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Milton Friedman Letters

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Among the most famous essays in economics is Milton Friedman's "The methodology of positive economics," published in 1953. Friedman's main point was that a theory's validity should not judged by the realism of its underlying assumptions but rather by the accuracy of its predictions. In effect, theories should be viewed "as if" their assumptions were true.

Although his essay generated enormous controversy, Friedman chose never to formally respond to his critics or further write on methodology. As a result, there has been a never-ending debate as to what Friedman "really believes" on this topic and whether his approach helps or harms research in economics and related disciplines, such as finance.

In the late 1980s, I was working on a paper eventually published in 1991, "A Transactional Approach to Economic Research." I sent an early version and later a final draft to Friedman and he replied in a June 2, 1988 letter and another letter on April 3, 1990. These letters contain useful insights not available in the academic literature, to my knowledge.

The next section quotes from the 1988 letter and illuminates Friedman's views on methodology in general. Following that is a section that repeats a portion of my paper which is highly critical of Friedman's methodology. And the final section contains Friedman's reaction to my argument and a brief summary discussion.

Friedman's Views

> … I wrote a paper on methodology in economics some thirty-odd years ago of which I am sending you a copy under separate cover. Hardly any paper I have ever written has given rise to as much controversy and further publication by others. I have consistently kept out of the argument and allowed it to proceed on its own for the very simple reason that I concluded, after writing the article, that I was more usefully employed doing economics than discussing how to do it. At the same time, in my more than three decades of experience as an empirical scientist since that date, I have seen no reason to alter my fundamental view about methodology.
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> … I believe in making use of all kinds of information, but I believe it is a mistake to believe that actual empirical work in economics has been one way, from theory to "facts," rather than a back-and-forth approach in which empirical evidence, whether experimental or observational, has tended to lead to a revision of theory and a revision of theory in turn to new ideas of what evidence to look for. Indeed, it has long been one of my beliefs that the only distinction between "theory" and "fact" is that theories are those "facts" that we take for granted and accept tentatively as valid for the purpose at hand.
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> One final word. I have always believed that those who insisted that there was a fundamental difference between scientific method in the physical sciences and in the social sciences were in error, but they were in error not because of what they believed about the social sciences but because of their beliefs about the physical sciences.

My Criticism of the Methodology of Positive Economics

The final section of my 1991 paper is as follows:

> Milton Friedman's (1953) "as if" methodology of positive economics represents a more limited context for theory improvement than the transactional view.
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> Viewed as a body of substantive hypotheses, theory is to be judged by its predictive power for the class of phenomena which it is intended to "explain" . . . The only relevant test of the *validity* of a hypothesis is comparison of its predictions with experience. (p. 8)
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> It can be argued that in many instances of complex economic phenomena, much can be learned by organizing and studying data as if certain assumptions applied. This learning opportunity need not be abandoned, even when particular assumptions are proved to be inaccurate on close scrutiny. The difficulty lies in generalizing Friedman's approach as a preferred methodology for economic theory improvement. Practitioners of Friedman's positive economics all too easily construct theory by transforming strongly held beliefs from their assumptive worlds into unrealistic assumptions. They contend that their theories should be viewed as descriptions as if the assumptions were true. The argument is then made that prediction is the ultimate proving ground and, consequently, criticism of assumptions is not relevant. A skeptical attitude toward particular assumptions is labeled as a misguided attempt to test the realism of assumptions. In addition, any criticism that the selection of assumptions may erroneously fix, at an early stage, the formulation of the problem is presumably deflected by Friedman's qualifying phrase "for the class of phenomena which it is intended to 'explain.'" The practical result, however, may often be to severely restrict both potential reformulations of the problem and the process of feedback-theory improvement.
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> Consider the following theory's superb record for prediction about when water will freeze or boil. The theory postulates that water behaves *as if* there were a water devil who gets angry at 32 degrees and 212 degrees Fahrenheit and alters the chemical state accordingly to ice or to steam. In a superficial sense, the water-devil theory is successful for the immediate problem at hand. But the molecular insight that water is comprised of two molecules of hydrogen and one molecule of oxygen not only led to predictive success, but also led to "better problems" (i.e., the growth of modern chemistry).
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> The transactional approach strives for theory improvement that not only improves predictive accuracy, but also nurtures further insights as to if and how variables are apparently related to the phenomena under inquiry. Strict adherence to Friedman's myopic goal of prediction can be counterproductive, because predictive accuracy may well be restricted to only a highly limited range of relevant experiential needs while hindering what should be the scientist's healthy skepticism. If economists give more attention to transactional epistemology, economic theorizing should improve.

Friedman's Response

In his 1990 letter, Friedman wrote, "… I have read your final section, I have no quarrel with it, and it has no quarrel with me, so let us leave it at that."

I take that to mean that Friedman accepted my criticism that researchers can easily misuse his methodology. In addition, he believes that his own attention to empirical research has adequately dealt with the issue of disregarding the realism of assumptions.

I continue to believe that a primary, and harmful, result of the methodology of positive economics has been to provide mathematically inclined researchers "cover" in constructing mathematically elegant theories based on unrealistic assumptions that, in fact, stymie more insightful inquiry. This situation was aptly summarized by Ronald Coase as follows:

> Economics has been becoming more and more abstract, less and less related to what goes on in the real world. In fact, economists have devoted themselves to studying imaginary systems, and they don't distinguish between the imaginary system and the real world. That's what modern economics has been and continues to be. All the prestige goes to people who produce the most abstract results about an economic system that doesn't exist.

My own research in finance has butted against the strong hold of a theory most assuredly built on unrealistic assumptions — the Capital Asset Pricing Model. For a discussion of this topic see pages 192 – 195 of my book, *CFROI Valuation — A Total System Approach to Valuing the Firm.*