RHETORIC AT THE EXPENSE OF COHERENCE:

A REINTERPRETATION OF MILTON FRIEDMAN'S METHODOLOGY

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ABSTRACT

In this paper a new interpretation of Friedman's *The Methodology of Positive Economics* is suggested. It amounts to distinguishing three mutually incompatible tendencies in his essay. These tendencies are labeled "positivist," "pragmatist–conventionalist," and "realist," and they are documented by textual evidence. It is suggested that the inconsistencies are part of Friedman's methodological rhetoric and that only by extending the pragmatist attitude toward economic theories to one which would encompass economic methodologies could he succeed in neutralizing his methodological inconsistencies.

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I. INTRODUCTION

1. "More than other scientists, social scientists need to be self-conscious about their methodology" (Friedman, 1953, p. 40). There are good grounds for agreeing with Friedman on this claim. But, interestingly enough, Friedman himself does not seem to obey his own maxim: after a careful study of his "The Methodology of Positive Economics" I dare to argue that Friedman is not very self-conscious about *his* methodology. My contention in this paper is that Friedman's "Methodology" is not easy to see as a coherent whole, but that there can be found—after suitable interpretation and reconstruction—three more or less separate and partly inconsistent tendencies within his essay. His lack of self-consciousness amounts to the fact that he does not seem to be aware of their incompatible nature.

I will characterize and document each of these tendencies in turn. For convenience, they are labeled "positivist," "pragmatist" (or "pragmatist–conventionalist"), and "realist" tendencies. If carried to their full realization they would form three coherent methodologies. Each of them contains a view as to the cognitive status of economic theory and as to the criteria of theory appraisal in economics. *Positivism* ties economic theory closely to given empirical facts in both respects: a theory is about relations among given data, and it is tested in a rule-governed way by its predictive success in establishing those relations. *Pragmatism*, in its turn, does not admit the existence of any givens—economic theory is only about some subjectively constructed facts—and holds that acceptance of theories is dependent on their congruence with tradition, on the aims of theorizing, on scientists' decisions that are not governed by formal rules, etc. *Realism* is cognitively more ambitious: economic theories should represent some deeper realities, even though it is possible that theory appraisal does not rest on given facts according to strict rules.

As will be seen, it is difficult to provide clear and complete accounts of these views on the basis of Friedman's text. What is possible, however, is to sketch some ingredients of the three tendencies. This implies, among other things, that in using the labels "positivism," "pragmatism," and "realism" I do not want to commit myself to any specific versions of them. Friedman's text leaves open the question of which versions would result if the tendencies were carried to their full realization. This is one reason for using these shorter and more neutral labels: F1 for positivist reconstruction, F2 for pragmatism, and F3 for realism.

I will draw examples primarily from "The Methodology of Positive Economics." Thus, what follows is to be considered as a new interpretation of that widely misunderstood essay. Here and there I will also refer to other parts of the *Essays in Positive Economics* (1953).

2. It is well known that Friedman denies the necessity of testing the "realism" of the "assumptions" of an economic theory. He emphasizes instead that only the predictive implications of the theory should be tested if the theory is to justify itself. On the basis of several passages of his essay it seems fair to conclude that Friedman is ready to subscribe to the following thesis:

F: *The realism of assumptions is irrelevant, and predictive power is relevant to the acceptance of economic theory.*

One of the points of departure in my endeavor is a conviction that the import of F, Friedman's basic thesis, is far from unambiguous. Nor does it exhaust Friedman's methodological view. This is why my three reconstructions consist of adding to F a few supplementary theses, some of which carry with them specifications for elements of F—for instance, "theory," "acceptance," "realism," "relevant."

II. POSITIVIST TENDENCIES

1. Many of Friedman's commentators have interpreted him as claiming that *predictive power* is not only relevant but actually *the only and absolute criterion* in appraising the acceptability of an economic theory.[1] This modification of F—a shift from "relevant" to "crucial" or even something stronger—can be supported by some quotations from Friedman's text. He commits himself to "the fundamental methodological principle that a hypothesis can be tested only by the conformity of its implications or predictions with observable phenomena" (1953, p. 40) and that "its performance is to be judged by the precision, scope, and conformity with experience of the predictions it yields" (p. 4).

2. The central ingredient in this reconstruction is a radically empiricist or even inductivist conception of theory. In this view, economic *theory is a set of empirical generalizations which pertain to observable phenomena.* Friedman tells us, e.g., that the task of economics "is to provide a system of generalizations" (p. 4) and that "economics as a positive science is a body of tentatively accepted generalizations about economic phenomena" (p. 39). The same idea is expressed elsewhere, in his criticism of "Lange on Price Flexibility and Employment": "The theorist starts with some set of observed and related facts, as full and comprehensive as possible. He seeks a generalization that will explain these facts" (p. 282).

3. From the notion of theory as a set of empirical generalizations, it naturally follows that *theoretical assumptions are always excludable:* all sentences in the theory not referring to observable phenomena or regularities can, whenever desired, be substituted for other sentences that have observable referents.

The evidence for this thesis comes from Friedman's examples from natural sciences. In his extended commentary on Galileo's law Friedman first reformulates it as stating that "under a wide range of circumstances, bodies that fall in the actual atmosphere behave *as if* they were falling in a vacuum" (emphasis his) and then as omitting all mention of a vacuum: "under a wide range of circumstances, the distance a body falls in a specified time is given by the formula $s = 1/2gt^2$" (p. 18; here s is distance; g, a gravational constant, 32 feet per second per second on earth acceleration due to gravity; and t, time). 'Vacuum' is sometimes regarded as a theoretical concept not referring to any observable, nonideal state of affairs, and if it can legitimately be eliminated in favor of observational statements, so, one might conclude, can theoretical expressions in general.

A more illuminating example supporting both the excludability thesis and the thesis that theory is equivalent to a set of empirical generalizations is a natural scientific "analogue of many hypotheses in the social sciences." He suggests the hypothesis that the leaves around a tree "are positioned as if each leaf deliberately sought to maximize the amount of sunlight it receives, given the position of its neighbors, as if it knew the physical laws determining the amount of sunlight that would be received in various positions and could move rapidly or instantaneously from any one position to any other desired and unoccupied position" (p. 19). Instead of this "theory" with highly "unrealistic" assumptions

> we could state the equivalent hypothesis, without any apparent assumption, in the form of a list of rules for predicting the density of leaves: if a tree stands in a level field with no other trees or other bodies obstructing the rays of the sun, then the density of leaves will tend to be such and such; if a tree is on the northern slope of a hill in the midst of a forest of similar trees, then . . . ; etc. This is clearly a far less economical presentation of the hypothesis than the statement that leaves seek to maximize the sunlight each receives. The latter statement is, in effect, a simple summary of the rules in the above list, even if the list were indefinitely extended . . . (p. 24).

In fact, he regards it as a central task of theoretical assumptions to describe or present a theory economically. This is why those assumptions do not add anything to our knowledge about the world: they are epistemologically as well as logically dispensable, but they are methodologically useful.

4. It is one of the roles of assumptions, according to Friedman, to specify "the conditions under which the theory is expected to be valid" (p. 23).

It is consistent with other theses in this "positivist" reconstruction to regard these *rules of application as part of theory and as explicitly formulated.* Indeed, Friedman writes that "to specify the circumstances under which the formula works . . . is itself an essential part of the hypothesis" (p. 18). In an orthodox positivist manner he tells us that a theory consists of two parts: an abstract model (an "algebra" or "logic"), and a set of rules defining the set of phenomena to which the theory can be applied and "specifying the correspondence between the variables or entities in the model and observable phenomena" (p. 24).

Our first point in this reconstruction (predictive power as a definite criterion of acceptance) would seem to require that the rules of application be explicitly formulated. It is difficult to find firm support for this idea in Friedman's text, although he does write (in the indicative mood) that "to a considerable extent the rules can be formulated explicitly" and (in the prescriptive mood) that "in seeking to make a science as 'objective' as possible, our aim should be to formulate the rules explicitly in so far as possible" (p. 25). So there seems to be at least a tendency to grasp the (ideal) formulation of the rules as being explicit.

5. It is a natural consequence of the previous theses of this reconstruction that (at least some of) the *assumptions are separately testable and capable of being announced false*. They are directly testable because they are themselves empirical statements or because they are translatable without remainder into empirical generalizations. Friedman writes that "in so far as" the realism of assumptions "can be judged independently of the validity of predictions" (and obviously it can, in Friedman's opinion), "truly important and significant hypotheses will be found to have 'assumptions' that are wildly inaccurate descriptive representations of reality. . . . To be important, therefore, a hypothesis must be descriptively false in its assumptions" (p. 14). In the same vein he criticizes the theory of monopolistic and imperfect competition, because the motivation behind its construction was "the directly perceived descriptive inaccuracy of the assumptions" of the theories of perfect competition and perfect monopoly (p. 15). To the same effect Friedman admits that "businessmen do not actually and literally" behave as they are assumed to behave (p. 22).

6. Perhaps Friedman has so often been labeled a positivist by various commentators because in the beginning of the essay he declares that "positive economics is in principle independent of any particular ethical position or normative judgements" (p. 4). This label is a correct one about the first few pages of Friedman's essay, where he presents the thesis of value-freedom and the belief that all theoretical disagreements can be solved by means of more and more empirical research. In this view, it is *the empirical facts* that are known independently of any theory or ethical standpoint that *guarantee the objectivity of science*.

Friedman writes, e.g., that "differences about economic policy among disinterested citizens derive predominantly from different predictions about the economic consequences of taking action—differences that in principle can be eliminated by the progress of positive economics—rather than from fundamental differences in basic values, differences about which men can ultimately only fight" (p. 5). Thus, given an agreement about the objectives—say, reducing poverty—economists sooner or later reach agreement on policy, irrespective of their theoretical framework, because the framework is totally governed by the neutral facts.

7. All these points are interpreted to form a somewhat coherent whole, which Friedman's incoherent text more or less distantly approximates. The coherence of this reconstruction consists roughly of the following. There are value-free and theory-free facts that are generalized, and the body of these generalizations is summarized in the form of a theory. The statements of scope or applicability are part of the theory. Thus, everybody familiar with the theory can apply it to phenomena in an unambiguous way. All this means that economic theory becomes a summary of observed facts, its content is equivalent to a set of observed facts or empirical generalizations based on them. Because of this, there is nothing in the substantive content of a theory (e.g., its assumptions) that could not be tested against the facts. You should obey the following methodological rules: test the implications, not the assumptions; if the evidence is favorable, accept the theory; if not, reject it. Or, if you happen to test both implications and assumptions, make the following conclusions from a negative test result: in the case of implications, reject the theory; in the case of assumptions, take no action.

III. PRAGMATIST AND CONVENTIONALIST TENDENCIES[2]

1. According to this reconstruction, predictive power is no longer *the* unconditional criterion of theory appraisal. Here *acceptance and rejection* are comprehended to be *contextually conditioned*. The context that influences theory appraisal can, of course, be understood in many ways. In Friedman's essay there seems to be present at least two such contexts, call them "theoretical" and "social."

When Friedman tells us that "the only relevant test of the validity of a hypothesis is comparison of its predictions with experience" (pp. 8–9), his statement matches F1: the only relevant relation is that between the implications of a hypothesis and experience. But the quotation continues:

"The hypothesis is rejected if its predictions are contradicted ('frequently' or more often than predictions from an alternative hypothesis). . . ." Here in the parentheses the dependence of theory appraisal on the relevant theoretical context is expressed: the only relation to be kept in mind is not only that between the theory to be tested and experience but also that between alternative theories. The predictive power of a theory becomes a relative matter, relative to "the accuracy achievable by an alternative theory with which this theory is being compared" (p. 17).

Friedman's nonfalsificationist leanings begin to peep out in the following quotation:

> The denial to economics of the dramatic and direct evidence of the "crucial" experiment does hinder the adequate testing of hypotheses; but this is much less significant than the difficulty it places in the way of achieving a reasonably prompt and wide consensus on the conclusions justified by the available evidence. It renders the weeding-out of unsuccessful hypotheses slow and difficult. They are seldom downed for good and are always cropping up again (p. 11).

When Friedman admits the difficulty of obtaining reliable empirical evidence for testing predictions, he in fact makes room for nonempirical and nonlogical considerations in deciding which theory to choose. According to this interpretation of Friedman, a theory is underdetermined by evidence, and this leaves room for its determination by social factors, i.e., factors related to the relevant scientific community, its members, and its history. This makes even the context of justification at least partly a nonlogical (both noninductive and nondeductive) affair, and Friedman a clear-cut nonpositivist and nonfalsificationist. Here is some uncontroversial evidence for the presence of an irreducible pragmatic or subjective or voluntary element in testing:

> Of course, neither the evidence of the economist nor that of the sociologist is conclusive. The decisive test is whether the hypothesis works for the phenomena it purports to explain. But a judgement may be required before any satisfactory test of this kind has been made, and, perhaps, when it cannot be made in the near future, in which case, the judgement will have to be based on the inadequate evidence available. In addition, even when a test can be made, the background of the scientists is not irrelevant to the judgements they reach. There is never certainty in science, and the weight of evidence for or against a hypothesis can never be assessed completely "objectively." The economist will be more tolerant than the sociologist in judging conformity of the implications of the hypothesis with experience, and he will be persuaded to accept the hypothesis tentatively by fewer instances of "conformity" (p. 30).

I have saved Friedman's most triumphant eulogy for a sort of paradigm-loyalty to the end of this item. Here the social context is given a prominent role in theory appraisal. I quote the eulogy in full:

> [The evidence for the maximization-of-returns hypothesis] is extremely hard to document: it is scattered in numerous memorandums, articles, and monographs concerned primarily with specific concrete problems rather than with submitting the hypothesis with test. Yet the continued use and acceptance of the hypothesis over a long period, and the failure of any coherent, self-consistent alternative to be developed and be widely accepted, is strong indirect testimony to its worth. The evidence for a hypothesis always consists of its repeated failure to be contradicted, continues to accumulate so long as the hypothesis is used, and by its very nature is difficult to document at all comprehensively. It tends to become part of the tradition and folklore of a science revealed in the tenacity with which hypotheses are held rather than in any textbook list of instances in which the hypothesis has failed to be contradicted (pp. 22–23).

2. The idea of a partially social determination of economic theory is strengthened by a few passages where Friedman lets us understand that *the rules of application may be only implicit and discretionary*. This stricture at least concerns their use: no matter how explicitly we succeed in formulating the rules "there inevitably will remain room for judgement in applying the rules. . . . It is something that cannot be taught; it can be learned but only by experience and exposure in the 'right' scientific atmosphere, not by rote" (p. 25). Ten pages later Friedman tells us that the set of rules is "mostly implicit and suggested by example" (pp. 35–36).

It is most interesting that Friedman here seems to anticipate some later ideas of the celebrated Thomas S. Kuhn: there is something in the rules of application that is socially conditioned, linguistically inexplicable, and comprised in a previous archetypical example of scientific practice. The orthodox positivist idea of correspondence rules as explicit components of a theory governing its use is replaced by Kuhn by the notion of an exemplar whose implicit guidance of theory application is brought into scientists' consciousness only through the socialization process of scientific education.

3. It would be in the spirit of this reconstruction to add that the criterion of predictive power works only within *the constraint of some nonempirical presuppositions* that are themselves not to be tested. These presuppositions form a *Weltbild* or a "vision" upon which a whole research tradition is built. They are regulative principles that lay down what problems are to be investigated, what types of solutions are admissible, which items are to count as facts, how the symbols and correlations are to be interpreted, etc. The two theses above about the nature of testing and rules of application give support to this idea. So does Friedman's (1972) talk about the (oral) tradition of Chicago economics.

If this were a correct interpretation, *the theoretical system as a whole* with all its background assumptions would become *the unit of economic knowledge*. This implies that there are no elements in the whole (e.g., single assumptions or predictions) which could be separately tested. It is the whole system that is to be appraised as to its congruence with tradition, its utility to our practical aims, etc.

4. There is one place in Friedman's essay that permits us to conclude that *facts and observations are theory-laden* and that *theories are not excludable*. The quotation is this: "Known facts cannot be set on one side; a theory . . . on the other. A theory is the way we perceive 'facts,' and we cannot perceive 'facts' without a theory" (p. 34). This is to say that there are no pure or brute facts that could function as neutral judges in our theoretical disagreements: people with different theories perceive the facts in different ways (or they may even perceive different facts). Thus, if economic theories are still regarded to be about empirical facts—as implied in this reconstruction—and if those facts are constituted by theories constructed by economists, then the objects of economic theories become subjectively constituted, not objectively given.[3] This is strongly contrary to the positivist doctrine.

Notice that Friedman does not explicitly say that theories are nonexcludable. But this seems to be implied in the quotation. If facts are determined by theories, theories cannot be results of generalization on independent facts. And if theories are not wholly determined by facts, they cannot be eliminated at least by reduction.

5. The pragmatist flavor in this reconstruction becomes clearer when we try to dig out Friedman's implicit semantics. Indeed, he seems to think that *the meaning of terms is dependent on their use*. In his article on "The Marshallian Demand Curve" he writes: "Demand and supply are . . . concepts for organizing materials, labels in an 'analytical filing box.' The 'commodity' for which a demand curve is drawn is another label, not a word for a physical or technical entity to be defined once and for all independently of the problem at hand" (p. 57). On suitable interpretation, one can see the same idea in the following quotation from the "Methodology": "Factual evidence alone can show whether the categories of the 'analytical filing system' have a meaningful empirical counterpart, that is, whether they are useful in analyzing a particular class of concrete problems" (p. 7). On the face of it, the latter quotation does not necessarily talk about the meanings of the terms of the theory. Those terms might have factual meanings independently of any contacts with "empirical counterparts"; indeed, we might quite plausibly require that the meanings be specified before any reliable contacts could be established. But Friedman seems to think that it is only through these contacts that the terms can acquire any factual meaning: without them the terms have only "logical" or "analytical" meaning. And the construction of these contacts is a question of usefulness.

6. To summarize, according to this reconstruction it is theories, irreducible to facts or generalizations, with their pretheoretical presuppositions and sociohistorical context, that dictate what are the facts and what conclusions to draw from any test result. This is to raise the prestige of theory as compared to F1: instead of being totally determined by facts, theory now becomes the ruler of facts. With F2, the economist is not busy in critically testing his theories against hard facts according to methodological rules specifying conditions under which the theories will be considered to be refuted. Central to this view is to make acceptance or refutation of theories depend more on theoreticians themselves than on any objective evidence and well-defined rules. In the pragmatist spirit, theory appraisal becomes dependent on the varying purposes of theorizing and the interests of theoreticians (note Friedman's recurring phrase: ''everything depends on the problem at hand'').

All this is obviously more in accordance with actual scientific practice than F1, especially in economics. No doubt there are strong traditions and schools, tenacity of theories against negative evidence, interpretation and reinterpretation of facts in the light of theory, idealized theories not reducible to empirical generalizations, etc. All this is excluded in the Puritan empiricism of F1.

IV. REALIST TENDENCIES

1. It remains to note that Friedman may be interpreted to give theory a still stronger status, as a representation of unobservable economic realities. ''Realism'' as a name for this view must be sharply distinguished from the cry for ''more realism in assumptions'' in a radically empiricist spirit. Only on this presupposition can realism perhaps be made to serve Friedman's strategy to defend ''unrealistic'' assumptions. Because realism in this sense runs counter to the instrumentalism by which Friedman's essay is most strikingly colored, I find this reconstruction to be very problematic. Here is the evidence, however:

> A fundamental hypothesis of science is that appearances are deceptive and that there is a way of looking at or interpreting or organizing the evidence that will reveal superficially disconnected and diverse phenomena to be manifestations of a more fundamental and relatively simple structure. And the test of this hypothesis . . . is its fruits—a test that science has so far met with dramatic success. If a class of ''economic phenomena'' appears varied and complex, it is, we must suppose, because we have no adequate theory to explain them. Known facts cannot be set on one side; a theory . . . on the other. A theory is the way we perceive ''facts'' . . . (pp. 33–34).[4]

2. As to *the objects of economic theory*, here they are not conceived as observationally given ''appearances''—thus being dependent on our perceptual capabilities, as in F1—nor as subjectively constituted—thus being dependent on our conceptual resources, as in F2. Instead, economic theory is supposed to be about some *independently existing deeper realities* (consisting of "more fundamental structures"), not necessarily observable as such. Thus, a theory cannot be a set of generalizations upon observed phenomena, nor can it be reduced to such a set.

3. *The theory-ladenness of facts,* which is a common element in F2 and F3, now gets a special meaning. It might here be justified as follows. If economic theory is about "a more fundamental structure" and if "deceptive phenomena" are manifestations of this structure, then those phenomena or empirical facts are to be seen *as* such manifestations, and this can be done only with the help of the theory.

4. Note that F3 also specifies *the meaning of 'realism' in F.* While, e.g., according to F1 assumptions of an economic theory are taken as being or not being "realistic" in the sense of accurately describing or not describing observable phenomena, with F3 a theory is or is not "realistic" in the sense of being a true or false representation of nonobservable or not-so-easily-observable fundamental realities.[5] Now F3 would serve as a special solution to the "realism of assumptions" issue: assumptions should be realistic in the sense of aiding us to construct a true representation of the "more fundamental structure" but not in the sense of reproducing carefully the "deceptive appearances." Along these lines, Friedman could maintain that neoclassical economic theory is an ideal theory in that it is realistic in one sense and unrealistic in the other, and that critics of that theory do not understand this distinction.

V. INCONSISTENCIES AS PART OF METHODOLOGICAL RHETORIC

1. If my reading of Friedman's essay is correct, it is highly problematic to attach any single unambiguous label to him—such as "positivist," "falsificationist," "conventionalist," or even "instrumentalist." It also follows that reference to something like "Milton Friedman's view of methodology" as an unambiguous view on the initial pages of several economic textbooks is unfounded. Neither can I agree with Boland (1979, p. 503), who maintains that Friedman's methodology in his essay "is both logically sound and unambiguously based on a coherent philosophy of science."

This is because, on the basis of his essay alone, we have found at least three different possibilities of reconstructing Friedman's view of economic theory and method. Each of them establishes a definite relationship between theory and "reality." According to F1, theories are only anchored to the observational world through testing. According to F2, theories attach themselves primarily to the social reality of scientific communities and traditions. In the case of F3, beside the methodological ties of empirical testing and social ties of paradigm-loyalty, there are direct semantic ties of reference to and representation of underlying economic realities, not unearthed in the empirical evidence nor determined by the social factors. I take F1 and F2 to be incompatible versions of instrumentalism, which the realism of F3 forbids.[6]

It should be emphasized that F1, F2, and F3 are not to be seen as separate theses stated by Friedman or interpretations which cancel each other out concerning what Friedman "really" said or meant. Rather, they are my interpretations of partly contradictory tendencies within his essay. Each reconstruction is an idealization which ignores a number of details. Here and there I have provided relatively strong interpretations of Friedman's text. This I have done for two reasons: first, in order to make the tendencies as coherent as possible; and, second, in order to push those tendencies as far as possible, to see more clearly what they would look like if realized in full. As wholes, so interpreted, they cannot be reconciled.[7]

2. There is a different kind of inconsistency which can be found by comparing Friedman's methodological declarations to his other undertakings. Let us take the thesis F and relate it to Friedman's (1977) critique of John Kenneth Galbraith. Most surprising in this critique is that the killing poison of empirical evidence is injected at all parts or levels of Galbraith's views: to "implications" or "predictive statements" as well as to "theories" or their "assumptions"—and more emphatically to the latter.

For example, Friedman approvingly cites Frank McFadzean, who blames Galbraith for lack of "realism": Galbraith does not understand how business firms actually behave (pp. 19–20). In the same vein Friedman appeals to Harold Demsetz, who has statistically tested Galbraith's claim that modern business firms aim at stability and sales maximization instead of maximum profits—with the result that there is no empirical evidence for the claim (pp. 24–26). From the point of view of F, these statements can obviously be regarded as "assumptions" that need not be tested at all.[8]

3. There is one final and extremely interesting question that must be encountered: why is Friedman inconsistent? One explanation could be that he has not made up his mind which methodology to choose to defend neoclassical assumptions against the critiques; if only he were shown the alternatives and given time enough to think it over, he would make a choice in favor of one of the alternatives.

There is another explanation which takes Friedman's essay as a piece of ingenious rhetoric. The aim of writing the essay most probably was to convince doubters that the critics were wrong. This was done by means of persuasion, polemics, propaganda, ad hoc arguments, conceptual obscurities, shifting the meanings of terms, etc. Examples of these have been given by many commentators on Friedman's essay.

What I have done in this paper may bring new light to one aspect of this rhetoric. Friedman wanted to persuade his readers. So he perhaps tried to fulfill (almost) everyone's expectations about the true or desirable nature of economics: positivism for those aware of the requirements of the then fashionable philosophy of science; logical positivism, in order to fortify the scientific respectability of Friedman's theoretical beliefs[9]; pragmatism and conventionalism for practicing economists with a spontaneous but sharp-eyed self-understanding; and realism for those who might want economics to be cognitively and ontologically more ambitious.

It should be noted that this explanation refers to rhetoric in methodology—"second-order rhetoric," if you like—not rhetoric as part of the method of economics—"first-order rhetoric," described by McCloskey (1983). No doubt you can find the latter, too, in Friedman, but that is not my point. What is important is that here we have rhetoric plus unjustified inconsistency in methodology. This is because Friedman does not have a theory underpinning the rhetoric which might provide a justification for the inconsistencies.

There is a third explanation which I would like to present as a hypothesis. Were it true, Friedman would be permitted to keep all his inconsistencies and still insist on a peculiar kind of coherence on a higher level. Rhetoric and lack of coherence would here strive for metamethodological justification. Perhaps Friedman thinks as follows. Just as one and the same business firm can be regarded as a perfect competitor and as a monopolist quite legitimately, economic theory can be analyzed on the basis of mutually incompatible philosophies of science. Just as there is ontological indifference on the nature of a particular firm, there is indifference on the nature of economic science: what it is about; how it is or should be structured and exercised. In this view there is no one descriptive or prescriptive methodology that could tell us what actually happens in economics or how we should proceed as economists. There is just a set of alternative methodological visions among which we may choose any one that best suits our purpose at hand, e.g., for a defense or a critique of a given economic theory. Thus, there are no "rules of the game" in economics that are or should be uniformly obeyed.

Were this hypothesis correct, Friedman would be a coherent champion of a voluntaristic pragmatism or conventionalism on every level of the scientific enterprise. Everything—how we understand facts, theories, methodologies, etc.—would become a matter of purposeful decision. The "first-order pragmatism" with respect to economic theories would be supplemented by a "second-order pragmatism" with respect to economic methodologies. When he, for example, writes that positive and normative statements should be distinguished, he does not mean that they really are or could be made separate, but only that for certain purposes it might be useful to treat them as distinct. Predictive power is perhaps not observed or prescribed as the primary aim and standard of economics; but whenever useful, treat it as *the* aim and criterion of success. Testing of assumptions should not be required when defending neoclassical theory, but it is proper to make the requirement when criticizing Galbraith's theory. Facts should be treated as given for some purposes but as constructed for others. Theories can be understood as being about observable phenomena when one is trying to persuade the positivist-minded; but when persuasion is directed at the realist-minded, one must take theories as being about deeper realities. And so on with other questions. This is how Friedman could be seen (in some respects) as an ally of Paul Feyerabend (1975), who has developed what he calls an anarchist theory of science with the maxim "anything goes".

To find out the plausibility of this metamethodological hypothesis, one should carefully study Friedman's undertakings as a practicing scientist. There are people who would not be surprised if as a result Friedman were revealed to follow a modified, constrained form of anarchism. His maxim might be: "Anything goes if it is useful for my purposes." Perhaps he is simply in love with certain visions, theories, and policies which he wants to defend—and, as we know, everything is allowed in love (and war).[10]

This is my metamethodological hypothesis that would, if true, explain Friedman's methodological inconsistencies and make his view coherent in a peculiar way. If the hypothesis is false and if the inconsistencies discussed in this paper are not just deceptive appearances, we may conclude that Friedman is just a bad methodologist, not being sufficiently "self-conscious about his methodology."[11]

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NOTES

1. Thus, Rotwein (1959, p. 556) takes it "to be the distinctive aspect of Friedman's methodological analysis . . . that the 'validity' of a 'theory' is to be tested *solely* by its 'predictions' with respect to a given class of phenomena." This idea is also present in Samuelson's (1963, p. 232) reconstruction of Friedman's view as the "F-Twist."

2. One anonymous referee became dissatisfied when I began to specify my loose and general category of pragmatism. Not all of my specifications seem to cohere with the referee's intuitions about what pragmatism is. But, of course, specifications are needed; I could not work with a notion which would be compatible with all conceivable versions of pragmatism. Strictly speaking, I should talk about "the pragmatist conventionalism interpreted out of a selected set of ingredients in Friedman's essay" instead of talking about "pragmatism." So let "pragmatism" or "pragmatist conventionalism" serve as a shorthand for the former expression. And keep in mind that I am not attempting to provide statements of the pragmatisms of such masters as Charles S. Peirce, John Dewey, or William James. I use "pragmatism"—note that I use it very sparingly—as a generic category which covers much of the thinking of such modern writers as Willard Quine, Thomas Kuhn, Steven Toulmin, Larry Laudan, and Richard Rorty.

3. One anonymous referee makes the claim that (in his/her words) "if one recognizes that there is no perfect algorithm of theory choice which enables us to make valid choices always on rational grounds it does not follow that" (now he/she quotes my words) " 'then the objects of economic theories become subjectively constituted, not objectively given,' " which, according to the referee, would make one a "subjectivist." But the referee here seems to confuse the lack of a "perfect algorithm of theory choice" with the theory-ladenness of facts. It is the latter which I take as the basis for the subjective constitution of the objects of economic theories.

Note also that if the notion of "subjectivism" is used in connection with F2 (I do not use it), it is to be understood as referring to some kind of "social subjectivism," not "individual subjectivism."

A related counterargument of this referee is the claim that the theory-ladenness of facts does not preclude their "objectivity" or agreement on the facts among those that hold different theories. I might agree with this claim, but this would not imply that there are compelling reasons which are independent of the social context and which could bring about agreement among people. The only thing which is presupposed by F2 is that the conditions and mechanisms of agreement and disagreement are irreducibly social in character. There are no asocial sense perceptions of atheoretical facts which could help accomplish the social state of agreement—these are presupposed by "objectivity" in the sense of F1. This does not necessarily make science a "chaotic enterprise" as the referee suspects; on the contrary, F2 implies that the social conditions of an orderly agreement can be very favorable indeed.

This means (to allay the suspicions of another anonymous referee) that F2 does not imply a "nihilistic" view of science, according to which "there is no way to resolve disagreements between different theorists as to what the facts are. If so, there can be no generally acceptable empirical tests, since these tests must rest on agreement concerning the facts." This claim by the second referee seems to have as its premise the belief that agreement in science can only be of the positivist kind, based on atheoretical and unproblematic sense perceptions. I think this premise is false.

4. Though I here interpret this passage along realist lines, I do not want to exclude the possibility of a conventionalist reading of the whole of it. The checking of this possibility is a task to be performed on another occasion, however.

5. I have added this passage as a response to one anonymous referee who claimed that what F3 contradicts is F and not F1 or F2. It seems that the referee did not notice the ambiguity of the term 'realism' in F.

6. This means that I take instrumentalism to be a more general category than positivism, pragmatism, or conventionalism, and one which is not necessarily incompatible with them: F1 and F2 are instrumentalist views since neither of them accepts that economic theories are or should be true representations of the independently existing world, whether observable or not. The latter view, denied by F1 and F2, is the realism of F3.

7. One anonymous referee makes a serious and important criticism that "in seeking a variety of tendencies in Friedman [the writer is] led down minor pathways and . . . [the] paper draws attention away from and obscures the place of Friedman's principal argument," this being concerned with the thesis that "the test of a theory's validity consisted solely of a test of its predictions, not its assumptions." I want to respond carefully to this criticism. I do not find it difficult to agree that something like that characterized by the referee was, indeed, Friedman's "principal argument." I have tried to capture it in the formulation of the basic thesis F. But F is far from unambiguous in its exact import. Therefore, most of my paper is devoted to showing how F and its individual components can be explicated—just by referring to Friedman's own text. A major thesis of my paper is that there are mutually incompatible explications to be found in this way. Thus, any reasonable discussion on "Friedman's principal argument" must begin with an explication (or explications) of it. So, instead of admitting that my paper "draws attention away from and obscures" Friedman's major thesis, I would like to maintain that my paper draws attention to the obscurities of Friedman's major thesis. This is not to deny that there still remain a huge amount of unclear points relating to F.

8. Other examples of the violation of F in Friedman's own work have been given, e.g., by Archibald (1961) and Caldwell (1980).

9. Now that Karl Popper is in fashion in some circles as an authoritative proponent of the scientific method, Friedman has indicated that his views "can be aligned with" those of Popper although he (Friedman) at the same time accepts instrumentalism (see Frazer and Boland, 1983, p. 129), which is—the counterarguments of Frazer and Boland notwithstanding—incompatible with Popperianism. This statement by Friedman clearly gives support to my interpretation of the nature of his methodological thought.

10. One anonymous referee wanted me to revise much of the paper in the spirit of the claim that Friedman is "a normativist who uses economics to justify his belief that 'the best government is the least government.' Thus, he could be construed as an instrumentalist in the sense that facts and theories are mere instruments in his 'proof' that the summum bonum is the 'free market,' defined as the absence of government interference or participation." So the referee would like me to be more specific as to what those "certain visions, theories and policies" are and how Friedman's commitment to them affects his behavior as an economist. It is evident that this suggestion accords well with what I say in the text. But to pursue this line of argument would make it necessary to incorporate in my paper much more varied sources of evidence than Friedman's essay alone, and to strive for an interpretation of the whole of Friedman's intellectual activity—not only of his methodological essay. As a matter of fact, the referee's suggestion implies performing exactly the task stated in the first sentence of the present paragraph in the text.

11. An important qualification is needed here. The foregoing portrays Friedman's essay as methodological rhetoric. But rhetoric is a pragmatic affair which can be considered from the angle of the writer/speaker or from that of the audience. In the foregoing the writer's angle (Friedman's conscious intentions) is put in the forefront. But we could opt for another approach. We might abstract from Friedman's intentions and take his essay as a text existing in its own right. This procedure would make the point of view of the audience central. Then we would be interested only in how the text is received by readers, how it affects readers, but not whether these effects are those intended by the writer. This is how we could try to liberate Friedman from understandable accusations of being a Machiavellian—but not from those of being a bad methodologist.

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