

Why an Economic Model with Unrealistic Assumptions Need Not be a Bad Model

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Milton Friedman's influential and controversial "The Methodology of Positive Economics" begins by posing a methodological question fundamental to economics, and indeed to all sciences; that of "how to decide whether a suggested hypothesis or theory should be tentatively accepted as part of the 'body of systematised knowledge concerning what is'" (Friedman, 1953, p. 3). Friedman's answer to the question is that "the only relevant test of the validity of a hypothesis is comparison of its predictions with experience" (Friedman, 1953, p. 8). Judging a theory instead by the "realism of its assumptions" is something he categorically, and with unsparing force, rejects.

In the following essay, an effort will be made to interpret what Friedman means when he refers, as he so often and ambiguously does, to assumptions of a theory as "unrealistic." This clarification will permit a clearer view of the points of disagreement and the room for common ground between Friedman and his causalist critics, who reject his instrumentalist view of science in favour of one where the realism of assumptions, particularly their causal role, is of paramount importance in judging the value of a theory. Once these competing claims are evaluated, I will argue that whether a model with unrealistic assumptions – in any sense of "unrealistic" considered – is a good model depends on the purposes for which it is being used.

The first order of business when assessing the merit of Friedman's defense of unrealistic theoretical assumptions is to understand what is meant both by a theory's assumptions and by the characterisation of them as unrealistic. Here, philosopher Ernest Nagel's work is of much use (Nagel, 1963). Nagel notes that the assumptions of a theory may refer to the most basic, foundational statements in a theory, its basic hypotheses. In economics these might include statements such as 'producers make production decisions so

as to maximise their profit given feasible technologies' or 'consumers make consumption decisions so as to maximise their utility given their budget.' From these basic statements other statements are logically derived (such as statements about how equilibrium prices and quantities in a market change given impositions of sales taxes, for example) and together these foundational and derived statements compose a theory.

The assumptions of a theory in a second sense, writes Nagel, refer to the "antecedent clauses of conditional theoretical statements" (Nagel, 1963, p. 212) of either the derived or foundational type.¹ However, many theoretical conclusions arrived at in economics rely on a cumulative theoretical framework. Here, a collection of foundational and derived theoretical statements work jointly as antecedents in a conditional statement for which the theory's prediction is the consequent. As such, this second definition of assumption encompasses the first. Testing the realism of a theory's assumptions therefore amounts to testing the realism of all the foundational and derived theoretical statements that are taken to provide support for the prediction of the phenomena.

But what is it to be (un)realistic? Friedman is unclear about what he means in general when referring to a theory's assumptions as lacking realism. Nagel argues that there are three ways in which Friedman classifies assumptions as being unrealistic. Sometimes he seems to mean that assumptions are unrealistic if they do not exhaustively enumerate all the varied characteristics of objects being studied. Alternatively, he seems to mean that assumptions are unrealistic because the theory is an idealised one, which, in the words of Nagel, "formulate[s] relations specified to hold under highly 'purified' conditions between highly 'idealised' objects or processes, none of which is actually encountered in experience" and which therefore are "not intended to designate anything actual" (Nagel, 1963, p. 215). There is a third sense in which an assumption can be considered unrealistic: if the theory is indeed intended to refer to actual objects in the world but provides descriptions that are patently false. Though it is not clear whether Friedman would hesitate to defend this sort of unrealistic assumption too, it is not the

1 The conditional theoretical statement 'if p then q' may be either a foundational theoretical statement, that is a basic hypothesis, or a statement derived from it, that is a derived theoretical statement, according to Nagel, where p is taken as the "assumption" in the second sense. If p is interpreted as the chain of logic, including foundational and derived statements that leads to theoretical conclusion q then the assumption of this conditional is the chain of foundational and derived statements, which is shown to be realistic if and only if all the foundational and derived statements are realistic (Nagel 1963).

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sense of “unrealistic” that his primary discussion suggests and so it will be ignored in the following analysis while a treatment of the other two senses will be given.

Friedman's argument contains much to suggest that he wishes to defend the use of unrealistic assumptions in the first, non-exhaustive sense. He maintains that such non-exhaustiveness is a central attribute of good scientific theories throughout the scientific disciplines. This is because a good theory should abstract a small number of “common and crucial elements” from the many elements out there co-occurring with the phenomena of study, seizing on these as the essential determinants of the phenomena, thus allowing much to be explained by little (Friedman, 1953, pp. 14-15).

Alternatively, trying to provide a completely realistic set of assumptions – in the sense of being descriptively exhaustive – is an impossible aim as it would require a complete specification of every characteristic possessed by the object referred to in the assumption (from a producers' cost aversion to their eye colour, to use Friedman's example, found below). Thus, an economist must always select some of the many possible descriptors to include in a given model, showing preference for those that are the “crucial” elements that determine the phenomena under study. Unless one has full prior knowledge of all the causal factors at play – making the need for scientific investigation unnecessary – the only way to tell which descriptive elements are the crucial ones is to test if the predicted phenomena result from the assumptions chosen. Friedman therefore dispatches with the feasibility of a fully exhaustive set of assumptions and redefines the resultant question as to which non-exhaustive assumptions to include in a theory as a question of testing theoretical predictions against experience.

In this way, Friedman maintains that concerns about the exhaustiveness of a set of assumptions are impractical; any attempt to measure the value of a theory by testing the realism of the theory's assumptions reduces to testing the accuracy of the predictions of a theory. Using a particular example chosen in response to criticisms of the neo-classical theory of the firm, Friedman summarizes:

What is the criterion by which to judge whether a particular departure from realism is or is not acceptable? Why is it more “unrealistic” in analysing business behavior to neglect the magnitude of businessmen's costs than the colour of their eyes? The obvious answer is because the first makes more difference to business behavior than the second; but there is no way of knowing that this is so simply by observ-

ing that businessmen do have costs of different magnitudes and eyes of different colour. Clearly it can only be known by comparing the effect on the discrepancy between actual and predicted behaviour of taking the one factor or the other into account. Even the most extreme proponents of realistic assumptions are thus necessarily driven to reject their own criterion and to accept the test by prediction when they classify alternative assumptions as more or less realistic (Friedman, 1953, pp. 32-33).

This first interpretation of what Friedman means by unrealistic assumptions opens the door for Friedman and his causalist critics to jointly endorse the same set of theories. Though Friedman would say he is endorsing them because they predict well, his reliance on making use of the “common and crucial elements” that determine the phenomenon of study suggests an implicit acceptance of the tendency laws of Mill. Mill and other causalists are not so foolish as to wish for economic theories that gratuitously include descriptive elements like eye colour which have no expected implication for economic behavior. Instead, they would prefer an economic theory to include only the essential underlying causes that determine a given economic phenomena across a range of cases (Mill, 1994, pp. 52-68) In essence, Friedman is revealing a preference for that very characteristic by aspiring to isolate the “common and crucial elements” relevant to a particular phenomena. In a way therefore he may be indirectly recognising a likely coincidence of those assumptions that are predictively powerful and those that are causally deterministic.

When it comes to Friedman's other usage of “unrealistic assumptions” (to designate theories that rely on idealised conditions not intended to apply to any actual domain) there is less room for causalists to endorse any of the theories of the sort promoted by Friedman. Friedman intends with his discussion of Galileo's theory of acceleration of a body dropped in a vacuum and with his sunlight-maximising tree story to endorse the use of such idealised theories. The primary causalist objection is that since idealised theories assert relations of dependence that hold under purified conditions, but do not exist in the world, one has no indication as to in which contexts they are applicable and in which they are not. While this objection is quite reasonable, if one looks into basic consumer models, for example, there is reason to think that through successive experimentation knowledge about when to apply aspects of economic theory can be gained even when the theory is grounded in idealised assumptions. That consumers are fully rational utility maximisers certainly must be seen as an unrealistic assumption in the ide-

alised theory sense. Consumers do qualitatively possess the traits of homo economicus, but the idealised consumer of economic theory is a limit case of the average consumers' actual psychological capabilities. Experimentation by Kahneman and Tversky and others has revealed domains of consumer choice (choices where status quo bias, framing issues, and base rate neglect are relevant, for example) in which standard models of the rational consumer do not predict well. This, they argue, is due to agents' divergence from idealised assumptions about consumer rationality (transitivity of preferences in particular), the computational skills necessary for consistent decision making and other basic assumptions made on consumers in standard economic models (Kahnemann & Tversky, 2003). Thus, while the idealised rational consumer model may have been initially applied to all consumers without any sense as to where it could accurately predict behavior, after observing domains of behaviour in which the model does not predict well, experimentation allows us to develop partial domains of its applicability and inapplicability.²

Causalists argue that a model is applicable in a given set of circumstances if it includes the main causes. It should be asked therefore, what criteria could a causalist provide for when a model includes the main causes? Would he be able to just know? Would he be fortunate enough as to always provide the correct intuition? If that is the answer – and the certainty with which Mill is committed to tendency laws as being invariant of experience suggests that a causalist like him might take it to be – then such an account cannot really be taken to be scientific, since it asserts truth independent of empirical verification. At times, Mill appears to be asserting that the tendency laws he envisions are invariant, omnipresent features of the world, that even if they are not confirmed by experimentation they must still exist with their effect only masked by disturbing tendency laws. In this sense, Mill might better be considered to have a metaphysical theory, one that asserts the nature of causal reality as embodied in tendency laws, rather than an epistemological theory that provides direction about how one comes to know about this reality or is given a method to determine when a theory is true or false.

In contrast, Friedman can be seen as advancing an epistemological the-

2 Admittedly, this is a progressive view of economics and many of the contributions found in the Kahneman and Tversky text come from those once working outside the traditional channels of economic research. However, I take the normative view that experimental and behavioral economics compose a fundamental part of valuable and instructive economic research.

ory, where Popperian falsificationism is taken as the method by which certain economic theories are shown to be invalid and untrue about the world. However, when it comes to the positive statement about what theories are true about the world, Friedman's stalwart method of comparing a theory's predictions with experience is not helpful. Simply because a given set of assumptions lead to accurate predictions one can not infer a causally extant link from the assumptions to the predicted phenomena. A model could be specified with assumptions whose presence is directly correlated with the actual unspecified factor responsible for the phenomena observed. More generally, any set of assumptions could for whatever reason just happen to lead a model to make accurate predictions without really being causally integral to the phenomena predicted. This is the heart of the causalist concern with Friedman's methodology. One could be predicting phenomena quite well under his method but have gained no real knowledge about why the phenomena are actually occurring.

Thus, whether Friedman's method is a good method, and whether models with unrealistic assumptions derived from it are useful models, depends on what one is using them for. If it is to fulfil the predictive function of science and develop theories that enable accuracy in prediction, then the method is appropriate and the models derived from it can be fruitful. If, on the other hand, it is to fulfil the explanatory function of science and provide understanding of the causal factors that determine how the world works, then there is significant doubt about whether Friedman can deliver.

References

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