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The Importance of Behavioral Assumptions in Economic Theory

INTRODUCTION

Milton Friedman wrote a methodological essay in 1953 that was highly influential in the fields of economics, accounting, and finance. Entitled *The Methodology of Positive Economics*, it is required reading in my philosophy of science seminar along with other classics such as Adam Smith's (1795) *The History of Astronomy*, Karl Popper's (1934) *The Logic of Scientific Discovery*, and Thomas Kuhn's (1962) *The Structure of Scientific Revolutions*. I find Friedman's essay worthy of study for new doctoral students because it is the most heavily cited methodological work in economics, and arguably the most controversial (Mäki 2009a). Importantly, the essay raises a rich array of theoretical issues and helps me explain economic theory-building within its historical and social context. Friedman became a prominent economist at the University of Chicago at about the same time that academics and administrators joined a major initiative to make American business education "scientific" (see Chapter 3). Thus, the influence of his methodological essay quickly spread from economics to economics-based research in accounting and finance. Accounting researchers were so influenced by Friedman's essay that they named their application of his research methodology *positive accounting theory* (Watts and Zimmerman 1986).[1]

Some researchers view economics as a scientific success thanks to its adherence to the methodology outlined in Friedman's essay. Others, however, view Friedman's methodology as "deeply flawed, even dangerous for

[1] My own view of Friedman's methodological essay began to change after discussions with experimental economists at Florida State University during my time on the faculty there. I thank David Cooper, Mark Isaac, and Tim Salmon for helping rid me of the simplistic view of Friedman's essay frequently held in accounting and finance.

the cognitive aspirations and social responsibilities of economics" (Mäki 2009a, 47). To be sure, Friedman's essay shaped economists' conceptions of what constituted good theory and good empirical evidence. After his essay, representative research in the top economic journals became "more *formal* – more mathematical, more analytical, less historical, less institutional, more standardized, and more narrow regarding admissible priors" (Hands 2009, 145, italics in the original). At the time of Friedman's essay, the theory of the firm was "an imperiled embryo" with multiple forces pulling it in different directions (Reder 2009). Friedman's essay narrowed the discussion space and pushed many formerly prominent topics to the periphery (Backhouse 2009, Williamson 2009). Consistent with Kuhn's (1962) characterization of scientific advancement, Friedman's essay helped the theory of the firm develop into a focused research paradigm. Thus, any book that proposes to introduce social norms into the theory of the firm must address the theoretical and methodological issues raised in Friedman's essay.

In this chapter I use Friedman's essay to stress the importance of behavioral assumptions in economic theory. Some may find this surprising given that Friedman has been credited with taking the opposite position in his essay. It is my view that Friedman's essay has largely been misunderstood in this regard, to the detriment of the theory of the firm. Thus, I begin by summarizing Friedman's theoretical and methodological arguments with the goal of yielding the most straightforward interpretation. For this task, I borrow insights from other classic works in the philosophy of science (Smith 1795, Popper 1934, Kuhn 1962). Similar to Mäki (2009b), I find ample evidence in Friedman's statements and subsequent behavior to support the opposite conclusion for which he is so often credited. In particular, I find ample support in his essay for the importance of behavioral assumptions in economic theory and a cost/benefit approach to the realism of such assumptions. I follow this summary of Friedman's essay with a brief cost/benefit analysis of incorporating social norms into the theory of the firm. I conclude by introducing the topics covered in the remaining chapters of this book.

FRIEDMAN'S METHODOLOGY OF POSITIVE ECONOMICS

Milton Friedman, like many economists of his generation, was attracted to the field of economics because of the suffering he witnessed during the Great Depression (Snowdon and Vane 1999). After completing his graduate studies at the University of Chicago in 1935, Friedman was initially unable to find academic employment. Thus, he found himself in

Washington, DC, working at the National Resources Committee (NRC). While at the NRC, Friedman was part of a task force assigned to providing a statistical answer to the debate over the causes of the Great Depression (Serrano and Bonilla 2009). Roosevelt's New Deal policies required government intervention in the US economy on a scale never seen before, and statistical analysis at the NRC was used to justify such intervention. In addition to giving Friedman valuable experience in data analysis, his experience at the NRC allowed him to witness first-hand how economic analysis could be used to serve a normative, political purpose. In 1941, Friedman was appointed as the principal economist at the Treasury's division of tax research, where he took direct part in policy-making debates. In 1946, Friedman completed his PhD from Columbia University and joined the faculty at the University of Chicago where his views of government intervention soon changed from being thoroughly Keynesian to laissez-faire (Friedman and Friedman 1998).

By the time his methodological essay was published in 1953, therefore, Friedman had overcome his modest beginnings and had gained extensive experience in public policy and data analysis. In particular, he had amassed an impressive résumé of policy-related research as a member of the NRC task force researching the influence of consumer behavior on the Great Depression and later as the principal economist at the Treasury's division of tax research. Friedman was part of a small cohort of US economists who had been attracted to the profession because of the Great Depression and had spent time in Washington shaping public policy to help fight it. This background gave Friedman a unique perspective regarding theoretical arguments for government intervention in the economy and the data used to justify such intervention. It also made him well qualified to write an essay about research methodology and the foundations of economic theory.

The chief aim of Friedman's essay was to promote a more scientifically based research methodology in economics. The 1940s had witnessed an explosion of advances in science and mathematics, and these advances were changing the way economics was practiced. John von Neumann and Oskar Morgenstern had published their *Theory of Games and Economic Behavior* in 1944 and Paul Samuelson had published his *Foundations of Economic Analysis* in 1947. The Cowles Commission, founded in Colorado in 1932, had moved to the University of Chicago in 1939 to make Chicago a mecca for mathematical economics. Cowles sponsored conferences and published papers deepening the rigor of economic theory in conference monographs and a new academic journal called *Econometrica*. While the

theory of the firm was becoming more mathematically rigorous, however, it was also becoming narrower. Thus, other forces pushed back against this trend in neoclassical economics. Prominent economists of the day criticized the simplified behavioral assumptions underlying the theory of the firm, calling for greater realism (Gordon 1948). Further, developments in psychology led some to conclude that the theory was outmoded and needed to be reconstructed in line with such developments (Friedman 1953, 30).[2]

Friedman's essay made some of the same arguments as Popper (1934) regarding what constitutes a testable hypothesis and how to develop formal tests of theory. In fact, some researchers have credited Friedman with introducing Popper's philosophy of science to economists (Walters 1987). For example, Friedman argued that factual evidence can never "prove" a hypothesis; it can only fail to disprove it. This falsification standard causes researchers to state that a given hypothesis has been "supported" or "confirmed" by the data and never "proven." Friedman also made a distinction between positive theory and normative theory, with the former relating to the scientific quest for "what is" and the latter relating to the often values-driven quest for "what ought to be." As the title of his essay suggests, Friedman promoted the advancement of positive theory in economics as a precursor to good public policy. Given Friedman's background and future role in public policy, however, it is unlikely that he ever advocated a complete retreat from normative theory.[3]

By characterizing the field of economics as a "positive science," Friedman set the boundaries of what constitutes good economics. He identified the ultimate goal of a positive science as "the development of a 'theory' or 'hypothesis' that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed" (Friedman 1953, 7). Friedman also identified two elements of such a theory: (1) it provides a language that is designed to promote systematic and organized methods of reasoning, and (2) it provides a body of substantive hypotheses designed to abstract essential features of complex reality. Friedman argued that as a language, the function of a theory "is to serve as a filing system for

[2] This movement eventually led to Cyert and March's (1963) behavioral theory of the firm. Cyert and March applied the concept of bounded rationality from psychology to propose that real firms aimed at satisficing rather than maximizing their results.

[3] As I discuss below, some researchers in accounting and finance used Friedman's (1953) essay on positive theory to justify retreating from normative theory relevant to public policy.

organizing empirical material and facilitating our understanding of it" (Friedman 1953, 7). As a positive science, therefore, good economics involves the search for theories that are able to explain "what is" and predict what one would expect to find given the right conditions.

While he promoted a more rigorous, scientifically based view of economic theory, Friedman readily acknowledged the role of subjective beliefs and tastes in theory development. As a language, theory defines the discussion space and helps determine what researchers in a field perceive as "acceptable." Friedman (1953, 29–30) acknowledged that the "single-minded pursuit of pecuniary self-interest" is a behavioral assumption that is more acceptable to a trained economist than to a sociologist, and repeated the popular view that economics "is a 'dismal' science because it assumes man to be selfish and money-grubbing." Further, he acknowledged the common criticism that economic theory rests on "outmoded psychology," and the desire on the part of some social scientists that the theory "be reconstructed in line with each new development in psychology." Friedman (1953, 31) argued, however, that "criticism of this type is largely beside the point unless supplemented by evidence that a hypothesis differing in one or another of these respects from the theory being criticized yields better predictions for as wide a range of phenomena."

The view that scientific theory is socially constructed was not new at the time of Friedman's essay, nor was it confined to the social sciences. A similar view appears in Adam Smith's account of the development of scientific theory in *The History of Astronomy* (Smith 1795). Smith argued that scientific theory is as much a work of the imagination as of empirical evidence.[4] He traced the theory of the heavens from Aristotle to Ptolemy, Copernicus, Galileo, and Newton. In praising Isaac Newton's recent discovery of the principle of gravity and its ability to explain the motions of heavenly bodies, Smith warned of the temptation to characterize Newton's new system as "true." While the new theory was capable of accounting for observed phenomena in terms of a smaller number of principles and could successfully predict future movement, Smith argued that it was still a mere product of the imagination.

Friedman (1953, 12) emphasized the importance of empirical evidence in theoretical work: "Empirical evidence is vital at two different, though closely related, stages: in constructing hypotheses and in testing their

[4] Adam Smith referred to science as "natural philosophy" and to scientists as "philosophers." The labels "science" and "scientists" were not widely used prior to 1839 (Wightman 1982, 13).

validity." Besides suggesting new hypotheses, empirical evidence assures that a given hypothesis explains what it sets out to explain. Friedman acknowledged, however, that empirical evidence itself is not a sufficient criterion for choosing among hypotheses. "If there is one hypothesis that is consistent with the available evidence, there are always an infinite number that are" (Friedman 1953, 9). Because hypothesis choices are underdetermined by the evidence, therefore, there is room for subjective judgments and social constraints in the development of economic theory (Mäki 2009b, 113). Friedman (1953, 23) was pragmatic about this underdetermination and the need for subjective judgment in economic theory: "Yet the continued use and acceptance of the hypothesis over a long period, and the failure of any coherent, self-consistent alternatives to be developed and be widely accepted, is strong indirect testimony to its worth."

Similarly, Adam Smith (1795) spoke of "prejudices" of the imagination in the development of scientific theory. He identified a number of prejudices that generated a resistance to new theoretical developments in astronomy, including prejudices of background and education. This prejudice of the imagination can explain why researchers in different fields prefer different behavioral assumptions in their theory and generally resist new assumptions. This resistance to new theoretical developments is a major theme in Thomas Kuhn's (1962) classic work, *The Structure of Scientific Revolutions*. Kuhn emphasized the role of the scientific community in the development of scientific theory. According to Kuhn, a given community of scholars makes a commitment to a given theory and commits resources and institutions to perpetuate that theory, even to the extent of ignoring inconsistent evidence. Over time, however, evidence inconsistent with the accepted theory begins to mount, perhaps due to advances in measurement and data availability. At some point, this process generates a crisis at which time a new theory can emerge and become the new research paradigm for the community of scholars.[5]

Given the presence of underdetermination in theory development (the fact that multiple theories can explain the same set of data), Friedman provided a list of virtues to guide economic researchers in choosing among theories. The list includes, foremost, simplicity and fruitfulness: "A theory is 'simpler' the less the initial knowledge needed to make a prediction within a given field of phenomena; it is 'fruitful' the more precise the resulting prediction, the wider the area within which the theory yields predictions, and the more additional lines for further research it suggests"

[5] I discuss research paradigms and paradigm shifts more fully in Chapter 7.

(Friedman 1953, 10). Logical completeness and consistency are also listed as important, although they play a subsidiary role to Friedman. Adam Smith (1795) emphasized that one theory may be preferred over another because it is simpler. When it comes to scientific theory, therefore, Smith and Friedman agree that less is more (Occam's razor). But by far the most important virtue, according to Friedman, is the usefulness of the theory for prediction. This is also consistent with Smith, who argued that a theory is satisfactory to the imagination only if it is coherent and capable of accounting for observed appearances.

In an often-quoted passage, however, Friedman (1953, 14) appears to discount the importance of the underlying behavioral assumptions of a theory: "Truly important and significant hypotheses will be found to have 'assumptions' that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions." This statement not only takes the view that the realism of underlying behavioral assumptions is irrelevant; it also turns the unrealism of such assumptions into a theoretical virtue (Mäki 2009b). This statement was controversial from its inception, and it continues to draw strong criticism from researchers in philosophy and economics (Mäki 2009a). Some researchers view Friedman's statement defending the use of unrealistic assumptions as the "weak spot" of his essay and some have even labeled his arguments in this regard as "philosophically amateurish" (Mayer 2009, 122). Other researchers have attributed Friedman's strong statement to the author's tendency to "revel in controversy" or "overreach" (Williamson 2009, 242). So many researchers have found this statement unacceptable and outrageous on its face, that a straightforward interpretation must be wrong.

Friedman's strong statement regarding the virtue of unrealistic assumptions has been interpreted as: "*economic theories should not be judged by their assumptions but by their predictive implications* – and in particular, *the unrealisticness of the assumptions of a theory is no reason for complaint or worry about the theory*" (Mäki 2009b, 93–94, italics in the original). Consistent with this interpretation, Friedman's essay has been used to justify simplifying behavioral assumptions in game theory and mathematical economics. His essay has also been used, however, to beat back attempts to enhance the realism of underlying behavioral assumptions in the theory of the firm. To the extent that this was Friedman's original goal, he has been largely successful. This is particularly true of economics-based research in accounting and finance. Some researchers in accounting and finance have used Friedman's essay not only to keep more realistic

behavioral assumptions out of the theory of the firm, but also to justify retreating altogether from normative theory relevant to public policy. This is highly unfortunate, but it appears to be the outcome of an overly simplistic interpretation of Friedman's positions.

AN ALTERNATIVE INTERPRETATION OF FRIEDMAN'S POSITION ON BEHAVIORAL ASSUMPTIONS

To be sure, Friedman (1953) devoted a large portion of his essay debating the importance of unrealistic assumptions to economic theory. Many of his arguments appear at odds with Adam Smith's (1795) argument that a theory is more pleasing to the imagination if it is stated in terms of assumptions that are at least plausible. To understand why Friedman would devote so much space defending the use of unrealistic assumptions, it is important to recall the environment in which he wrote his essay. Prominent economists at the time were critical of the realism of the assumptions underlying the neoclassical theory of the firm (Gordon 1948). Further, developments in psychology were exposing the theory of the firm to the criticism that it was outmoded and needed to be reconstructed in line with such developments (Friedman 1953, 30). Friedman initially developed his essay as a response to the 1946–1947 marginalist controversy that had broken out between Lester, Machlup, and Stigler, where he questioned Lester's view of theory and Machlup and Stigler's view of empirical evidence (Backhouse 2009). His main target, however, was the emerging theory of monopolistic competition. While objecting to the claim that the theory could prove the basis for a more general theory, Friedman particularly objected to the claim that it represented an improved theory because of the increased realism of its underlying assumptions (Williamson 2009).

These developments posed a potential threat to the neoclassical theory of the firm. As such, Friedman's defense of the use of unrealistic behavioral assumptions can be viewed as a defense of the neoclassical theory of the firm from its detractors. In light of Kuhn's (1962) classic work on the philosophy of science, Friedman's defense is symptomatic of a strong theoretical paradigm. According to Kuhn, normal science functions within a given theoretical paradigm. In particular, normal science involves the laborious process of accumulating detail in accord with the established theory, without questioning or challenging the underlying assumptions of that theory. In essence, Friedman was concerned that critics had gone too far in questioning the underlying assumptions of the established

neoclassical theory of the firm. Rather than question these assumptions, he argued for getting on with the task of accumulating more detail in accord with the established theory. From this perspective, Friedman's essay performed a valuable service in preserving the accepted neoclassical paradigm for future researchers.

Recent scholarship regarding Friedman's (1953) methodological essay, however, suggests that his position regarding the importance of behavioral assumptions has largely been misunderstood. Why would Friedman devote most of his methodological essay to the underlying assumptions of a theory if such assumptions were unimportant? Given this unevenness in treatment, some researchers have argued that the main message of Friedman's essay is really the importance of a theory's assumptions (Mäki 2009b). For example, Friedman attacks Edward Chamberlin's theory of monopolistic competition based on its criticism of the neoclassical theory's assumption of "perfect competition" rather than on its inferior ability to predict. His criticism of Chamberlin's theory conveys the opposite position for which Friedman is credited in his essay. In fact, Friedman's essay and future behavior suggest that the underlying assumptions of a theory really do matter (Mäki 2009b).

One simplifying assumption dominates all others in neoclassical economic theory. This assumption is called "rational expectations" or "rationality." These labels capture the overarching assumption that individuals or firms have a well-defined utility function and attempt to maximize their utility through their decisions and behavior. Economic research, therefore, can be described as the study of rational behavior on the part of individuals, groups, or firms to maximize their utility given a set of information and a world of limited resources. In this regard, there is considerable overlap between the descriptive goal of economic theory (positive theory) and the prescriptive goal of economic theory (normative theory). In particular, the rationality assumption is used by economists both to describe what economic agents *actually do* and to prescribe what they *should do* given a specific economic setting. In developing both positive and normative economic theory, therefore, economists have found it convenient to assume rationality on the part of both individuals and firms. The importance of the rationality assumption to economic theory emphasizes the point that the assumptions of a theory do matter, and very much.

It has been my experience that the underlying assumptions of a theory are very important. Although my research training is in experimental economics, I have joined with theorists to publish a noisy rational

expectations model of the information environment of market analysts (Barron, Kim, Lim, and Stevens 1998) and a principal-agent model demonstrating how incorporating a social norm for promise-keeping improves the descriptive, prescriptive, and pedagogical usefulness of the model (Stevens and Thevaranjan 2010).[6] The review process at top academic journals has convinced me of the importance of underlying assumptions. As any theorist knows, simplifying assumptions are not only critical for mathematical tractability; they also drive the results of an economic model. In addition to providing mathematical proofs, therefore, journal editors and reviewers require theorists to justify the underlying assumptions of their models. This is why theorists typically begin with a model that is already firmly established in the literature and then make small, incremental changes.[7] Thus, while Friedman's theoretical virtues of simplicity and fruitfulness (unifying power) are certainly important to an economic theory, and mathematical elegance is a plus, the realism of a theory's assumptions is also very important.

Melvin Reder (2009, 173) identifies another theoretical virtue to help researchers select among theories: "consilience of a theory with other beliefs, especially those associated with other theories, that have wide acceptance." Researchers have long promoted the merits of enhancing neoclassical economic theory with insights from the other social sciences such as psychology (Cyert and March 1963, Wilson 1998). The incorporation of behavioral assumptions from other successful research paradigms, while potentially fruitful, is commonly resisted due to prejudices of the imagination (Smith 1795) and socialization in a community of researchers (Kuhn 1962). I have experienced this resistance first-hand in my own research in the theory of the firm. In the course of my academic career, I have presented my research before audiences trained in diverse research paradigms from economics, accounting, and finance to psychology and moral philosophy. The prejudices in the various research disciplines are inbred and deep, and include prejudices of methodology as well as behavioral assumptions.

[6] I consider myself neither a mathematician nor a statistician, although I have taken graduate courses in both fields and have taught statistics and research methodology seminars. (I was tempted to write, "but I did sleep in a Holiday Inn Express last night," but thought better of it.)

[7] As Joel Demski once told me, an established model is a theorist's playground. When pressed to justify one of the underlying assumptions of his model at a research workshop, he admitted that his model was just "a toy."