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## ASSUMPTIONS IN ECONOMIC THEORY

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Sound conclusions are sometimes supported by erroneous arguments, and the error is compounded when a sound conclusion is declared to be mistaken on the ground that the argument for it is mistaken. This general observation must serve as my *apologia* for venturing to discuss an important and much debated methodological issue in economics, though not myself an economist. In his well-known essay, "The Methodology of Positive Economics,"<sup>1</sup> Professor Milton Friedman defends the use of abstract (and in particular, neoclassical) theory in economic analysis, in effect by defending the principle that the adequacy of a theory must be judged, not by assessing what he calls the "realism of its assumptions," but rather by examining the concordance of the theory's logical consequences with the phenomena the theory is designed to explain—a principle which many economists continue to reject, frequently because arguments similar to his seem to them mistaken. I also think that his argument provides no firm support for this principle; and, indeed, my paper is a critique of his defense of it. However, the relevance of my paper is not, I think, limited to Professor Friedman's essay, for I hope to show that despite the inconclusiveness of his argument his conclusion is sound.

### I

Since the notions of theory and assumption are central in discussions of the principle at issue, it is convenient to begin by noting some distinctions.

1. The word theory is often used in the social sciences (including economics) rather loosely, to designate almost any general statement, however narrow its intended range of application may be. Thus, the label is commonly given to empirical generalizations (often stated in the form of equations obtained with the help of techniques of curve fitting) that are simply extrapolations from observed statistical regularities, and are asserted to hold only for behaviors occurring in a given community during some particular historical period. On the other hand, many economists (including Professor Friedman) employ the word far more selectively, and approximately in the sense associated with it when it occurs in such phrases as "the Newtonian theory of motion."

<sup>1</sup> It is published in his *Essays in Positive Economics* (Chicago, 1953). All page references, unless otherwise noted, are to this book.

It is in this second sense that theory will be used in this paper. Accordingly, an economic theory (e.g., the neoclassical theory of consumer choice) is a set of statements, organized in a characteristic way, and designed to serve as partial premisses for explaining as well as predicting an indeterminately large (and usually varied) class of economic phenomena. Moreover, most if not all the statements of a theory have the form of generalized conditionals, which place no spatiotemporal restrictions on the class of phenomena that may be explained with their help. For example, the law of diminishing returns can be expressed in this form: If the quantity of a factor of production is augmented by equal increments, but the quantities of all other factors are kept constant, then the resulting increments in the product will eventually diminish. Space is lacking for discussing adequately the anatomy of theories, but a few additional features distinctive of them must be briefly mentioned.<sup>2</sup>

2. In a given codification of a theory, the statements belonging to it can be divided into three subgroups. The first consists of statements which count as the fundamental ones, and are often called the theory's "assumptions" (or basic "hypotheses"); the second subgroup contains the statements that are logically deducible as theorems from statements in the first. However, the term "assumption" is sometimes also used to refer to the antecedent clause of a conditional theoretical statement in either of these subgroups. This is the way Professor Friedman seems to use the word when, in discussing Galileo's law for freely falling bodies (i.e., "if a body falls toward the earth in a vacuum, its instantaneous acceleration is constant"), he asks whether this law does in fact "assume" that bodies actually fall through a vacuum.

The third subgroup of theoretical statements can also be readily characterized, if we recall that many (and perhaps all) statements in the first two subgroups contain expressions which designate nothing actually observable and are not explicitly definable in terms of expressions that do. Familiar examples of such expressions (for easy reference I will call them "theoretical terms") are "vacuum" in Galileo's law, "gene" in biological theory, and "elasticity of demand at a point" in neoclassical economic theory. Theoretical terms signify either various entities that cannot be specified except by way of some theory which postulates their existence, or certain ideal limits of theoretically endless processes. It is therefore evident that statements containing such terms cannot possibly explain or predict the course of actual events, unless a sufficient number of theoretical terms (but not necessarily all of them) are co-ordinated with observable traits of things.

<sup>2</sup>A more detailed analysis is contained in my *The Structure of Science* (New York, 1961), especially Chaps. 5 and 6.

Thus, although the theoretical terms "instantaneous acceleration" and "perfectly divisible commodity" describe nothing that can be identified in experience, the expressions do in fact correspond to empirically determinable features in certain actual processes as a consequence of various rules employed (usually tacitly) by physicists and economists. In addition to the two subgroups already mentioned, a theory will in general therefore also contain a third subgroup of statements (though commonly not fully formulated) that indicate among other things such correspondences. It must be emphasized, however, that these statements do not define theoretical terms by way of terms signifying observable traits, so that theoretical terms cannot be eliminated from formulations in which they occur with the help of these statements.<sup>3</sup>

3. One further point deserves mention in this connection. In most disciplines, theoretical formulations (particularly those in the first two subgroups) are normally treated as statements about some subject matter, so that as in the case of other statements questions about the truth or falsity of such formulations are regarded as significant though difficult to answer. On the other hand, theoretical formulations are sometimes denied the status of "genuine" statements and are said to be simply rules which are instrumental for drawing inferences from genuine statements but which cannot be properly characterized as true or false. It is impossible in the space available to examine the merits of these opposing views on the status of theories. I have mentioned them to call attention to the fact that a defense of the methodological principle under discussion is intelligible only on the supposition that economic theory is a set of genuine statements, so that considerations of their truth or falsity are not irrelevant to the objectives of economic analysis.

## II

Professor Friedman rests his argument for the methodological principle on some general reflections concerning the nature of theories *überhaupt*. He notes that a theory cannot explain a class of phenomena, unless it abstracts a small number of "common and crucial elements" (in terms of which the phenomena may be predicted) from the mass of differing circumstances in which the phenomena are embedded. Ac-

<sup>3</sup>This point is of major importance. Professor Friedman also recognizes a category of statements in a theory roughly equivalent to the third subgroup of theoretical statements distinguished above; but he appears to believe that theoretical terms can be eliminated with the help of statements in this category. The point at issue cannot be adequately discussed in short compass, but an example will perhaps make clear why such a belief is dubious. Quantum theory is stated in terms of various theoretical terms, referring to such elementary particles as electrons. However, although physicists are certainly able to apply quantum theory to observable processes with the aid of statements in the third subgroup, such statements of correspondence do not permit the elimination of terms like "electron" from quantum theory.

cordingly, the assumptions of a satisfactory theory are inescapably "descriptively false" or "unrealistic," so that it is pointless to assess the merits of a theory by asking whether or not its assumptions are realistic. The relevant question is whether or not the theory yields predictions which are "sufficiently good approximations for the purpose at hand."<sup>4</sup>

However, an assumption may be unrealistic in at least three senses important for the argument, though Professor Friedman does not distinguish them.

1. A statement can be said to be unrealistic because it does not give an "exhaustive" description of some object, so that it mentions only some traits actually characterizing the object but ignores an endless number of other traits also present. However, no finitely long statement can possibly formulate the totality of traits embodied in any concretely existing thing; and it is difficult to imagine what a statement would be like that is not unrealistic in this sense, or what conceivable use such a statement could have. But in any event, it is with this rather trivial sense of the word in mind that Professor Friedman seems frequently to defend the legitimacy of unrealistic assumptions in economic theory;<sup>5</sup> and although it is not clear whether any economists have maintained a contrary thesis, his defense is fully conclusive.

2. A statement may be said to be unrealistic because it is believed to be either false or highly improbable on the available evidence. Such lack of realism can sometimes be established on the basis of what Professor Friedman calls a "directly perceived descriptive inaccuracy"; but in general, statements can be shown to be false only "indirectly," by first deducing from them some of their logical consequences (or implications), and then comparing the latter with "directly" observed matters of fact. Since it is usually not possible to establish the falsity of theoretical statements directly, Professor Friedman correctly stresses the relevance of this indirect procedure for ascertaining whether a theory is unrealistic. Nevertheless, as he recognizes and even illustrates,<sup>6</sup> the distinction between an assumption and its implications is a sharp one only in a given formulation of a theory—an implication of some assumption in one formulation may in another formulation be a premise implying that assumption. Accordingly, his repeated claim that an assumption can be rightly tested for its realism only indirectly obviously needs qualification.

But in any event, if by an assumption of a theory we understand one of the theory's fundamental statements (i.e., those belonging to

<sup>4</sup> Pp. 14-15.

<sup>5</sup> Pp. 18, 25, 32, 35.

<sup>6</sup> Pp. 26-27.

the first of the three subgroups previously noted), a theory with an unrealistic assumption (in the present sense of the word, according to which the assumption is false) is patently unsatisfactory; for such a theory entails consequences that are incompatible with observed fact, so that on pain of rejecting elementary logical canons the theory must also be rejected. On the other hand, a universal conditional neither asserts nor presupposes that the conditions explicitly stated in its antecedent clause are actually realized; accordingly, a theoretical statement having this logical form is not proved to be false by showing that the specifications in its antecedent are not embodied in some given spatiotemporal region (or for that matter, in any region). Professor Friedman is therefore quite right in maintaining that a theory is not necessarily erroneous merely because its assumptions are unrealistic—provided that he is taken to mean by an “assumption of a theory,” as he sometimes appears to mean, an antecedent clause of some theoretical statement. However, a theory whose assumptions are in this sense unrealistic for a given domain is simply inapplicable in that domain, though it may be applicable in another. But what is to be said of a theory whose assumptions are ostensibly unrealistic for every domain? The aspect of this question that is especially relevant to Professor Friedman’s essay is best treated after the third sense of unrealistic has been explained.

3. In many sciences, relations of dependence between phenomena are often stated with reference to so-called “pure cases” or “ideal types” of the phenomena being investigated. That is, such theoretical statements (or “laws”) formulate relations specified to hold under highly “purified” conditions between highly “idealized” objects or processes, none of which is actually encountered in experience. For example, the law of the lever in physics is stated in terms of the behavior of absolutely rigid rods turning without friction about dimensionless points; similarly, a familiar law of pricing in economics is formulated in terms of the exchange of perfectly divisible and homogeneous commodities under conditions of perfect competition. Statements of this kind contain what have previously been called “theoretical terms,” which connote what are in effect the limits of various non-terminating series and which are not intended to designate anything actual. Such statements may be said to be unrealistic but in a sense different from the two previously noted. For they are not distinguished by their failure to provide exhaustive descriptions, nor are they literally false of anything; their distinguishing mark is the fact that when they are strictly construed, they are applicable to nothing actual.

However, laws of nature formulated with reference to pure cases are not therefore useless. On the contrary, a law so formulated states how

phenomena are related when they are unaffected by numerous factors whose influence may never be completely eliminable but whose effects generally vary in magnitude with differences in the attendant circumstances under which the phenomena actually recur. Accordingly, discrepancies between what is asserted for the pure case and what actually happens can be attributed to the influence of factors not mentioned in the law. Moreover, since these factors and their effects can often be ascertained, the influence of the factors can be systematically classified into general types; and in consequence, the law can be viewed as the limiting case of a set of other laws corresponding to these various types, where each further law states a modified relation of dependence between the phenomena because of the influence of factors that are absent in the pure case. In short, unrealistic theoretical statements (in the third sense of the word) serve as a powerful means for analyzing, representing, and codifying relations of dependence between actual phenomena.

### III

Professor Friedman's discussion of unrealistic assumptions in examples of theoretical statements drawn from physics and biology sheds important light on his defense of such assumptions in economic theory. It will therefore be useful to examine his account of one of these examples.

1. In his discussion of Galileo's law, Professor Friedman notes that the law is stated for bodies falling in a vacuum, but also declares that the law "works" in a large number of cases (i.e., it is in sufficiently good agreement for certain purposes with the actual behavior of bodies in these cases), though not in others. He therefore suggests that the law can be restated to read: Under a wide range of circumstances, bodies that fall in the actual atmosphere behave *as if* they were falling in a vacuum. Indeed, he seems to think that the law can be rephrased without mentioning a vacuum, as follows: Under a wide range of circumstances, the distance a body falls in a specified time is given by the formula  $s = \frac{1}{2}gt^2$ . Accordingly, he maintains that the circumstances in which the law works (and is therefore acceptable) must be specified as "an essential part" of the law, even though this specification (and in consequence also the law) may need revision in the light of further experience.<sup>7</sup>

However, as has already been indicated, the term "vacuum" is a theoretical one, so that Galileo's law in its standard version is formulated for pure cases of falling bodies. Professor Friedman's proposed paraphrase which omits all mention of a vacuum thus rests on the supposition that theoretical terms can in general be replaced by non-

<sup>7</sup> Pp. 18-19.

theoretical ones, without altering the meaning and function of the statements containing them. But the possibility of such a replacement is dubious on formal grounds alone; and what is more important, the suggestion that unless theoretical terms can thus be eliminated the statements containing them are scientifically otiose, overlooks the rationale for stating laws in terms of pure cases. In point of fact, the proposed paraphrase mistakenly assumes that Galileo's law can be assigned the functions actually performed by statements of correspondence (belonging to the third subgroup of theoretical statements) without impairing the effectiveness of the standard formulation for achieving systematic generality in theoretical physics.

2. The example Professor Friedman uses for the most part in his defense of unrealistic assumptions in economics is the familiar "rational maximization of returns" hypothesis in the theory of the firm. However, he states it as follows: "Under a wide range of circumstances, individual firms behave *as if* they were seeking rationally to maximize their expected returns and had full knowledge of the data needed to succeed in this attempt."<sup>8</sup> He freely admits that as a rule businessmen lack such knowledge and do not perform the intricate calculations required for ascertaining the indicated maximum. Indeed, he declares that "the apparent immediate determinants of business behavior" could be anything at all; e.g., ingrained habit or a chance influence. He nevertheless claims that these admitted facts do not affect the validity of the hypothesis. The relevant evidence, according to him, is the large set of facts in good agreement with various implications of the hypothesis, including the fact that firms whose actions are markedly inconsistent with it do not survive for long.

It is pertinent to ask, however, whether the operative premise from which these implications really follow is perhaps the supposition, suggested by Professor Friedman's discussion, that is rendered by: "Under a wide range of circumstances, the behavior of individual firms brings them returns approximately equal to a certain magnitude (called the maximum of expected returns by economists)"; or whether the operative premise is the hypothesis as he formulates it. On the first alternative, most of the matters mentioned in his "as if" formulation are irrelevant to the substantive content of the hypothesis. In particular, the hypothesis must then not be understood as either asserting or implying that firms conduct their affairs in order to achieve some objective. To be sure, the statement of the hypothesis contains the expression "the maximum of expected returns"; nevertheless, this expression simply designates a set of rules used by economists rather than by firms for calculating a certain magnitude. In short, the hy-

<sup>8</sup> P. 21.



pothesis in this case is a somewhat loosely expressed empirical generalization about the returns firms actually receive as the outcome of their overt behavior, and it specifies no determinants in explanation of that behavior.<sup>9</sup> Accordingly, although the hypothesis is not an exhaustive description of anything, it is not clear in what sense other than this trivial one the hypothesis is in this case unrealistic if, as Professor Friedman claims, it is in good agreement with experience. On the second alternative, however, it is difficult to avoid reading the hypothesis as saying that firms do seek to maximize their returns in a rational manner, since otherwise it appears to be asserting nothing whatsoever. But the hypothesis must then be understood as dealing with pure cases of economic behavior, requiring the use of theoretical terms in its formulation which cannot be replaced by nontheoretical expressions. Accordingly, the various facts Professor Friedman freely admits but thinks are irrelevant may in this case be quite pertinent in assessing the merits of the hypothesis.

Professor Friedman's essay does not indicate explicitly which alternative renders the hypothesis as he understands it. In consequence, the essay is marked by an ambiguity that perhaps reflects an unresolved tension in his views on the status of economic theory. Is he defending the legitimacy of unrealistic theoretical assumptions because he thinks theories are at best only useful instruments, valuable for predicting observable events but not to be viewed as genuine statements whose truth or falsity may be significantly investigated? But if this is the way he conceives theories (and much in his argument suggests that it is), the distinction between realistic and unrealistic theoretical assumptions is at best irrelevant, and no defense of theories lacking in realism is needed. Or is he undertaking that defense in order to show that unrealistic theories cannot only be invaluable tools for making predictions but that they may also be reasonably satisfactory explanations of various phenomena in terms of the mechanisms involved in their occurrence? But if this is his aim (and parts of his discussion are compatible with the supposition that it is), a theory cannot be viewed, as he repeatedly suggests that it can, as a "simple summary" of some vaguely delimited set of empirical generalizations with distinctly specified ranges of application.<sup>10</sup>

Curiously enough, something like the notion that theories can be viewed in this manner underlies one criticism of Professor Friedman's

<sup>9</sup>In particular, the hypothesis does not include the assumption, integral to many formulations of neoclassical theory, that firms are purposive agents, whose decisions are based on rationally formed estimates of the relative advantages and risks associated with alternative courses of action open to them. See, for example, Frank H. Knight, *Risk, Uncertainty and Profit* (London, 1957), and Paul A. Samuelson, *Foundations of Economic Analysis* (Cambridge, Mass., 1947), Chap. III.

<sup>10</sup>P. 24.

defense of the maximization-of-returns hypothesis. Thus Professor Koopmans argues that if (as Professor Friedman holds) the fact that firms whose behavior diverges from it are not likely to survive is a basis for accepting the hypothesis, "we should postulate that basis itself and not the profit maximization which it implies in certain circumstances."<sup>11</sup> This seems like a recommendation that since a basis for accepting Newtonian gravitational theory is the fact that observed regularities in the motions of the planets are in agreement with various special laws deduced from the theory, we should postulate those regularities rather than the theory—a recommendation that would replace the theory by the empirical evidence for the theory. Such a proposal not only rejects the conception that theories have an explanatory function; it also overlooks the irreplaceable role theories have in scientific inquiry in suggesting how empirical generalizations may need to be corrected, as well as in directing and systematizing further empirical research. Unless I have seriously misunderstood Professor Friedman's essay, he would reject a proposal of this sort. Nevertheless, at various points in his argument he seems to construe theoretical statements in a manner that is almost indistinguishable from what is implied by such a proposal. I have therefore tried in this paper to show where his argument lacks cogency, as well as to indicate why the main thesis he is ostensibly defending is nonetheless sound.

<sup>11</sup> Tjalling C. Koopmans, *Three Essays on the State of Economic Science* (New York, 1957), p. 140.