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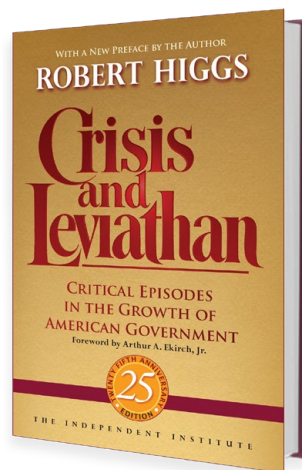
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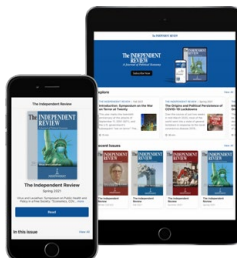
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Milton Friedman,
1912–2006



JULIO H. COLE

Let us now praise famous men.
—Ecclesiasticus 44:1

Milton Friedman, who died in the early morning of November 16, 2006, was a world-famous economist and an ardent and effective advocate of the free-market economy. Much of his celebrity derived from his role as public intellectual, an aspect of his work that was reflected largely in popular books such as *Capitalism and Freedom* (1962) and the hugely successful *Free to Choose* (1980)—both coauthored with his wife, Rose, and the latter based on the television documentary of the same title—and in the *Newsweek* opinion columns he wrote for many years.

Although he was already well known by the time he received the Nobel Prize in Economics in 1976, this award greatly enhanced his stature as a public figure and his effectiveness as a policy advocate, and in the vast outpouring of obituaries and public testimonials prompted by his passing, these aspects of his life and work have received the greatest emphasis.

Most professional economists (and probably Friedman himself), however, would regard another aspect of his career as far more important than his incursions in the policy arena. Indeed, even if “Friedman the public intellectual” had never existed, “Friedman the economic scientist” would still be renowned and respected (though perhaps not as a bona fide world-class celebrity), and his memory will live long in the

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lore of economics. In this article, I focus primarily on this other aspect of his life and work.

Education and Professional Background

Milton Friedman was born in Brooklyn, New York, on July 31, 1912, the youngest child in a family of poor Jewish immigrants from Carpatho-Ruthenia (then in the Hungarian part of Austria-Hungary, now part of independent Ukraine). He received his early schooling in the public schools of Rahway, New Jersey, where he grew up, and in 1928 he obtained a state scholarship to attend Rutgers University, which he entered with the intention of majoring in mathematics (his original career plan was to become an actuary). In college, however, chance intervened, as he said, in the form of “two extraordinary teachers [of economics] who had a major impact on my life”: Homer Jones and Arthur F. Burns (Friedman 2004, 68). Under their influence, he switched majors from mathematics to economics.

Upon graduation from Rutgers in 1932, Friedman received two scholarship offers for graduate study, one to study economics at the University of Chicago, the other to study applied mathematics at Brown University. Both, it seems, were equally attractive: “It was close to a toss of a coin that determined which offer I accepted” (Friedman 2004, 69). In the event, he opted for Chicago and became an economist.

At Chicago, where he earned his master’s degree in 1933, his teachers included Frank Knight, Lloyd Mints, Henry Simons, Henry Schultz, and Jacob Viner.¹ There, he also met two fellow graduate students, W. Allen Wallis and George J. Stigler, who would become life-long friends and colleagues.² His friendship with Stigler was especially significant because the Stigler-Friedman team, more than any other pairing of individuals, eventually defined and personified what became known as the “Chicago school” of economics.³

He went to Columbia University in 1934, where he studied mathematical statistics under Harold Hotelling and economics with Wesley C. Mitchell and John M. Clark. He later returned to Chicago as research assistant to Henry Schultz, who was

1. Friedman had fond recollections of Viner (“[His] course was unquestionably the greatest intellectual experience of my life” [Friedman 2004, 70]), and several generations of Viner’s students have attested to Viner’s qualities as teacher, though he also seems to have been quite fearsome in class. Another great economist recalls: “I had the opportunity to take Jacob Viner’s celebrated course in graduate economic theory—celebrated both for its profundity in analysis and history of thought, but also celebrated for Viner’s ferocious manhandling of students, in which he not only reduced women to tears but on his good days drove returned paratroopers into hysteria and paralysis” (Samuelson 1972, 161).

2. Another classmate was Rose Director, Friedman’s future wife and coauthor. They were married on June 25, 1938.

3. See Friedman’s touching tribute to his friend and colleague (Friedman 1999) and the recently published Friedman-Stigler correspondence (Hammond and Hammond 2006). The Chicago school became a powerhouse in academic economics, and the University of Chicago’s Department of Economics is to date the institution with the largest number of Nobel Prizes in Economics to its credit (see Rowley 1999 for profiles of five Chicago Nobelists).

then working on his massive study of empirical demand curves (Schultz 1938).⁴ From 1937 to 1940, Friedman worked on analysis of income-expenditure surveys at the National Bureau of Economic Research (NBER).

At this point, it is perhaps useful to pause and reflect on this remarkable educational experience. Although it resulted from a seemingly fortuitous combination of circumstances, a program better suited to his future professional development would have been difficult to plan deliberately. At the theoretical level, the Chicago influence was of course decisive, though one of the most important aspects of Friedman's approach to economic research—his careful and detailed analysis of empirical evidence—did not come from Chicago, but from his contact with Wesley Mitchell and the NBER. In fact, the empirical research now regarded as a hallmark of "Chicago" economics owes much to Friedman's later influence. In the 1930s, with the somewhat marginal exceptions of Henry Schultz and Paul Douglas, the Chicago economists emphasized theory more than empirical analysis (Reder 1982).

In the early stages of Friedman's career, however, Hotelling had the strongest influence on him. Indeed, at first Friedman showed more signs of becoming an eminent statistician than a great economist. In one of his first professional publications, he developed a nonparametric technique for the analysis of variance under certain conditions (Friedman 1937). As in most of his analytic contributions, the motivation for the "Friedman test" was to help solve practical problems in the analysis of data (in this case, income and expenditure data).⁵

During World War II, after a brief stint at the Treasury Department, Friedman was a member of the Statistical Research Group (SRG) at Columbia, working on combat problems and quality inspection for war materials.⁶ This group comprised a

4. In the introduction to this volume, Schultz wrote: "In the fall of 1934, when I returned from a year's stay abroad and was faced with the prospect of having to train and build up an entirely new staff of assistants in order to finish the work, Milton Friedman, a former graduate student of mine, came to my rescue and for a year continued to render valuable assistance" (1938, xi). A further and more specific acknowledgment is noted on p. 569 (note 1): "I am profoundly grateful to Mr. Milton Friedman for invaluable assistance in the preparation and writing of these chapters [18 and 19] and for permission to summarize a part of his unpublished paper on indifference curves in Sec. III, chap. xix." The section Schultz was referring to is entitled "The Friedman Modification of the Johnson-Allen Definition of Complementarity" and is based on an unpublished paper by Friedman entitled "The Fitting of Indifference Curves as a Method of Deriving Statistical Demand Curves" (January 1934). This essay must have been Friedman's first technical paper in economics (note that he was twenty-one years old at the time!). It was never published, though it is cited occasionally in the literature on complementarity (see, for instance, Samuelson 1974), and two Japanese scholars have recently developed some implications of Friedman's analysis (Tsumimura and Tsuzuki 1998). I thank Mr. Takashi Yoshida of Keio University for kindly providing me a copy of the Tsumimura-Tsuzuki paper.

5. Though not much used by economists, the test is widely used in other fields. Indeed, it has become so standard in the field of nonparametric statistics that it is often referred to simply as the "Friedman test," without further attribution, so most of the analysts who routinely use it are probably not aware that the creator of this useful test and the famous economist are the same person. See, for instance, Gibbons 1976, 310–17.

6. On the history of the Columbia SRG, see Wallis 1980. Rees 1980 provides a briefer discussion of the material Wallis covers, but sets it in a somewhat broader context. Wallis reports some of the titles of the studies the SRG prepared, one of which is rather chilling: "Relative Effectiveness of Caliber 0.50, Caliber

truly dazzling collection of brilliant statistical minds, and their joint efforts resulted in, inter alia, the development of “sequential analysis,” an important advance in statistical theory. Friedman, together with Allen Wallis and navy captain Garret Schuyler, noticed that the conventional method of taking samples of a predetermined size was inefficient because it did not take into account information generated by the sample process itself. The idea was later rigorously developed by Abraham Wald, who proved the basic theorem underlying sequential testing, which was quickly adopted and adapted as the standard method for inspection sampling.⁷

After the war, Friedman served briefly on the faculty of the University of Minnesota, but in 1946 he returned to the University of Chicago as professor in the Department of Economics, where he remained until his retirement in 1977. The return to Chicago coincided with a major change in the focus of his research activity, which shifted away from pure statistics and eventually centered almost entirely on economics. He was back home.

The Methodology of Positive Economics

Friedman had a profound impact on economic research during his lifetime, and his influence reached far beyond the particular fields he chose for his own research. Much of this influence came from his opinions on methodological issues, which he clarified at an early stage of his career. His famous 1953 essay “The Methodology of Positive Economics” is arguably his best-known work among professional economists, as well as one of the most controversial.

Friedman began his essay by distinguishing between *positive economics*, a “body of systematized knowledge concerning what is,” and *normative economics*, “a body of systematized knowledge discussing criteria of what ought to be” (1953, 3; unless otherwise stated, all page references in parentheses in this section refer to this work). The two disciplines are related, but the conclusions of positive economics are independent of ethical positions or normative judgments. The purpose of positive economics is to “provide a system of generalizations that can be used to make correct predictions about the consequences of any change in circumstances” (4). Economic theories should be evaluated according to strictly empirical criteria: “Viewed as a body of substantive hypotheses, theory is to be judged by its predictive power for the class

0.60, and 20 mm Guns as Armament for Multiple Anti-aircraft Machine Gun Turrets” (August 28, 1945). This report was written by Milton Friedman.

7. See Armitage 1968 for a brief introduction to the literature on sequential analysis. Stigler was also a member of the SRG, though not for as long as Friedman (ten months and thirty-one months, respectively). Stigler’s view of the experience is characteristic of him: “[The SRG] was a pioneer American branch of the new craft called operations research, which applied statistical and economic theory to combat problems and to wartime procurement. . . . Our group had illustrious successes, such as the invention by Wald of a new method of statistical analysis called sequential analysis. That method of quality inspection saved the economy more money per month in the purchase of rocket propellant than the entire wartime cost of our organization. My role in our work was so modest that my claim must be that I did not aid the enemy” (1988, 61–62).

of phenomena which it is intended to ‘explain.’ Only factual evidence can show whether it is ‘right’ or ‘wrong’ or, better, tentatively ‘accepted’ as valid or ‘rejected’” (8). This standard is stressed repeatedly throughout the essay: “[T]he only relevant test of the *validity* of a hypothesis is comparison of its predictions with experience. The hypothesis is rejected if its predictions are contradicted (‘frequently’ or more often than predictions from an alternative hypothesis); it is accepted if its predictions are not contradicted; great confidence is attached to it if it has survived many opportunities for contradiction” (8–9, emphasis in original).

Using language that is now associated with Karl Popper’s ([1934] 1959) philosophy of science, Friedman added that “factual evidence can never ‘prove’ a hypothesis; it can only fail to disprove it, which is what we generally mean when we say, somewhat inexactly, that the hypothesis has been ‘confirmed’ by experience” (9).⁸

To be sure, economic phenomena present special difficulties because controlled experiments, explicitly designed to eliminate complicating factors, are usually impossible to perform. Therefore, “we must rely on evidence cast up by the ‘experiments’ that happen to occur” (10). Friedman held, however, that “the inability to conduct so-called ‘controlled experiments’ does not . . . reflect a basic difference between the social and physical sciences because it is not peculiar to the social sciences—witness astronomy—and because the distinction between a controlled experiment and uncontrolled experience is at best one of degree. No experiment can be completely controlled, and every experience is partly controlled in the sense that some disturbing influences are relatively constant in the course of it” (10).

Furthermore, “evidence cast up by experience is abundant and frequently as conclusive as that from contrived experiments; thus the inability to conduct experiments is not a fundamental obstacle to testing hypotheses by the success of their predictions.” Such evidence, however, is admittedly “far more difficult to interpret. It is frequently complex and always indirect and incomplete. Its collection is often arduous, and its interpretation requires subtle analysis and involved chains of reasoning, which seldom carry real conviction” (10–11). In short, a “crucial” experiment is seldom possible in economics, which hinders adequate hypothesis testing, although

8. Friedman nowhere cites Popper in his essay, which at first glance might seem puzzling, given the similarity of their views in this regard. It seems, however, that by the time of his first meeting with Popper, Friedman had already developed his methodological notions independently: “Shortly after I had completed a first draft [of the 1953 essay], George Stigler and I had long discussions with Karl Popper in 1947 at the founding meeting of the Mont Pelerin Society. The part of those discussions that I remember best had to do with scientific methodology. Popper’s book, *Logik der Forschung*, published in Vienna in 1934, had already become a classic analysis of the methodology of the physical sciences, but my German was too limited for me to have read it even though I may have known about its existence. It was not translated into English until 1959, . . . so these discussions at Mont Pelerin were my first exposure to his views. I found them highly compatible with the views that I had independently come to, though far more sophisticated and more fully developed” (Friedman and Friedman 1998, 215). The Mont Pelerin Society is an international association of scholars founded at a conference organized by F. A. Hayek in 1947 and committed to the preservation and dissemination of the ideals of classical liberalism. (On the history of the Mont Pelerin Society, see Hartwell 1995. Friedman, at age thirty-four, must have been one of the youngest of the thirty-nine founding members. Because he lived a very long life, it is likely that he was the last surviving member of that original group.)

“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” (11). In economics, the process of weeding out failed hypotheses is slower than in other sciences. On occasions, however, casual experience provides evidence just as compelling as the result of a controlled experiment—the empirical correlation between monetary growth and price inflation is a good example.

In Friedman’s approach, the criteria for acceptance or rejection of hypotheses are strictly empirical. Unlike his teacher Wesley Mitchell, however, Friedman was by no means opposed to abstract theory per se. In fact, one of his objectives in the methodology essay was to defend the abstract nature of neoclassical economic theory, which was often criticized for its lack of realistic assumptions. Friedman thought these critiques were misplaced and that scientific hypotheses should not be judged by the realism of their assumptions because they can never be “realistic” in a descriptive sense. In fact,

the relation between the significance of a theory and the “realism” of its “assumptions” is almost the opposite of that suggested by the view under criticism. 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 (in this sense).⁹ The reason is simple. A hypothesis is important if it “explains” much by little, that is, if it abstracts the common and crucial elements from the mass of complex and detailed circumstances surrounding the phenomena to be explained and permits valid predictions on the basis of them alone. (14)

Theoretical assumptions are simplifications of reality, and in this sense they *must* be descriptively false (that is, they take into account only the features regarded as important because the success of the hypothesis shows that all other circumstances are irrelevant to the explanation of the phenomenon). To Friedman, the realism of the assumptions was unimportant, and “the relevant question to ask about the ‘assumptions’ of a theory is not whether they are descriptively ‘realistic,’ for they never are, but whether they are sufficiently good approximations for the purpose in hand,” which can be determined only by “seeing whether the theory works, which means whether it yields sufficiently accurate predictions” (15).

The “Methodology” essay was (and remains) controversial, and it generated a large secondary literature.¹⁰ Friedman, however, having stated his case, preferred to

9. He was quick to add that “the converse of this proposition does not of course hold: assumptions that are unrealistic (in this sense) do not guarantee a significant theory” (14 n.).

10. See Boland 1979 for a good review of the early critical literature. Most economists now probably agree with Mayer that Friedman’s essay is best interpreted as “an attempt to provide practicing economists with some useful ground rules, specifically with a way of healing the unfortunate split between theoretical and

let others argue about it, and he never responded to any of his critics. Instead, he decided to move on and was more concerned with applying his principles in practice.

Monetary Studies

After around 1950, Friedman's interests began to center on monetary economics, the field in which he achieved his greatest prominence. A notable collection of empirical studies edited by Friedman, *Studies in the Quantity Theory of Money* (1956b), was based on doctoral dissertations supervised in his famous Money and Banking Workshop at Chicago. A longer-run project resulted from his association with the NBER, where he took charge of the monetary aspects of a much larger-scale project on business cycles. The detailed investigations related to this project resulted in three volumes coauthored with Anna J. Schwartz: *A Monetary History of the United States, 1867–1960* (1963a), *Monetary Statistics of the United States* (1970), and *Monetary Trends in the United States and the United Kingdom, 1867–1975* (1982).

Friedman laid out the theoretical framework of this empirical research and linked it to previous monetary traditions at Chicago in his introduction to the *Studies* volume, "The Quantity Theory of Money—A Restatement" (1956a).¹¹ He interpreted

empirical economics that prevailed [at the time]. . . . Friedman aimed to provide a useful heuristic for working economists and not a sophisticated philosophical analysis . . . and [his] essay is broadly consistent with the methodology that most economists now affirm, at least in principle" (1993, 213–14). Few working scientists ever pay much attention to what philosophers say about science (or about anything, for that matter), so it is not surprising that criticisms from that corner have never made much of a dent in the essay's appeal, which is not to say that it is above reproach. In fact, it has been subjected to devastating criticism, not from a philosopher, but, fittingly, from an economist (and a *Chicago* economist, no less!): "The view that the worth of a theory is to be judged solely by the extent and accuracy of its predictions seems to me wrong. . . . Except in the most exceptional circumstances, the data required to test the predictions of a new theory . . . will not be available or, if available, will not be in the form required for the tests and . . . will need a good deal of manipulation of one sort or another before they can be made to yield the requisite predictions. And who will be willing to undertake these arduous investigations? . . . [F]or the tests to be worthwhile, someone has to believe in the theory, at least to the extent of believing that it might well be true. . . . If all economists followed Friedman's principles in choosing theories, no economist could be found who believed in a theory until it had been tested, which would have the paradoxical result that no tests would be carried out . . . [so] acceptance of Friedman's methodology would result in the paralysis of scientific activity. Work would certainly continue, but no new theories would emerge" (Coase 1988, 64, 71).

11. In the "Restatement" essay, Friedman stressed the Chicago roots of his approach to the quantity theory, though Patinkin (1969) later criticized him for trying to link his own theoretical contributions to an allegedly quite different "oral tradition" at Chicago. Johnson went further, imputing questionable motives and actually accusing Friedman of "scholarly chicanery" (1971, 11). Friedman responded, "I shall not defend my 'Restatement' as giving the 'flavor of the oral tradition' at Chicago in the sense that the details of my formal structure have precise counterparts in the teachings of Simons and Mints. After all, I am not unwilling to accept some credit for the theoretical analysis in that article. Patinkin has made a real contribution to the history of thought by examining and presenting the detailed theoretical teachings of Simons and Mints, and I have little quarrel with his presentation. But I certainly do defend my 'Restatement' as giving the 'flavor of the oral tradition' at Chicago in what seems to me the much more important sense in which, as I said, the oral tradition 'nurtured the remaining essays in' *Studies in the Quantity Theory of Money*, and [in] my own subsequent work. And, in any event, it is clearly not a tradition that, as Johnson charges, I 'invented' for some noble or nefarious purpose" (Friedman 1972, 941). The Patinkin-Johnson critique provoked considerable scholarly debate involving many authors, but Friedman, as in other cases of controversies motivated by his writings, opted to observe from the sidelines. For a good review of this literature and its background, see the two articles by Leeson (1998, 2000). Don Patinkin was a great

the quantity theory as essentially a theory of the demand for money. Although the monetary authorities might control the *nominal* money supply, what really matters for the public is the *real* money supply (the money supply expressed in terms of its purchasing power). The scientific problem consists in determining the variables that affect the demand for money—the amount of real monetary balances the public holds. According to Friedman, the demand for money is a stable function of real income and the opportunity cost of holding money. Two essays (Friedman 1959; Friedman and Schwartz 1963b) later explored this idea and its implications empirically.

The stability of the demand for money had certain implications concerning effects of variations in the money supply that were inconsistent with the Keynesian analysis that prevailed at the time. A frontal assault on Keynesian theory was made in an extensive empirical study in which Friedman and Meiselman (1963) compared two basic theories: (1) a Keynesian multiplier model, relating national income to “autonomous” expenditures (investment, government spending, and net exports), and (2) a “monetarist” model (the term *monetarist* had not yet been invented), relating income to the money supply via the velocity of money. The results showed that in practice the money supply had much greater explanatory power than autonomous expenditures. The Friedman-Meiselman study set off the “Keynesian-monetarist” debate that came to dominate discussions of macroeconomic policy for many years.¹²

The main conclusions from this and later “monetarist” studies were that: (1) though increased public spending has an effect on nominal income, it soon “fizzles out,” whereas an increase in the money supply has a permanent effect; (2) the adjustment of nominal income to an increased rate of monetary growth involves “long and variable lags”; (3) in the long run, an increase in the rate of monetary growth affects only the inflation rate and has no effect on real output; and (4) in the short run, variations in the rate of monetary growth can have devastating effects on both prices and real output (the most notorious example being the “Great Contraction” of 1929–33, as explained in chapter 7 of *A Monetary History* [Friedman and Schwartz 1963a]).¹³

The Economist as Public Intellectual

Shortly after receiving his Nobel Prize, Friedman retired from the University of Chicago, and the Friedmans moved to San Francisco, where Milton established an association with the Hoover Institution at Stanford University that lasted for the rest of his life. Though he remained active in economic research for years after his retire-

scholar and intellectual historian, and he seems to have felt very strongly about this matter, though in retrospect it is hard to understand what the fuss was all about, and the whole episode seems rather bizarre.

12. Although Friedman was critical of “Keynesian economics,” he always expressed great respect and admiration for Keynes the economist. See, for instance, Friedman 1997.

13. For two brief and relatively nontechnical summaries of his monetary studies, see Friedman 1968 and 1970.

ment from Chicago, most of his original scientific work had been done, and his interests shifted increasingly toward popular writing and involvement in public-policy issues.

He was already well known among the broader public as a staunch critic of government intrusions in the economy and as an exponent of the virtues of an unhampered free market, having expressed such views in *Capitalism and Freedom* (Friedman and Friedman 1962) and in the triweekly *Newsweek* columns he wrote from 1966 to 1984. His leap to celebrity, however, came with the filming of the television documentary series *Free to Choose* and with the publication of a book of the same title (Friedman and Friedman 1980), which eventually became a worldwide best-seller.¹⁴ His general ideas regarding capitalism and the market economy are well known and need no elaboration here. Let me briefly explore, however, some of the reasons for his remarkable success in spreading his ideas to the broader public.

Friedman succeeded as a communicator at least in part because his rhetorical style was much less ideologically colored than that of other free-market advocates. Although he had a strong ideological commitment to values such as personal liberty and individual responsibility, his arguments on specific policy issues tended to stress practical matters, such as economic efficiency and how government interventions often led to consequences worse than the evils they ostensibly sought to remedy. This approach allowed many people to agree with him on specific issues even though they might not accept his entire social philosophy. A related rhetorical feature is what we might call his “incremental” approach to the ideal of a free-market economy. Many policy issues are not matters of “all or nothing,” but of “more or less,” and Friedman was often willing to settle for a compromise solution if it offered a clear possibility of moving closer to the free-market ideal.

A good example is his active role in the movement that eventually ended the military draft in the United States. This issue was not an abstract matter of “capitalism, take it or leave it,” but a specific policy with enormous implications for the personal liberty of millions of flesh-and-blood individuals. It was also an issue that, in the midst of an unpopular war, could enlist the support of many people across the political spectrum.¹⁵

14. On the impact of *Free to Choose*, see the papers collected in the Dallas Fed festschrift (Wynne, Rosenblum, and Formaini 2004) and especially the paper by Boettke (2004).

15. For an early statement of his views in this regard, see Friedman 1967. On the role Friedman and many other prominent economists played in the 1969 President’s Advisory Commission on an All-Volunteer Force (also known as the Gates Commission) and other initiatives that eventually resulted in the ending of the draft, see Henderson 2005. (This paper should be required reading for every American young man on his eighteenth birthday. It is available online at <http://www.econjournalwatch.org>.) An interesting anecdote related to the Gates Commission hearings is worth retelling. Among economists, Friedman had a reputation as the best stand-up debater in the profession. General William Westmoreland, formerly the commander of U.S. forces in Vietnam, discovered this debating ability the hard way: “Like almost all military men who testified, [Westmoreland] testified against a volunteer armed force. In the course of his testimony, he made the statement that he did not want to command an army of mercenaries. I stopped him and said, ‘General, would you rather command an army of slaves?’ He drew himself up and said, ‘I don’t

Another example is his school-voucher proposal, elaborated in *Capitalism and Freedom* and based on an earlier paper (Friedman 1955). Under this system, the government would ideally no longer be involved in the administration of educational institutions, though it would still be involved in financing education. The voucher proposal was thus not a pure free-market solution, but it was clearly a step in the right direction from Friedman's point of view.¹⁶ Separation of government financing of education and government operation of schools, he argued, would give parents at all income levels greater freedom in choosing the schools their children were to attend. Moreover, one does not have to accept Friedman's ultimate vision of a purely private market in education in order to appreciate the efficiency and welfare-enhancing features of the "intermediate" voucher solution: more choice would entail greater competition and hence greater efficiency in school provision.¹⁷

Finally, another likely factor that explains Friedman's greater success in spreading his ideas, especially among professional economists, is that he (and Chicago economists in general) used essentially the same language as most mainstream economists. Indeed, as Israel Kirzner noted many years ago,

The price theory that underlies the contributions of the "Chicago" writers is not fundamentally different from that accepted by American economists generally, including those holding the efficiency and justice of the market system in deep mistrust. It is merely that the "Chicago" economists apply their price theory more consistently and more resolutely, assigning to it a scope of relevance far wider than that granted by others. . . . "Chicago" price theory, like that taught in most United States economics departments, is solidly in the Anglo-American neoclassical tradition associated most importantly with Alfred Marshall. (1967, 102)

To use a bit of economic jargon, one might say that Friedman had a comparative advantage in communicating with mainstream economists, as compared to other leading classical-liberal economists, such as Ludwig von Mises and F. A. Hayek, whose Austrian school background was much more alien to other members of the profession.

like to hear our patriotic draftees referred to as slaves.' I replied, 'I don't like to hear our patriotic volunteers referred to as mercenaries.' But I went on to say, 'If they are mercenaries, then I, sir, am a mercenary professor, and you, sir, are a mercenary general; we are served by mercenary physicians, we use a mercenary lawyer, and we get our meat from a mercenary butcher'" (Friedman and Friedman 1998, 380). Whether the general had anything else to say after he picked his head up from the floor is not reported. In any case, "that was the last we heard from [him] about mercenaries" (380).

16. "Murray [Rothbard] used to call me a statist because I was willing to have government money involved. But I see the voucher as a step in moving away from a government system to a private system" (from the interview in Doherty 1995, 36). For Rothbard's critique, see Rothbard [1971] 2002.

17. On the progress of the voucher idea in the half-century since Friedman's initial proposal, see Enlow and Ealy 2006. The "choice in schooling" issue was near and dear to Friedman's heart, and in 1996 he and Rose established the Milton and Rose D. Friedman Foundation, whose sole purpose is expanding the range of options for parental choice in education.

Of course, the foregoing observations are my personal impressions and conjectures, and I may be wrong in my interpretation of the reasons for Friedman's phenomenal success as social critic and policy advocate. Whatever the reasons, however, the fact itself is indisputable.

A Personal Reminiscence

In the foreword of a collection of essays honoring Milton and Rose Friedman, Alan Greenspan wrote recently: "My first contact with Milton was in 1959, when I mailed him a copy of an article on the impact of the ratio of stock prices to replacement cost on capital investment. I am sure he had never heard of me, yet he took the time to reply with several very thoughtful suggestions. I have never forgotten that" (2004, xii). This remembrance was not the first I had encountered along the same lines, and I do not think these reports describe isolated cases. In fact, Greenspan's experience reflects an important aspect of Friedman's personality: he was very generous with his time, even to complete strangers, as I can personally attest.

I, too, once maintained a correspondence with Milton Friedman. The first time I wrote him was to comment on one of his *Newsweek* articles. At that time, I lived in Bolivia and worked at a sugar mill. Of course, I did not expect him to reply. Why would he write to a perfect stranger? Imagine, then, my surprise when I received a polite and detailed letter from him. I answered it, and he answered back, and our correspondence continued for several years.

I even got the chance to meet him in person, soon after I began my career as a university professor. At the time, I was translating some of his monetary studies. He was interested in the project and encouraged me in his letters (no e-mail then). Because I was consulting him constantly about many minor details, he suggested at one point that maybe I should visit him, so we could sit down for a whole day with the materials and resolve all my queries. After we agreed on a date, I traveled to San Francisco, and we met in his office at Stanford University.

Our meeting was very productive, but I soon realized that although its ostensible purpose was to discuss his papers, he was not really much interested in talking about his work. Rather, he seemed much more interested in my own projects and concerns. What courses was I teaching? Had I published any papers? What other things was I working on? It so happens that I was then working on a book of my own, my first book, on Latin American inflation, and, as I recall, we actually spent more time that day talking about my book than about his own writings. At midday, he invited me to lunch at the faculty club, where we were joined by George Stigler, and in the afternoon we continued to talk until I had to leave for my return flight. I will always remember his gracious generosity, his encouragement, and his willingness to devote part of his valuable time to a young, budding academic. His kindness meant the world to me.

Milton Friedman was a great economist and a fine man. He lived a long and productive life. May he rest in peace.

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