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**Milton Friedman (1912 – 2006)**

*por*  
*Julio H. Cole*

## MILTON FRIEDMAN (1912 – 2006)

by Julio H. Cole<sup>1</sup>

*“Let us now praise famous men, ...”*  
(Ecclesiasticus, 44:1)

### Abstract

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 co-authored 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.

Though he was already well-known by the time he received the Nobel Prize in Economics, in 1976, both his stature as public figure and his effectiveness as policy advocate were greatly enhanced by that award, and this is what has been mostly stressed in the vast outpouring of obituaries and public testimonials prompted by his recent passing.

It is important to recall, however, that there was another aspect of his career, one which most professional economists (and probably Friedman himself) would regard 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 lore of economics.

It is primarily this other aspect of his life and work that I wish to focus on in this essay.

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## 1. 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, nowadays a part of independent Ukraine).

His early schooling was in the public school system 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 eventually become an actuary). In college, however, chance intervened, as he puts it, 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, p. 68). Under their influence, he switched majors from mathematics to economics.

Upon graduation from Rutgers in 1932, in the middle of the Great Depression, 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, p. 69). In the event, he opted for Chicago, and became an economist.<sup>2</sup>

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.<sup>3</sup> There, he also met two fellow graduate students, W. Allen Wallis and George J. Stigler, who would become life-long friends and colleagues.<sup>4</sup> His friendship with Stigler was especially significant, as the Stigler-Friedman team, more than any other pairing of individuals, would eventually define and personify what came to be known as the “Chicago School” of economics.<sup>5</sup>

The following year he went to Columbia University, where he studied mathematical statistics under Harold Hotelling, and economics with Wesley C. Mitchell and John M. Clark. In 1935, Friedman returned to Chicago as research assistant to Henry Schultz, who was then working on his massive study of empirical demand curves.<sup>6</sup> From 1937 to 1940 he worked on

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<sup>2</sup> Whether he would have had an equally illustrious career had he opted for mathematics at Brown is anybody’s guess.

<sup>3</sup> Friedman had fond recollections of Viner—“[His] course was unquestionably the greatest intellectual experience of my life” (Friedman, 2004, p. 70)—and several generations of Viner’s students have attested to his 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, p. 161).

<sup>4</sup> Another classmate was Rose Director, his future wife and co-author. They were married on June 25, 1938.

<sup>5</sup> Stigler, a great economist in his own right (he got his own Nobel Prize in 1982), also possessed an inimitable and delightful sense of humor (delightful, that is, provided one was not at the receiving end). For a generous sampling of Stiglerian wit see his intellectual autobiography (Stigler, 1988), snippets of which will be quoted in passing throughout this essay. See also Friedman’s touching tribute to his friend and colleague (Friedman, 1999), and the recently published Friedman-Stigler correspondence (Hammond and Hammond, 2006).

<sup>6</sup> Schultz (1938). In the introduction to this volume, Schultz wrote the following: “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” (p. xi). A further, and more specific, acknowledgement 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 [i.e., 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” (Jan 1934). This must have been Friedman’s first technical paper in economics (note that he was 21 years old at the time!). It was never published,

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. Though it resulted from a seemingly fortuitous combination of circumstances, it would have been very hard to deliberately plan a program better suited to his future professional development. 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, though empirical research is now regarded as a hallmark of "Chicago" economics, this is at least partly due to the later influence of Friedman himself. In the 1930's, and with the somewhat marginal exceptions of Henry Schultz and Paul Douglas, at Chicago the emphasis was more on theory than on empirical analysis (Reder, 1982).

In the early stages of his career, however, the most important influence was that of Hotelling. Indeed, at first Friedman showed more signs of becoming an eminent statistician than a great economist. One of his first professional publications developed a non-parametric technique for the analysis of variance under certain conditions (Friedman, 1937). As in the case of most of his analytic contributions, the motivation for the "Friedman test" was to facilitate the solution of practical problems posed by data analysis (in this case, income and expenditure data).<sup>7</sup>

During World War II, and after a brief stint at the Treasury Department, Friedman was a member of the "Statistical Research Group" at Columbia, working on combat problems and quality inspection for war materials.<sup>8</sup> This group comprised a truly dazzling collection of brilliant statistical minds, and their joint efforts would result, *inter alia*, in the development of "sequential analysis," a very important advance in statistical theory. Essentially, Friedman, together with Allen Wallis and a Navy captain (Garret Schuyler), noticed that the conventional method of taking samples of a predetermined size was inefficient, since 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.<sup>9</sup>

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though it is occasionally cited in the literature on complementarity (see, for instance, Samuelson, 1974), and two Japanese scholars have recently developed some implications of Friedman's analysis (Tsujimura and Tsuzuki, 1998). (I would like to thank Mr. Takashi Yoshida, of Keio University, for kindly providing me a copy of the Tsujimura-Tsuzuki paper.)

<sup>7</sup> Though not much used by economists, it is widely used in other fields. Indeed, it has become so standard in the field of non-parametric statistics that it is often referred to as simply the "Friedman test," without further attribution, and thus most of the people who use it routinely are probably not aware that the creator of this useful test and the famous economist are in fact the same person. See, for instance, Gibbons (1976), pp. 310-17.

<sup>8</sup> On the history of the Columbia-SRG see Wallis (1980). See also Rees (1980), who provides a briefer discussion of the material covered by Wallis, but set in a somewhat broader context. Wallis reports some of the titles of the studies prepared by the SRG. One particular title is rather chilling: "Relative Effectiveness of Caliber 0.50, Caliber 0.60, and 20 mm Guns as Armament for Multiple Anti-Aircraft Machine Gun Turrets" (Aug 28, 1945). This report was written by Milton Friedman.

<sup>9</sup> See Armitage (1968) for a brief introduction to the literature on sequential analysis. Stigler was also a member of the SRG, though not as long as Friedman (10 months and 31 months, respectively). Stigler's take on the experience is characteristic: "[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

After the war, Friedman served briefly on the faculty of the University of Minnesota, and in 1946 he returned to the University of Chicago as professor in the Department of Economics, where he stayed 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.

## 2. 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 was due to his opinions on methodological issues, which were clarified at an early stage of his career. A famous 1953 essay on “The Methodology of Positive Economics” is arguably his most well-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” (Friedman, 1953, p. 3; unless otherwise stated, all page references in parentheses in this section are to this work). Both disciplines are related, though 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” (p. 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 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’” (p. 8). This is stressed repeatedly throughout the essay:

*... the 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 (pp. 8-9, italics in the original).*

Using language that is nowadays associated with Karl Popper’s philosophy of science (Popper, 1959 [1934]), 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” (p. 9).<sup>10</sup>

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enemy” (Stigler, 1988, pp. 61-62).

<sup>10</sup> 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, p. 215). The Mont Pelerin Society is an international association of scholars, founded at a conference in 1947 organized by F. A. Hayek, and committed to the

To be sure, the nature of economic phenomena presents special difficulties, since it is usually impossible to perform controlled experiments, explicitly designed to eliminate complicating factors. Therefore, “we must rely on evidence cast up by the ‘experiments’ that happen to occur” (p. 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 both 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” (p. 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” (p. 10). On the other hand, such evidence 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” (pp. 10-11). In short, the “crucial” experiment is seldom possible in economics, which hinders adequate hypothesis testing, though “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” (p. 11). The process of weeding-out failed hypotheses is slower than in other sciences. On occasions, however, casual experience provides evidence that is just as dramatic as any 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 thus 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 this essay was precisely to defend the abstract nature of neo-classical economic theory, which was often criticized due to 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, since these 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).<sup>11</sup> 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 (p. 14).*

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preservation and dissemination of the ideals of classical liberalism. (On the history of the Mont Pelerin Society see Hartwell, 1995.) Incidentally, Friedman, at age 34, must have been one of the youngest of the thirty-nine founding members. Since he lived a very long life, it is likely then that he was the last surviving member of that original group. The irrepressible Stigler had this to say about the 1947 conference: “The popularity of Hayek’s book [*The Road to Serfdom*, 1944] led a conservative Midwest foundation, ... , to contribute to the support of a meeting he called in Switzerland for ten days in the spring of [1947]. I had never met Hayek but my Chicago teachers certified my eligibility for the coming totalitarian firing squads. It showed my lack of inner conviction of the imminence of totalitarianism that the thought never entered my mind” (Stigler, 1988, p. 142).

<sup>11</sup> 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” (p. 14n).

Theoretical assumptions are simplifications of reality, and in this sense they *must* be descriptively false (i.e., they take into account only the features regarded as important, since 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” (p. 15).

The “Methodology” essay was (and still remains) quite controversial, and it generated a large secondary literature.<sup>12</sup> Friedman, however, having stated his case, preferred to let others argue about it, and never responded to any of his critics. Instead, he decided to move on, and was more concerned with applying his principles in practice.

### 3. Monetary Studies

Since about 1950 his interests began to center on monetary economics, and in this field he achieved his greatest prominence. A notable collection of empirical studies edited by Friedman (*Studies in the Quantity Theory of Money*, 1956) 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 co-authored with Anna J. Schwartz: *A Monetary History of the United States* (1963), *Monetary Statistics of the United States* (1970), and *Monetary Trends in the United States and the United Kingdom* (1982).

The theoretical framework underlying this empirical research, and the link to previous monetary traditions at Chicago, was Friedman’s introduction to the *Studies* volume: “The Quantity Theory of Money—A Restatement” (Friedman, 1956).<sup>13</sup> Friedman interpreted the

<sup>12</sup> See Boland (1979) for a good review of the early critical literature. Most economists nowadays would 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 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” (Mayer, 1993, pp. 213-14). Very few working scientists ever pay much attention to what philosophers have to say about science (or about any other issue, for that matter), so it is not surprising that criticisms from that corner have never made much of a dent in this essay’s appeal, which is not to say it is above reproach. In fact, it has been subjected to devastating criticism, and 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, pp. 64, 71). Though I am myself a convinced Friedman-type positivist, I must admit that I do not know how to respond to this critique.

<sup>13</sup> 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 (1971) went further, imputing questionable motives and actually accusing Friedman of “scholarly chicanery” (p. 11). Friedman responded that “I shall not defend my ‘Restatement’ as giving the ‘flavor of the



Quantity Theory as, essentially, a theory of the demand for money. Though 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 affecting the demand for money, i.e., the amount of real monetary balances held by the public. According to Friedman, the demand for money is a stable function of real income and the opportunity cost of holding money. This idea and its implications were later explored empirically in Friedman (1959) and Friedman and Schwartz (1963a).

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. The frontal assault on Keynesian theory appeared in an extensive empirical study (Friedman and Meiselman, 1963) which 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 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 far 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.<sup>14</sup>

The main conclusions from this and later “monetarist” studies were that: (1) though increased public spending has an impact-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; (4) in the short run, however, variations in the rate of monetary growth can have devastating effects, both on prices and on real output (the most notorious example being the “Great Contraction” of 1931-33, as explained in Chapter 7 of the *Monetary History* volume).<sup>15</sup>

#### 4. 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 he became associated for the rest of his life with the Hoover Institution, at Stanford University. Though he remained active in economic

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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 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, p. 941). The Patinkin-Johnson critique provoked considerable scholarly debate involving many authors, but, as in other cases of controversies motivated by his writings, Friedman opted to observe from the sidelines. For a good review of this literature and its background see the two articles by Leeson (1998, 2000). Patinkin was a great 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.

<sup>14</sup> In this context, it is perhaps worth mentioning that, though Friedman was quite critical of “Keynesian economics,” he always expressed great respect and admiration for Keynes the economist. See, for instance, Friedman (1997).

<sup>15</sup> For two brief and relatively non-technical summaries of his monetary studies, see Friedman (1968, 1970).

research for some years after retirement, most of his original scientific work had been done, and his interests shifted increasingly towards 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 an exponent of the virtues of an unhampered free market, views which he had expressed in *Capitalism and Freedom* (1962), and in the tri-weekly *Newsweek* columns he wrote from 1966 to 1984. His leap to celebrity, however, came with the filming of the TV documentary series “Free to Choose,” and the publication of a book with the same title (Friedman and Friedman, 1980) which eventually became a world-wide bestseller.<sup>16</sup>

There is no point in elaborating here on his general ideas regarding capitalism and the market economy, since these are well known. Rather, I will attempt to briefly explore some of the reasons for his remarkable success in spreading his ideas to the broader public.

Part of his success as a communicator was probably due to the fact that his rhetorical style was much less ideologically-driven than that of other free-market advocates. Though he himself 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 that were worse than the evils they sought to avoid. This approach allowed many people to agree with him on specific issues, even though they might not coincide with his entire social philosophy. Related to this is what we might call his “incremental” approach to the ideal of a free-market economy. Many policy issues are not a matter of “all or nothing” but of “more or less,” and Friedman was often quite 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 which eventually ended the military draft in the United States. This was not an abstract question of “capitalism, take it or leave it,” but a very specific policy issue with enormous implications for the personal liberty of millions of flesh and blood individuals. It was also an issue that, at the time and in the midst of an unpopular war, could enlist the support of many people across the whole political spectrum.<sup>17</sup>

Another example is his school-voucher proposal, elaborated in Chapter 6 of *Capitalism and Freedom*, and based on an earlier paper (Friedman, 1955). Under this system

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<sup>16</sup> 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).

<sup>17</sup> For an early statement of his views in this regard see Friedman (1967). On the role of Milton Friedman and many other prominent economists 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 male on his 18<sup>th</sup> birthday. It is available online at: [www.econjournalwatch.org](http://www.econjournalwatch.org)). There is an interesting anecdote related to the Gates Commission hearings that is worth retelling. Among economists, Friedman had a reputation as the best stand-up debater in the profession. This was discovered the hard way by General William Westmoreland, formerly commander of U.S. forces in Vietnam: 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 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, p. 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.”

the government would, ideally, no longer be involved in the actual administration of educational institutions, though it would still be involved in the financing of education, so it is not a purely free-market solution. On the other hand, it was clearly a “step in the right direction,” from Friedman’s point of view.<sup>18</sup> By separating government financing of education from government operation of schools, he argued, parents at all income levels would have greater freedom in choosing the schools their children 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 involve greater competition, and hence greater efficiency in school provision.<sup>19</sup>

Finally, another likely factor that explains his greater success in spreading his ideas, especially among professional economists, is that Friedman (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 contribution 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 (Kirzner, 1967, p. 102).*

In this sense, 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.

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

## 5. A PERSONAL REMINISCENCE

A couple of years ago Alan Greenspan prepared a foreword to a collection of essays honoring Milton and Rose Friedman, and in that foreword he wrote the following:

*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 (Greenspan, 2004, p. xii).*

<sup>18</sup> “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” (Doherty, 1995, p. 36). The reference for the Rothbard critique is Rothbard (2002 [1971]).

<sup>19</sup> On the progress of the voucher idea in the half-century since Friedman’s initial proposal, see the papers collected in 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, with the sole purpose of furthering the range of options for parental choice in education.

This was not the first time I had heard or read about similar experiences, and I do not think they are 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, and I can attest to this personally.

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 was working at a sugar mill. Of course, I did not expect him to reply. Why would he? To a perfect stranger? Imagine then my surprise when I got in the mail a very polite and detailed letter in response.

I answered him, and he answered back! And that was the beginning of a correspondence that lasted 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, and it was all by letters—no e-mail then—and since I was constantly consulting him about many minor details, at one point he suggested that maybe I should visit him, so we might sit down for a whole day with the materials and resolve all my queries. Well, I did just that. We agreed on a date, and I traveled to San Francisco and we met in his office at Stanford University.

It was a very productive meeting, although I soon realized that, though the purpose of the meeting was ostensibly to discuss his papers, he wasn't 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 (we were joined by George Stigler), and in the afternoon we talked some more, and kept on talking until it was time for me to catch 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. It meant the world to me.

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

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