction of theories and hypotheses in nonformal sciences like economics is the generation of true predictions.[31] But this claim does not logically imply that theories and hypotheses in nonformal sciences are neither true or false. There is no inconsistency in identifying prediction as the goal of theory construction and allowing that theories are true or false. Finally, Friedman often talks in a manner strongly suggesting that he takes economic hypotheses and theories to have a truth-value. For instance, he speaks about the confidence we may place in existing theories and hypotheses in economics.[32] And there is no reason to think that by "confidence we may place in" anything else is meant than "confidence we may place in the *truth of*."

In a fairly recent paper Lawrence Boland interprets Friedman as an instrumentalist. He says:

> "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.
>
> 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 Friedman, an instrumentalist, hypotheses are chosen because they are successful in yielding true predictions.[33]

The instrumentalism Boland attributes to Friedman in these passages is different than Wong's instrumentalism. Unlike Wong's, Boland's instrumentalism allows that theories may be true. Boland's instrumentalism seems to consist of the following claims:

(a) The sole purpose of having theories and hypotheses in economics (or any nonformal science) is the generation of true predictions;

(b) The truth-value of a theory or hypothesis (and any components like assumptions) does not matter for the question of whether the theory or hypothesis generates true predictions;

(c) A theory or hypothesis should be chosen or accepted if all its predictions have so far turned out to be true.

Friedman does say the goal of theory construction in economics is to generate true predictions. So I readily grant part (a) of Boland's instrumentalism is attributable to Friedman. Part (b) apparently means that a false theory can generate the same true predictions as a true theory can.[34] This would seem to be guaranteed by the principle of the underdetermination of theory by evidence which, as I have indicated in this paper, is accepted by Friedman. Let T_1 be a *true* theory confirmed at time t_1. By the underdetermination principle there is a theory T_2 compatible with T_1 and therefore false, but consistent with the same empirical evidence that confirms T_1. Thus at any time t_0 *prior* to t_1, theory T_2 could have been used to generate any of the true predictions yielded by T_1 between t_0 and t_1. As for part (c) of Boland's instrumentalism, it obviously resembles epistemic rule (R1) which I have attributed to Friedman. In sum, I have little quarrel with Boland's ascription of his instrumentalism to Friedman.

However, it is worth briefly comparing Boland's instrumentalism with the position attributed to Friedman earlier in this article. The two differ in significant respects. Epistemic rules (R2) and (R3) are part of the methodology of economics I ascribed to Friedman, but neither is included in Boland's instrumentalism. Elsewhere Boland does acknowledge that Friedman advocates appealing to simplicity and fruitfulness to decide between competing theories equally compatible with available evidence, though apparently Boland nowhere attributes rule (R2) to Friedman.[35] Also, the simple hypothetico-deductivist view of testing scientific theories is not a separately identifiable element of Boland's instrumentalism, but it is a key part of the position ascribed to Friedman in this paper. Finally Boland's instrumentalism does not clearly and explicitly include the denial of (01) and the denial of (02), (01) and (02) being the pair of claims affirmed by Friedman's self-chosen opponents in his 1953 article. In sum, Boland's instrumentalism is a rather incomplete account of Friedman's methodological views.

The last instrumentalist interpretation of Friedman that I wish to consider is Daniel Hausman's. Hausman says this:

> Milton Friedman, in contrast to the above two defenders of microeconomics, concedes that microeconomic general statements are false, or inapplicable because they contain antecedents that are not true of any real economic situation, at least that is how I understand his view of them as "unrealistic assumptions." He denies that their falsity matters. If the theory is well-confirmed (is a good "predictor") in the class of cases in which economists are interested, it is a good theory; otherwise not. Even assertions as abruptly counterfactual as the attribution of consciousness to tree leaves are perfectly acceptable in theories of leaf distribution. All that matters is how successfully leaf distribution is "predicted."
>
> Friedman's position seems to be a special sort of instrumentalism—which must be distinguished from the kind, discussed above, that Machlup has on occasion espoused. Friedman does *not* deny that theoretical statements have truth-values. In fact the distinction be-

> tween theoretical and observational terms is of no importance to Friedman. What he denies is that the truth values of any statements matter if the statements do not result in incorrect predictions concerning the phenomena of interest to us.[36]

The instrumentalism Hausman here ascribes to Friedman would seem to consist of the following claims:

(d) A theory or hypothesis in economics is acceptable or good if and only if it generates true predictions which are of interest to economists;

(e) Predictions generated by a theory or hypothesis but of no interest to economists are irrelevant to its appraisal.

There is similarity between (d) and (e) and rules (R1) and (R2) which I have attributed to Friedman. Let H be an economic theory well confirmed at a given time. Later, one prediction of H turns out to be false. But imagine this prediction is of no interest to economists—perhaps the prediction concerns what firm managers say about their goals in deciding on a level of output for the firm and economists are only interested in the nonverbal behavior of firm managers. In this situation Hausman's (d) and (e) have the result that H continues to be a good theory or acceptable despite the fact that it has generated a false prediction. A similar claim can be made for epistemic rules (R1) and (R2). As indicated in the previous section, in the type of situation being envisaged here (R1) and (R2) make it epistemically permissible for economists to continue to accept H.

However, Hausman's (d) and (e) do not represent Friedman's views in an entirely accurate way. There is little or no textual evidence for saying Friedman relies on a distinction between predictions of interest to economists and predictions of no economic interest.[37] Certainly in rebutting charges that neoclassical microeconomics incorporates this or that unrealistic assumption, Friedman does *not* allow that neoclassical theory generates *false* predictions but claims that these predictions are of no interest to economists. As indicated earlier, Friedman counters the charge of lack of realism by saying the false statements allegedly showing this or that neoclassical assumption is unrealistic are *not* predictions or implications of neoclassical theory at all, and therefore are irrelevant to the issue of the realism of its assumptions.

The Defects of Friedman's Methodology of Economics

The core of Friedman's methodology is his epistemic rules (R1), (R2), and (R3). However, Friedman is not much concerned with

(R3). After the rule is stated in "The Methodology of Positive Economics" it pretty much drops out of the picture. He makes no effort to argue for its adoption by economists.[38] But Friedman does argue for the adoption of epistemic rules (R1) and (R2), albeit in an indirect fashion. I will examine Friedman's case for these two rules. Doing so will prove a convenient way to bring out one of the chief defects in Friedman's position.

Friedman argues strenuously for the incorrectness of (01) and (02), the twin claims of his opponents on methodological matters. And Friedman apparently thinks that the only alternative to (01) and (02) is acceptance of his own rules (R1) and (R2). In support of attributing this belief to Friedman, consider the following passage: "As I shall argue at greater length below, the only relevant test of the *validity* of a hypothesis is comparison of its predictions with experience. The hypothesis is rejected if its predictions are contradicted ("frequently" or more often than predictions from an alternative hypothesis); it is accepted if its predictions are not contradicted. . . ."[39] The reader will recognize the second sentence of this passage as Friedman's formulation of his epistemic rules (R1) and (R2). The first sentence promises to argue later on and at length for these two rules. It would appear that Friedman fulfills this promise in section III of "The Methodology of Positive Economics." In fact what we find in section III is not any direct argument for (R1) and (R2), but instead a case against (01) and (02). All this suggests that Friedman thinks that he can secure our assent to (R1) and (R2) by disposing of (01) and (02). For ease of reference later on it will be convenient to state Friedman's argument for (R1) and (R2) in the following fashion:

(P1) *Either (R1) and (R2) are correct, or (01) and (02) are correct.*

(P2) *It is false that (01) and (02) are correct.*
Therefore (C) rules (R1) and (R2) are correct.

Premise (P1) expresses Friedman's belief that his own position and that of his opponents who adopt (01) and (02) exhaust the alternatives worthy of serious consideration. Admittedly premise (P1) is not an explicit premise of the line of argument in "The Methodology of Positive Economics." But Friedman must tacitly rely on some such premise as (P1). Premise (P2) alone does not logically imply the above conclusion (C). But (P2) conjoined with (P1) does validly yield (C).

Though Friedman argues energetically for premise (P2) of the above argument for his rules (R1) and (R2), he offers no reason whatsoever for thinking premise (P1) is true. As was said earlier, the conjunction of (01) and (02) is indeed incompatible with Friedman's rule (R1), and therefore incompatible with the conjunction of (R1) and (R2). But this does not mean premise (1) of Friedman's argument is true. It only means we cannot accept the conjunction of (01) and (02) and *also* regard (R1) and (R2) as correct. Or alternatively, the disjuncts of premise (P1) are merely contraries, they could both be false. Thus there is nothing in the logical relations of the disjuncts of premise (P1) which justifies saying (P1) is true.

The preceding paragraph indicated that we need not accept premise (P1) of Friedman's case for (R1) and (R2). There is no epistemic obligation to accept (P1). But a stronger conclusion than this is in order. It would be unreasonable to accept premise (P1). Let us suppose (01) and (02) are incorrect or unacceptable. Still we should not accept Friedman's rules (R1) and (R2). The reason for this is as follows. Recall that the simple hypothetico-deductivist view of testing scientific theories—for short, the simple H-D view of testing—is presupposed by Friedman's epistemic rules (R1) and (R2). Rule (R1) affirms that an economic theory or hypothesis H is acceptable if it has passed one or more tests as described by the simple H-D view and failed none. Rule (R2) asserts that H is unacceptable if H has often failed tests of the sort the simple H-D view describes. But as will be argued in the next three paragraphs, the simple H-D view of testing theories and hypotheses—whether in economics or any other nonformal science—is seriously flawed. Thus, even if we reject claims (01) and (02) as incorrect, it would still be unreasonable to accept Friedman's rules (R1) and (R2). Rejecting (01) and (02) does not negate the fact that (R1) and (R2) presuppose the erroneous simple H-D view. In sum, premise (P1) of Friedman's case for his rules (R1) and (R2) is unacceptable. If, rejecting (01) and (02), it is still unreasonable to accept (R1) and (R2), then premise (P1) must itself be unreasonable.

In showing that the simple H-D view of testing is flawed, what I will say should be familiar to philosophers of science. But it is important to indicate to philosophically minded economists and other social scientists that the simple H-D view cannot be maintained. What is perhaps the chief difficulty with the simple H-D view is best conveyed by an example.[40] Consider the following pair of statements:

(1) *Changes in the price of a stock selling on the New York Stock Exchange (NYSE) are statistically independent of one another, i.e. there is zero correlation between a change in the price of a stock at time t and a change in the price of a stock at time t* + 1 (t + 1 could be t + 1 day or t + 1 week, etc.).

(2) *There are invisible and otherwise indetectable leprechauns present on the floor of the NYSE during trading.*

Statement (1) is a well-attested hypothesis in the study of financial markets often called "the random-walk hypothesis." Statement (2) has been invented to make a philosophical point. Now the following prediction can be deduced from the random-walk hypothesis and suitable auxiliary statements:

(3) *During March 1990 a change in the price of Procter and Gamble stock on any given day will be statistically independent of the change in its price one day later.*

Suppose in March 1990 we observe changes in the price of Procter and Gamble stock on successive days. And perhaps by plotting our observations on a scatter diagram, we discover that the prediction (3) is true. By the simple H-D view of testing, the random-walk hypothesis has had a successful test. But note that since statement (1) above implies the prediction (3), the conjunction "(1) and (2)" has also had a successful test according to the simple H-D view. The prediction (3) is deducible from the conjunction of (1) and (2), and the prediction has turned out to be true in the situation we are envisaging. This constitutes a successful test according to the simple H-D view.[41] However, something has surely gone awry here. Presumably we want to say that a successful test of a theory or hypothesis H positively affects or increases the credibility or worth of H, or at least it does so in the absence of any previous unsuccessful tests or disconfirmations of H.[42] But I do not think we wish to affirm that the credibility of the conjunction of statements (1) and (2) has increased should the implied prediction (3) turn out to be true. If we were to affirm this, we should have to say the existence of leprechauns is more credible after we discover (3) is true than it was before. For the existence of leprechauns is logically implied by the conjunction of (1) and (2), and presumably an increase in the credibility or reasonableness of a statement spells a rise in the credibility of its logical consequences. As the example involving statements (1), (2), and (3) indicates, a major difficulty with the simple H-D view is that it sunders the connection between successful testing and an increase in the worth or credibility of the theory or hypothesis tested. The simple H-D view counts hypotheses and theories as successfully tested whose credibility has not increased at all on account of the test.

The criticism of the simple H-D view in the previous paragraph should not be seen as an objection that can easily be met by some minor adjustment to the simple H-D view. In support of this I will discuss two minor adjustments to the simple H-D view which represent prima facie plausible attempts to avoid the criticism in the previous paragraph.[43] According to the simple H-D view, testing a theory or hypothesis H is deducing a prediction P from H and then determining the truth-value of P; the test is successful if P turns out to be true, and unsuccessful should P turn out to be false. Let us try to supplement Friedman's simple H-D view with the following condition:

(C1) *If a prediction P of a hypothesis or theory H turns out to be true, then the test of H is still not successful as long as H is a conjunction "A and B" such that P is a prediction of A itself, i.e., P can be generated from A without relying on B.*

Now if (C1) were tacked onto the simple H-D view, the resulting view of testing would not be open to the criticism presented in the preceding paragraph. For the random walk hypothesis, statement (1) of the preceding paragraph, generates the prediction I labeled (3) without relying on the leprechauns hypothesis (2). Thus, by condition (C1), discovering in March 1990 that (3) is indeed true would *not* constitute a successful test of the conjunction of statements (1) and (2). However, condition (C1) is not satisfactory. It is much too strong or restrictive, ruling out as successful tests which in fact are successful. Reasoning from Newton's theory and suitable auxiliary statements, the English astronomer Edmund Halley made the following prediction:[44]

(P1) *The great comet of 1682 will be visible from the Earth in December 1758.*

(P1) turned out to be true and Newton's theory received a successful test. The credibility of Newton's theory was significantly increased in the minds of the members of the European intellectual community of the day. Now (P1) was implied by the conjunction:

(4) Newton's theory, and comets are on occasion visible from the Earth from 1750 on.

The term "Newton's theory" in (4) abbreviates the set of statements comprising Newton's theory. (P1) turning out to be true in 1758 could rightly have been regarded as a successful or favorable test of statement (4). It is hard to see how this could be denied given that (P1) turning out to be true constituted a successful test of the left conjunct of (4) and logically implies the right conjunct. However, condition (C1) above would *exclude* the truth of (P1) from constituting a successful test of (4). For (4) is a conjunction whose left conjunct—to wit, Newton's theory—is capable of generating prediction (P1) *without* relying on the right conjunct of (4). Thus condition (C1) is erroneous. Adding it to Friedman's simple H-D view of testing yields an account of testing which illegitimately narrows the class of successful tests.

A second attempt to make a relatively minor adjustment to the simple H-D view would supplement it with the following condition:

(C2) *If a prediction P of a hypothesis or theory H turns out to be true and H is a conjunction "A and B", then the test is still not successful unless P is* relevant *to* both *A and B taken separately.*

It might be claimed that prediction (3) above is not relevant to, has no bearing on, the leprechaun hypothesis (2). Thus, by condition (C2), discovering the truth of prediction (3) in March 1990 does afford a successful test of the conjunction of the random-walk hypothesis and the leprechaun hypothesis. However, adding (C2) to Friedman's simple H-D view would be of little use. The view of testing resulting from such an addition is subject to the same type of criticism I urged against the simple H-D view posited above. Let us conjoin the following statement with the leprechaun hypothesis (2):

(5) *If leprechauns are on the floor of a stock exchange during trading, then they arrange it so that changes in the price of a given stock are statistically independent of one another.*

The conjunction of (5) and the leprechauns hypothesis (2) implies the prediction (3) during March 1990 a change in the price of Procter & Gamble stock on a given day will be statistically independent of a change in its price a day later. Now consider the conjunction whose left conjunct is statement (1), i.e., the random-walk hypothesis, and whose right conjunct is the conjunction of (5) and (2). This statement, which can be written '(1) and [(5) and (2)]', also implies the prediction (3), as each of its two conjuncts separately implies (3). Now imagine that in March 1990 we discover that the prediction (3) is indeed true. According to the simple H-D view, '(1) and [(5) and (2)]' has had a successful test. For an implied prediction has been verified. Can we invoke condition (C2) above to *deny* that '(1) and [(5) and (2)]' has had a successful test? I do not see how. The prediction (3) would seem to be relevant to *both* conjuncts of '(1) and [(5) and (2)]'. The conjunction of (5) and (2) implies the prediction (3) just as (1) does. Thus, adding (C2) to the simple H-D view taken on its own, commits us to regarding certain hypotheses as successfully tested whose worth has *not* been raised by the test. I am certainly not prepared to say the credibility of '(1) and [(5) and (2)]' is increased by the prediction (3) turning out to be true in March 1990.

In the second paragraph of this section Friedman's own argument for his epistemic rules (R1) and (R2) was set out. We can now see that the argument breaks down because its premise (P1) is unacceptable. But the line of argument employed to show (P1) is wrong at the same time shows Friedman's rules (R1) and (R2) are themselves incorrect. Again (R1) and (R2) both presuppose Friedman's simple H-D view of testing scientific theories. And as indicated at some length above, the simple H-D view is erroneous. Moreover, the problems with the simple H-D view also spell trouble for Friedman's epistemic rule (R3). (R3) asserts that if a number of alternative or competing theories are equally confirmed, then the simplest and most fruitful should be chosen. I certainly have no quarrel with saying that simplicity and fruitfulness may be used to decide between competing theories when both have withstood testing equally well. Thus in part Friedman's rule is acceptable. But (R3) is formulated using Friedman's notion of confirmation which is defined in terms of the erroneous simple H-D view. To the extent (R3) presupposes the simple H-D view of testing it is objectionable. In sum, the three epistemic rules of Friedman's methodology of economics are all defective. And what makes them defective is that they presuppose the mistaken simple H-D view of testing. This is a major part of what is really wrong with Friedman's position in his 1953 article.

The remainder of this section will be devoted to examining Friedman's criticism of (01) and (02), the claims of his opponents on methodological matters. In rejecting these two claims Friedman commits himself to a view of the role of the realism of assumptions in appraising economic theories and hypotheses that has made him the object of a good deal of criticism. In my view Friedman is correct in regarding (01) as mistaken, though there is an important element of truth in (01) which Friedman overlooks. But Friedman is wrong to reject (02) as it is true.

By way of supporting this assessment of Friedman's attitude toward (01) and (02), let us examine Friedman's effort to dispose of (02). What Friedman does is argue, by means of an example drawn from the physical sciences, that *in general* the realism (truth or approximate truth) of assumptions of a theory or hypothesis can be determined *only* by ascertaining the correctness of its predictions. This is of course inconsistent with the claim (02) makes about the realism of the assumptions of *economic* theories and hypotheses. My statement of Friedman's argument in the next paragraph will make reference to the following piece of reasoning—call it "argument A".[45]

(1) *A compact ball dropped from the roof of a certain building will behave as if it is falling in a vacuum.*

(2) *The distance traveled by a body falling to the Earth in a vacuum is given by the formula* $s = 16t_2$, with s = the distance traveled in feet and t = the time in seconds.
(3) *The distance the ball will travel from the top of the building to the ground is 256 feet.*
Therefore, (4) the time the ball will take to travel from the top of the building to the ground is approximately four seconds.

Imagine statement (1) here is an as-if hypothesis the realism of whose assumptions we wish to investigate. The assumptions of the hypothesis are of course the statement "it (the ball) is falling in a vacuum." Statement (2) is the application of the law of freely falling bodies to the Earth. Statement (3) is verified by measuring the distance from the top of the building to the ground in the situation envisaged by argument A. Statement (4) is a prediction of the as-if hypothesis (1). This prediction follows by logic and mathematics from (1) in conjunction with the auxiliary statements (2) and (3).

Now those who accept (02) would no doubt be willing to say that the realism of the assumptions of statement (1) of argument A can be determined *apart from* ascertaining the truth-value of prediction (4). Friedman represents the proponents of (02) as attempting to determine the realism of (1)'s assumptions by measuring the air pressure in the situation envisaged by argument A, and seeing whether it is close to zero.[46] As Friedman notes, at sea level the air pressure is about 15 pounds per square inch. This means the assumptions of hypothesis (1) are not *exactly* true—the measurement would have to be zero for this to be the case. But the assumptions of statement (1) of argument A are still realistic as long as they are *approximately* true, or sufficiently close to the truth. But how is *this* to be determined? Friedman's answer is that the *only* way to do so is to ascertain whether *prediction* (4) of argument A is accurate or true.[47] We can find out whether or not the assumption "it (the ball) is falling in a vacuum" is approximately true only by seeing whether the ball takes about four seconds to fall from the top of the building to the ground. This is the time the fall would take were the ball to fall 256 feet in a vacuum. Friedman concludes that the realism of the assumptions of an as-if hypothesis like (1) of argument A is only determinable by ascertaining the accuracy of predictions of that hypothesis. The application of this conclusion to economics means that (02) is mistaken.

Prima facie Friedman's case against (02) may seem cogent. Friedman is surely right in suggesting that measuring the air pressure in the situation argument A is concerned with cannot determine whether the assumptions of statement (1) of the argument are sufficiently close to the truth. As Friedman notes, "it is falling in a vacuum" is close enough to the truth when "it" refers to a compact ball, but very far from the truth when it is a feather that is being dropped from the building.[48] Yet whether it is a ball or a feather that is being dropped, measurement of the air pressure in the situation would give the same 15 pounds per square inch figure. And if measurement of the air pressure is powerless to determine whether the assumptions of statement (1) are realistic, what else is there to do the job but accuracy of the predictions of (1) such as step (4) of argument A? Well, there is something else to do the job which Friedman overlooks.

We can make a distinction between two ways of determining whether or not a statement P is realistic (true or approximately true). One way to do so is by ascertaining the accuracy of predictions of P, or better, by subjecting P to empirical test and seeing whether the test is favorable or positive. Another way is by ascertaining the logical relations of P—inductive as well as deductive—to *other* statements which are *already* justifiably accepted. An example from economics will illustrate the distinction. In a paper published in 1940 and widely read by economists, Freidrich Lutz attempts to construct a theory or model about the market for bonds or debt obligations that would explain the different shapes yield curves can assume. (A yield curve shows the relationship between yield to maturity and term to maturity of different bonds at a single point in time.) Lutz's approach is to start out with a version of his theory that has unrealistic assumptions, and then, by relaxing the assumptions piecemeal, move in the direction of a model whose assumptions are realistic. The assumptions or axioms of the initial version of Lutz's model are as follows:[49]

(A1) *All participants in the bond market (i.e., lenders and borrowers) have accurate or correct expectations about future short-term interest rates.*

(A2) *There are no costs for either lending or borrowing in the bond market.*

(A3) *There is complete shiftability, i.e., neither any lender nor any borrower has a preference for debt obligations of one maturity rather than another.*

Let us focus on assumption (A1). (A1) is unrealistic. But to determine this it is *not* necessary to subject (A1) to empirical test and get an unfavorable or negative result, i.e., infer one or more predictions from (A1) and then find out that the predictions are inaccurate.[50] We *already* accept the following statement:

(5) *Participants in the bond market often turn out to be wrong in their expectations about the course of future short term interest rates; and indeed, bond market participants usually lack any specific expectations about short term rates beyond one or two years in the future.*

Statement (5) implies that (A1) is rather far from the truth. Thus, whether assumption (A1) of Lutz's model is realistic is determinable by ascertaining its logical relations to already accepted statements like (5). To be sure, (A1) logically implies the denial or negation of (5). But the negation of (5) is not a prediction of (A1), as it is not a statement whose truth-value is as yet undetermined by us. Thus discovering the unrealistic quality of (A1) by reference to its logical relations to already accepted statements like (5), is distinct from determining (A1)'s lack of realism by examining its predictions.

The rather commonsensical distinction drawn in the preceding paragraph applies to Friedman's argument against (02). Friedman is wrong in suggesting that the *only* way to determine the realism of the assumption of statement (1) of argument A is by checking the accuracy of predictions of (1). We can determine the realism of the assumptions of (1) by ascertaining the logical relations of those assumptions to already accepted statements. One pertinent statement we already accept is this:

(6) *When bodies such as compact balls and decent-size rocks fall to the Earth, the effect of air resistance is negligible.*

We can judge the assumptions of the as-if statement (1) of argument A to be realistic on the basis of (6). Given (6), we can say "it (the ball) is falling in a vacuum" is an approximation to the truth. We need *not* await what observation tells us about (1)'s assumptions.[51] From what has been said I conclude that Friedman's case against (02) described several paragraphs back is unsound. Moreover, the distinction drawn in the preceding paragraph clearly warrants saying (02) is *correct*, and thus Friedman is wrong to reject it. (02) affirms that the realism of the assumptions of an economic theory or hypothesis H is determinable apart from H's predictions. The distinction of the preceding paragraph applied to economics justifies saying this is true.[52]

Though Friedman is mistaken in rejecting (02), he is right to reject (01). Recall that (01) says that an economic theory or hypothesis is acceptable only if its assumptions are realistic. Friedman provides examples of as-if hypotheses outside of economics that he claims are acceptable despite the unrealistic quality of their assumptions. One such example is as follows:

(1) *Leaves are positioned around a tree as if each leaf deliberately seeks to maximize the amount of sunlight it gets, knows the physical laws determining the amount of sunlight it would get in the various positions on the tree, and is able to move quickly to any unoccupied position on the tree.*

Concerning this as-if hypothesis Friedman says the following:

> Is the hypothesis rendered unacceptable or invalid because, so far as we know, leaves do not "deliberate" or consciously "seek," have not been to school and learned the relevant laws of science or mathematics required to calculate the "optimum" position? Clearly, none of these contradictions of the hypothesis is vitally relevant; the phenomena involved are not within the "class" of phenomena the hypothesis is designed to explain"; the hypothesis does not assert that leaves do these things but only that their density is *as if* they did. Despite the apparent falsity of the "assumptions" of the hypothesis, it has great plausibility because of the conformity of its implications with observation.[53]

Here Friedman rightly says the assumptions of hypothesis (1)—what comes after the term "as if" in (1)—are false or unrealistic. But he claims (1) is acceptable or highly plausible. Now we can readily grant Friedman the general point he is trying to make with his example of (1), viz., as-if hypotheses can be accepted though their assumptions are unrealistic and are known to be so. The acceptability of an as-if hypothesis no more depends on the realism of what comes after "as if" than the acceptability of "if it is raining hard, then the streets are wet" depends on the truth or approximate truth of the antecedent "it is raining hard." Since the point holds for as-if hypotheses in general, it holds for such hypotheses in economics. Thus (01) is false as Friedman claims.

Despite (01)'s falsity, there is an element of truth in it which should not be overlooked. Recall that there are two types of items to which the term "assumptions" applies. What comes after the term "as if" in an as-if hypothesis are assumptions of the hypothesis, and the axioms of a deductive systematization of a theory count as assumptions of the theory. Now (01) concerns the acceptability not only of economic hypothesis, but theories or models as well. (01) says an economic theory is acceptable only if its assumptions are realistic. But surely this is reasonable, bearing in mind that we can only speak of the assumptions of a theory relative to some particular systematization of it. Suppose we discover that the axioms of a systematization of an economic theory are unrealistic, i.e., not even approximately true. In that case case the theory includes statements that are not even approximations to the truth. Surely it would not be epistemically permissible to accept such a theory. Friedman's case against (01) discussed in the preceding paragraph focuses exclusively on the assumptions of as-if hypotheses and ignores the assumptions of economic theories or models. But this leads Friedman to overlook the fact that what (01) says about the acceptability of theories in economics is correct.

Conclusions

I have tried to give a somewhat different picture of Friedman's position in "The Methodology of Positive Economics" than has been presented in the literature to date. On this interpretation, the methodology of economics Friedman recommends to his fellow economists consists of the epistemic rules (R1), (R2), and (R3). Moreover, Friedman rejects as mistaken the view of the role of the realism of assumptions in appraising economic theories and hypotheses represented by claims (01) and (02). My treatment of Friedman has, I believe, enabled me to bring out more adequately than has been done hitherto what is really wrong with Friedman's position in his 1953 article. The defect of Friedman's epistemic rules (R1), (R2), and (R3) is that they presuppose the mistaken simple hypothetico-deductivist view of testing scientific theories and hypotheses. As for the claims of Friedman's opponents on economic methodology, though Friedman is right in thinking (01) is false, he fails to see that what (01) says about economic *theories* is right. And finally, Friedman's rejection of (02) is vitiated by his failure to recognize that examining predictions of a theory or hypothesis is not the only way to determine the realism of its assumptions; statements already justifiably accepted also provide a way to do this. The fact that Friedman is wrong to reject (02) causes the collapse of his effort to save neoclassical microeconomic theory from the charge that it incorporates unrealistic assumptions. For, as I pointed out, Friedman's effort to rebut this charge relies on his view that (02) is wrong.

1. Milton Friedman mentions such criticisms. See Friedman, "The Methodology of Positive Economics," in Friedman, *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953), p. 15, pp. 30-31.
2. Friedman, "The Methodology of Positive Economics," p. 3.
3. Ibid., p. 11.
4. Ibid., p. 26. Actually Friedman's view of theories is a bit complex. In his writings he offers two characterizations of theories which he does not bother to relate to one another. Friedman regards a theory as the union of a set of so-called tautologies and a set of so-called substantive hypotheses. See ibid., p. 7. But he also regards a theory as a deductively systematizable set of statements containing one or more ideal-object terms such as "perfectly competitive market" or "mass point." See ibid., p. 34. In discussing Friedman's views I have chosen to use "theory" to simply mean a set of statements or sentences susceptible of deductive systematization. No serious distortion of Friedman's views will result from adopting this use of "theory." And doing so provides a clear sense of "theory" in which the term is in fact applicable to economic theories or (as economists are more likely to call them) models.
5. Friedman, "The Methodology of Positive Economics," pp. 12-13.
6. In talking of statements about observable phenomena it must not be thought I am foisting on Friedman the logical empiricist view that the language of a nonformal science like economics contains observation sentences. These are supposed to contain so-called observation terms plus logical and mathematical terms, but no theoretical terms. Nowhere does Friedman indicate that he accepts the observation term/theoretical term dichotomy built into the logical empiricist notion of an observation sentence. Indeed, at one point Friedman denies the independence of observation from theory presupposed by that dichotomy. See Friedman, "The Methodology of Positive Economcs," p. 34. However, I am attributing to Friedman the view that a nonformal science includes statements which describe only what the observable phenomena are like. For a discussion of this type of statement and the need to distinguish it from the observation sentences of the logical empiricists, see Bas Van Fraassen, *The Scientific Image* (London: Oxford University Press, 1980), pp. 13-19; 53-54.
7. Friedman, "The Methodology of Positive Economics," p. 9. Friedman emphasizes that in this use predictions are not necessarily about the future. They may be about situations in the past or present whose existence we have yet to determine.
8. Ibid.
9. See Carl Hempel, *Philosophy of Natural Science* (Englewood Cliffs, N.J.: Prentice Hall, 1966), p. 18.
10. See Karl Popper, *The Logic of Scientific Discovery* (New York and Evanston, Ill.: Harper & Row, 1968), chap. 10. The relation between Popper and Friedman is discussed by William Frazier and Lawrence Boland, "An Essay on the Foundations of Friedman's Methodology," *American Economic Review* 73 (1983): 129-144.

11. Friedman, "The Methodology of Positive Economics," p. 9.
12. It is generally agreed nowadays by philosophers that usually a scientific theory does not on its own imply any evidence statements; suitable auxiliary statements must be conjoined with the theory to generate predictions. This principle, which might be called "the principle of the underdetermination of evidence by theory," is not discussed by Friedman. But nothing he says about the empirical testing theories is incompatible with it.
13. Terms of epistemic appraisal are terms we use to appraise or evaluate people's acceptance or rejection of statements. Examples are "unreasonable," "justified," "more reasonable than," "acceptable," and "should not reject." An epistemic rule of a discipline is a sentence with this feature: it specifies a condition (necessary, sufficient, etc.) for applying some term of epistemic appraisal to the acceptance or rejection of statements or theories in the discipline. The methodology of a discipline can be regarded as a set of epistemic rules for the discipline.
14. Friedman, "The Methodology of Positive Economics," pp. 8-9.
15. Ibid., p. 10. Some commentators have overlooked Friedman's acknowledgement of the role of simplicity and fruitfulness in theory appraisal. For example, Alexander Rosenberg says Friedman holds theories are to be judged *solely* by their predictive success. See "Friedman's 'Methodology' for Economics: A Critical Examination," *Philosophy of the Social Sciences* 2 (1972): 17.
16. See Willard Quine, *Word and Object* (Cambridge, Mass.: Harvard University Press, 1960), p. 19.
17. Friedman, "The Methodology of Positive Economics," p. 10.
18. Ibid., p. 14.
19. The first two appear in Freidman, "The Methodology of Positive Economics," pp. 21-22 and p. 37 respectively. The third appears in Friedman and Leonard Savage, "The Expected Utility Hypothesis and the Measurability of Utility," *Journal of Political Economy* 60 (1952): p. 463.
20. Friedman, "The Methodology of Positive Economics," p. 15.
21. Ibid., p. 26.
22. Ibid., p. 25.
23. Roughly an absolute general term F is an ideal object term if F is part of the vocabulary of some discipline yet relative to accepted views there could not be anything in the real world denoted by F. Examples are "perfectly competitive market," "frictionless surface," and "Euclidean point."
24. Friedman, "The Methodology of Positive Economics," pp. 14-15.
25. Ibid., p. 16.
26. See Paul Samuelson, "Problems of Methodology—Discussion," *American Economic Review: Papers and Proceedings* 53 (1963): 233.
27. Richard Lester's "Shortcomings of Marginal Analysis of Wage-Employment Problems," *American Economic Review* 36 (1946):65. Lester's questionnaire indicated that actual and expected firm sales were the chief considerations managers took into account in deciding how much labor to hire.
28. Friedman, "The Methodology of Positive Economics," p. 31.
29. Friedman's as-if analysis of motivational hypotheses in economics is behind his claim that (1) is not a prediction of MPT. For Friedman the hypothesis that firms maximize profits is to be construed as affirming (a) firms behave as if the managers have as their goal maximizing profits and have the knowledge needed to reach this goal. Note that (a) does not imply anything about the goals or other psychological states of firm managers, any more than "Jack behaves as if he is insane" implies that Jack is insane. Nor does (a) imply or predict anything about what firm managers *say* about their goals or bases for making decisions.
30. See Stanley Wong, "The F-Twist and the Methodology of Paul Samuelson," *American Economic Review* 63 (1973): 314.
31. Friedman, "The Methodology of Positive Economics," p. 7.
32. Ibid., p. 41.
33. Lawrence Boland, "A Critique of Friedman's Critics," *Journal of Economic Literature* 17 (1979): 507, 508-509, 511.

34. This is supported by Boland, "Friedman's Methodology vs. Conventional Empiricism: A Reply to Rotwein," *Journal of Economic Literature* 18 (1980): 1556.
35. Boland mentions simplicity and fruitfulness in "A Critique of Friedman's Critics," pp. 511-512. He appears to ascribe to Friedman the epistemic rule (a) a theory generating one or more false predictions should be rejected or eliminated. See ibid., p. 511. But (a) is not the same as (R2) which does not make a single false prediction sufficient for rejectability.
36. Daniel Hausman, "Defending Microeconomic Theory," *The Philosophical Forum* 15 (1984): 394-395. Though I am critical of Hausman's views, private correspondence with him has been useful to me in thinking about Friedman's relation to instrumentalism.
37. In his article Hausman does not quote any passages from Friedman's writings. But in support of attributing (d) and (e) to Friedman he does cite pages 14 and 19-20 of "The Methodology of Positive Economics." I cannot detect in these pages the presence of any distinction between predictions generated by a theory that are of interest and predictions of no interest.
38. Friedman says it is generally agreed that simplicity and fruitfulness are relevant to choosing a unique theory from among alternatives that are equally confirmed by available evidence. See Friedman, "The Methodology of Positive Economics," p. 10. Given this Friedman presumably thinks arguing for (R3) would be otiose.
39. Friedman, "The Methodology of Positive Economics," pp. 8-9.
40. One account of the difficulty appears in Stephen Barker, *Induction and Hypothesis* (Ithaca, NY: Cornell University Press, 1967), pp. 155-156. Another version appears in Clark Glymour, *Theory and Evidence* (Princeton, NJ: Princeton University Press, 1980), pp. 29-31. The difficulty with the simple H-D view of testing and confirmation attaches to both the inductivist and noninductivist versions.
41. It might be said—though nowadays few philosophers would say it—that statement (2) is empirically meaningless, and so the conjunction of (1) and (2) is itself empirically meaningless. And, it might be added, since empirically meaningless sentences are not admissible for testing in the first place, the simple H-D view is not applicable to such sentences. But the problem with these remarks is that the alleged distinction between empirically meaningful and empirically meaningless sentences has never been successfully drawn. For a discussion of a variety of unsuccessful attempts to draw this supposed distinction, see Carl Hempel, "Empiricist Criteria of Cognitive Significance: Problems and Changes," in *Aspects of Scientific Explanation* (New York: Free Press, 1965), pp. 101-122.
42. This epistemic principle can be accepted by noninductivists like Friedman. It does not carry a commitment to saying there are any good arguments other than deductively valid ones. Specifically, the principle does not imply that the fact that a hypothesis has withstood testing affords good though less than conclusive grounds for thinking it is true. Though the principle conjoined with this fact logically implies that the hypothesis is more worthy of acceptance than before testing.
43. Both adjustments were proposed to me in conversation by Ted Watkins (Department of Economics, San Jose State University). The adjustments are minor in that their addition to the simple H-D view does not result in a version which is substantially more complex. A complicated version of the hypothetico-deductivist view of testing and confirmation is found in Gary Merrill, "Confirmation and Prediction," *Philosophy of Science* 46 (1979); 103-106. Glymour has argued that Merrill's version is unacceptable. See Glymour, "Hypothetico-Deductivism is Hopeless," *Philosophy of Science* 47 (1980): 322-325.
44. Halley's prediction and the test it afforded of Newton's theory is discussed in Ronald Giere, *Understanding Scientific Reasoning* (New York: Holt, Rinehart, Winston, 1979), pp. 85-88. Giere does not employ the example in the context of criticizing any variant of the hypothetic-deductive method.

45. Argument A does not appear in "The Methodology of Positive Economics." But several component statements of the argument do appear in Friedman's article. And argument A is a useful device in stating his case for thinking realism of assumptions is ascertainable only by examining predictions. Friedman's case is developed in "The Methodology of Positive Economics," pp. 16-18.
46. Friedman, "The Methodology of Positive Economics," p. 16.
47. Ibid., p. 16-17.
48. Ibid.
49. Friedrich Lutz, "The Structure of Interest Rates," *Quarterly Journal of Economics* 55 (1940): 36-37.
50. I assume that *in part* testing a theory or hypothesis is obtaining from it one or more predictions and determining the correctness of those predictions. This assumption is consistent with sophisticated or complex forms of hypothetico-deductivism, as well as the Bayesian approach defended by philosophers of science such as Wesley Salmon. In the present context no harm will result from ignoring aspects of testing other than inferring predictions from the hypothesis under test and checking the accuracy of those predictions.
51. It might be said that our basis for accepting statement (6) is that in actual situations resembling the one envisaged by argument A predictions of statements like step (1) of the argument have turned out to be true. That is, our basis for accepting (6) is that in cases we have experienced the actual behavior of falling bodies like compact balls closely fits the behavior predicted by the formula $s = 16t^2$. This may be, but it does not affect my point. The fact is that the realism of the assumptions of a hypothesis such as (1) of argument A is ascertainable without reference to the predictions of (1) itself. And it must be emphasized that Friedman's view is *not* that the realism of the assumptions of a hypothesis such as (1) of argument A is determinable by reference to a different and already accepted hypothesis such as (6) whose predictions have in the past turned out to be true. Such past predictive success of a different hypothesis is, in Friedman's view, irrelevant to determining the realism of the assumptions of the hypothesis at hand. Friedman's view is that the realism of the assumptions of a given hypothesis or theory are only ascertainable by reference to the predictions of that hypothesis or theory itself. This is the view Friedman commits himself to in rejecting and arguing against (02).
52. Among Friedman's numerous critics Peter McClelland is unusual in recognizing that Friedman is wrong to reject (02) because it overlooks the possibility of determining the realism of the assumptions of a hypothesis or theory by examining the logical relations of those assumptions to already accepted statements. See McClelland, *Causal Explanation and Model Building in History, Economics, and the New Economic History* (Ithaca, N.Y.: Cornell University Press, 1975), pp. 136-139. However, McClelland is not justified in saying Friedman inconsistently rejects (02) and also allows that the correctness of the assumptions of a theory can be assessed other than by examining its predictions. In the passage on page 26 of "The Methodology of Positive Economics" which McClelland cites to support this, Friedman is *not* discussing the assessment of the assumptions of a theory but the decision of which of the statements belonging to it to use as axioms or assumptions.
53. Friedman, "The Methodology of Positive Economics," p. 20. Friedman is speaking carelessly in the midsection of this passage when he says such statements as "leaves do not deliberate" are contradictions of hypothesis (1). For, as the latter half of the passage attests, Friedman does not think (1) implies that leaves do deliberate.