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MILTON FRIEDMAN'S EMPIRICAL APPROACH TO ECONOMICS: SEARCHING FOR SCIENTIFIC AUTHORITY WHILE SHAPING THE UNIVERSITY OF CHICAGO ECONOMICS DEPARTMENT

BY
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Milton Friedman is usually presented as an economist characterized by his empirical approach to economics. His binary classification of economics into positive means *and* normative ends *relies on the empirical content of predictions. Throughout his career, he used extensive, data-based statistical techniques. While important scholarly attention has been devoted to Friedman's academic and political trajectories, his methodological prescriptions, and the development of economics at the University of Chicago, we know much less about the interplay of these elements. This paper proposes an intertwined reading of them. My aim is threefold. First, to understand Friedman's work and methodological choices, I relate his empirical approach to his early training in statistics. Second, I articulate Friedman's understanding of economics as an empirical policy science in the process of building the image of economists as neutral advisers in the policy-making process. Third, I claim that Friedman's empirical methodological framework, developed while he was in the Economics Department of the University of Chicago, established the guidelines for an institutional long-term project that shaped it.*

I. INTRODUCTION

In 1976, in the 194th volume of *Science,* the magazine of the American Association for the Advancement of Science, the Swiss macroeconomist Karl Brunner presented to the broad scientific community that year's winner of the Nobel Memorial Prize in Economics,

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Milton Friedman. Brunner introduced Friedman as an economist characterized by a "sense of empirical science." Brunner portrayed Friedman as *the* economist who, during the early postwar period, when mathematics and econometric methods spread in the discipline, "guarantee[s/d] a useful understanding of economics as an empirical science" (Brunner 1976, p. 595), in sharp contrast with many other protagonists of the period. Economists from the University of Chicago also took pride in highlighting this "empirical sense." For instance, in 1982, Melvin W. Reder published an article in the *Journal of Economic Literature,* describing the evolution of the main ideas associated with economics at the University of Chicago. For Reder, a graduate student in the Economics Department during the 1940s and, from 1974 until his retirement, professor in the Graduate School of Business, the main difference between Chicago and other institutions concerned "the appraisal of applied economists, the quality of whose performance is measured by the interest and reliability of their empirical findings rather than by theoretical ingenuity or display of technical virtuosity" (Reder 1982, p. 33).

This paper analyzes Milton Friedman's empirical approach to economics. My aim is threefold. First, I relate Friedman's early training in statistics to what I call his *approach to empirical research in economics.* Second, I articulate Friedman's understanding of economics as an empirical science in the process of building the image of economists as neutral advisers in the policy-making process. Third, I characterize the influence that Friedman's approach had on the Economics Department at the University of Chicago. I claim that, as reflected in the Economics Department's curriculum and practices, Friedman's methodological prescriptions set the guidelines for a long-term project that reinforced economics' character as an applied policy discipline.

While extensive scholarly attention has been devoted to Milton Friedman's academic and political trajectory,[1] his methodological prescriptions,[2] and the development of economics at the University of Chicago,[3] we know much less about the about the interplay between these three elements. This paper proposes an intertwined reading of them. This is significant, because it expands both our knowledge of Friedman and Chicago's role during the second half of the twentieth century and our understanding of how knowledge is produced and reproduced within economics. Moreover, the processes studied here are key pieces to understanding the heterogeneity that existed within the economics mainstream and explaining the effectiveness of economists' views in the policy arena.

II. ECONOMICS AS AN EMPIRICAL SCIENCE: FRIEDMAN'S DATA-DRIVEN VIEW OF ECONOMIC SCIENTIFICITY

Today, thanks to the work of historians of economics, we know that throughout his career as a scholar and a professional economist, Milton Friedman never strayed far from

[1] See, for example, Hammond (1999c), and Cord and Hammond (2016). For an exhaustive list of Friedman's publications, see Cole (2012).

[2] For an analysis of Friedman's methodology, see, for example, Frazer and Boland (1983), Hirsch and de Marchi (1991), Teira (2007, 2009), Mäki (2009), Cherrier (2011), and Hands (2003).

[3] See, for example, Samuels (1976), Patinkin (1981), Emmett (1998, 2010c), Hammond and Hammond (2006), Harberger and Edwards (forthcoming), Mirowski et al. (2011), Stapleford (2011), and Van Horn, Mirowski, and Stapleford (2011).

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the collection, production, or analysis of data. His repertoire of empirical methods can be traced back to his academic training and the beginning of his professional career. His training in statistics is noteworthy.[4] As an undergraduate student at Rutgers (1928 to 1932), he received a heavy dose of statistical methods and quantitative analysis from Arthur F. Burns. As a graduate student, shifting back and forth between Chicago and Columbia, he honed his statistical skills under Henry Schultz at Chicago and Harold Hotelling at Columbia. This academic experience made him well acquainted with the statistical methods introduced by Ronald Fisher in the 1920s.[5] Later, as was also the case with several of his fellow Chicago students, Friedman worked in Washington, DC, at key institutions responsible for gathering, producing, and analyzing economic data. First, between 1935 and 1937, Friedman was at the National Resources Committee (NRC), a federal agency devoted to supporting economic planning, and later, between 1941 and 1943, at the Treasury's division of tax research as principal economist.[6] However, his experience at the National Bureau of Economic Research (NBER), between 1937 and 1941, is perhaps the most relevant to understanding Friedman's approach to empirical research in economics.[7]

At the NBER, Friedman worked on a wide-ranging statistical survey intended to quantify consumer incomes and expenditures in the United States during the period 1935–36 and to provide a statistical answer to the debate over the causes of the Great Depression. More concretely, Friedman's tasks ranged from data collection and analysis to the development of procedures for the collection and statistical testing of data. So, as his Washington sojourn had done, the New York City experience at NBER fostered his statistical training, on both practical and theoretical levels. Friedman also participated in a collective effort to renew the teaching of statistics in American higher education, a task he first undertook during his brief stay at the University of Wisconsin (1940–41) and later at Chicago (Hammond 2019, pp. 173, 177–182), where he contributed, in 1949, to the creation of a separate department of statistics (Olkin 1991, pp. 125–127).[8]

[4] See Hammond (1999b, 2019), Frazer and Boland (1983, p. 133), Karl Brunner (1976, p. 595), and Ericsson, Hendry, and Hood (2016, pp. 91–93).

[5] On the spread of Fisher's ideas into statistics and economics in the United States, see Biddle (2017).

[6] At the NRC, Friedman was recruited by fellow student and future Chicago School mainstay Allen Wallis, who had been hired earlier in 1935; Friedman's future wife, Rose Director, also worked briefly on the same project, as did Erika Schienberg, a student of Paul Douglas, and Alice Hanson Jones, another doctoral student at Chicago. George Stigler also worked for the NRC (1935–36) but on a different project from Friedman's. As Thomas Stapleford (2011) suggests, the presence at the NRC of so many young Chicago graduate students reflects the department's strengths in quantitative analysis and also the dismal job market for academic economists in the mid-1930s.

[7] More generally, the connections between the Economics Department of the University of Chicago and the NBER lasted over many years, because many members of the department had affiliations with the NBER (Gary Becker and George Stigler are but two examples). Close personal relations also developed between Friedman, Stigler, and Wallis with Arthur Burns. See Reder (1982, p. 5).

[8] Friedman viewed statistics through the lens of personal probability and was greatly influenced by Jimmie Savage's Bayesian way of thinking about probabilities. Savage was Friedman's coauthor of two articles on expected utility (1948, 1952) and his colleague at the Statistical Research Group at Columbia during World War II. However, it would be anachronistic to classify Friedman's empirical research as Bayesian. What we now call "Bayesian analysis" was developed in the 1950s and later by Savage and others. See Dwyer (2016), and Ericsson, Hendry, and Hood (2016, p. 93).

Milton Friedman's statistical trajectory echoes closer institutionalist approaches than those usually labeled as "neoclassical" (see Stapleford 2011; Hammond 2019). Contrary to the views held today, Malcom Rutherford (1994, 2010, 2013) showed that postwar Chicago economists and institutionalists associated with quantitative analysis have significant methodological ties. As Abraham Hirsch and Neil de Marchi (1991) have argued, except for their disagreement over the empirical validity of "neoclassical" price theory, Friedman's own methodological outlook was quite similar to Wesley Clair Mitchell's. His attitudes toward methods of data collection and analysis were strongly influenced by his experiences and ties at the NBER (Ericsson, Hendry, and Hood 2016), as exemplified by his books coauthored with Anna Schwartz (1963, 1970, 1982) .[9]

Throughout his life, Friedman's work remained associated with Mitchell and the NBER. Paul Samuelson's characterization in 2007 is revealing. In response to Kenneth Arrow's comments on his paper, "Classical and Neoclassical Harmonies and Dissonances" (2007), Samuelson wrote:

> I value IQ in a theorist very low. This includes Kaldor, Kalecki, M. Friedman, Johnson and others. If Abba and Milton have equal I.Q.'s, their lifetime *theoretical* contributions are far from equal…. No surprise then that he [Friedman] was the last Luddite believer in the 1890 Marshallian partial equilibrium and in the 1930–1950 Mitchell-Burns National Bureau.
>
> I could write more but I've gotta walk my dog.[10] (emphasis added)

Samuelson's remarks reflect two important things. The first is that close links with the institutionalists are far from a direct path to scientific legitimacy within the American economics community. Since the postwar period, the members of the Chicago Economics Department were aware of this. Around 1955, as Eric Schliesser (2012) suggests, a hardening of attitudes toward institutionalist approaches took place at Chicago. For instance, George Stigler—mostly through his writings on the history of economics and economic methodology (Stigler 1965)—constructed a narrative in which the approach associated with institutionalism increasingly came to be seen as not belonging to economics at all. The second is that, at Chicago, distance from institutionalism did not mean closeness with the then new, more abstract-oriented approaches for doing research in economics that progressively were entering the mainstream (Morgan and Rutherford 1998). Indeed, the view emanating from Friedman's approach to empirical research in economics of what counted as truly scientific work—his data-driven view of economic scientificity—made this amalgamation impossible.

Throughout his tenure at Chicago, Friedman repeatedly brought up a conflict between what he called "the theorist's urge to be realistic" and the urge to "satisfy the canons of formal logic" (Friedman 1946, p. 621). This idea was already present in his writings from the early 1940s. As Marcel Boumans (2016) showed, in his 1942 paper with Allen

[9] Some of the methods employed by Friedman and Schwartz that exemplify these ties are: data adjustment, model augmentation, model selection, simple regressions, and subsample estimation. The general idea of all these methods is to fit simple models that are checked on subsamples. Friedman's (1940) review of Jan Tinbergen's modeling revealed that his views about data analysis were crystallizing early in his career (Ericsson, Hendry, and Hood 2016, p. 134).

[10] Paul A. Samuelson Papers, Box 12, folder Kenneth Arrow, "Economist Papers Archive," David M. Rubenstein Rare Book & Manuscript Library, Duke University.

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Wallis, Friedman opted for avoiding "schizoid concepts," i.e., those "thoroughly competent in the field of deductive analysis but utterly incompetent for quantitative analysis" (Friedman and Wallis 1942, p. 176). And he concluded: "The more mathematical constraints are imposed by the definition of a theoretical concept, the less value it will have for the organization of empirical data" (Friedman and Wallis 1942, p. 186). Friedman presented this procedure as the mark of what he labeled a "Walrasian approach," which he canonically criticized in his 1946 review of Oscar Lange's *Price Flexibility and Employment* (1944) (de Vroey 2009). According to Friedman, the overall result of Lange's work was a "formal entity," an "enormously oversimplified universe" with "no direct empirical counterpart" (Friedman 1946, p. 619), and no "comprehensive set of observable and related facts" (p. 618) to support the "lengthy and abstruse theoretical argument" (p. 614) at the heart of the reasoning.

Yet, as Friedman made explicit in the review, the critique of Lange's work was but a means to reach a bigger target: what he called "taxonomic (or formal) theorizing," an approach that, during the postwar period, had its headquarters at the neighboring Cowles Commission.[11] During the six years (1939 to 1945) that research center was located at Chicago, many of Cowles's men were also affiliated with the Economics Department, and the presence of members of the Economics Department was not unusual in the Commission's seminars. Nevertheless, as we know well today, the tensions between the two institutions were important.

To be clear, the idea here is not to offer a polar characterization of Chicago and Cowles as data-driven versus abstract reasoning. It would be more appropriate to frame the tensions between the two institutions as revolving around two alternative empirical approaches for economics (Boumans 2016). In fact, Lange's work was not completely in line with the orientation adopted in the mid- and late 1940s at Cowles, when empirical work enjoyed a brief productive period, after Jacob Marschak's appointment as research director on January 1, 1943. Marschak's research program for the Commission was very much oriented by Trygve Haavelmo's way of doing empirical economics (Bjerkholt 2014). Lawrence Klein's lifelong purpose of building large-scale macroeconometric models of the complete structure of the economy is the archetype of the work done at Cowles under Marschak's direction. However, while Klein's project involved an important component of empirical work, Friedman's critiques of Cowles did not stop (Pinzón-Fuchs 2016).

Cowles Commission researchers' and Friedman's alternative empirical approaches for economics illustrate the difficulty associated with grasping and establishing what *empirical research* really is or what it should account for. *Empirical research* is not an object that our concepts or language can track down, and every conception of what it is and what are its uses has institutional implications. Indeed, its meaning and uses are intimately related to very real resources, careers, funding, prestige, status, public relevance, and so on.[12] In order to understand the tensions between researchers at the

[11] On the history of the Cowles Commission at Chicago, see Bjerkholt (2014), Dimand (2019), Hildreth (1985), and Mirowski (2002). For an official historical record of the Cowles Commission's years at Chicago, see Christ (1952, 1994).

[12] Gabriel Abend (2014) proposes an analysis of the institutional implications of the establishment of what *theory* is in the academic field. Abend's analysis illuminates our analogous case of the establishment of what *empirical research* is.

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Cowles Commission and Chicago, it is important to relate Friedman's criticisms and methodological prescriptions to the economists' quest for scientific authority in the policy arena and to the long-term research project that he institutionalized and led in the Economics Department, as I will do in sections III and IV, respectively.

III. ECONOMICS AS SCIENCE IN THE POLICY ARENA: FRIEDMAN'S PREDICTIONS AS NEUTRAL INSTRUMENTS OF ECONOMIC POLICY

Both Friedman's and Cowles's empirical approaches to economics could be related to the economist's quest for the authority of science in the policy arena. However, they were driven by very different ideas regarding the organization of society. Throughout his life, Friedman advocated for a free market economic system.[13] In contrast, the Cowles Commission's purpose, especially under Marschak's direction, was to advise government agencies and make policy recommendations related to economic planning (Epstein 1987, p. 61). In the years after the Great Depression, these differences became significant and are key to explaining Friedman's critiques of Cowles's empirical approach and, in particular, of Klein's macroeconometric project.

After the New Deal's regulatory and subsidy programs and the central planning for the war effort, the belief in the efficiency of a strong and active central government was widespread (Morgan and Rutherford 1998). The Employment Act of 1946 codified the responsibility of the federal government to promote maximum employment, production, and purchasing power, and established the President's Council of Economic Advisers. Several federal and national agencies were born in this context, including the National Resources Committee of the US House of Representatives (a body created by executive order of the president and devoted to discussing planning in the country). As a result, both the use of economic research by government departments and economic publications on policy issues grew. The American Economic Association (AEA) was very active in this process (Bernstein 1990, 2001, 2008). By the end of World War II, the AEA noticeably appointed a series of committees to present "the consensus prevailing in the discipline regarding issues of public policy" (Committee on the Focusing of Informed Opinion 1944, p. 424), to those involved in the policy-making process. The Committee on the Focusing of Informed Opinion, the first of these committees, emerged from a proposal by Frank D. Graham during the 1943 Annual Business Meeting to circulate questionnaires to some (but not all) AEA members, in an attempt to discover if there was a consensus on various issues.[14] During the 1940s and 1950s, two related committees grew out of Graham's initiative: first the Consensus Committee and later the Committee on Public Issues. Both committees established a series of subcommittees to prepare statements on specific issues of public policy.[15] In trying to identify points of consensus on issues of policy, these committees wanted to systematize the economic knowledge

[13] For instance, just one year after the end of World War II, he was part of the executive committee of the "The Free Market Study." See Hammond (1999a) for a detailed analysis.

[14] This was not the AEA's first attempt to explore the extent and potential of consensus among economists. For information on the Committee on the Focusing of Informed Opinion and the previous consensus initiative of the AEA, see Coats (1985).

[15] See, for example, Bell (1945).

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produced by AEA members and then argue that they, and not simple opinions, should be used in the process of policy-making.

As with other fields bearing on matters of policy, economics is exposed to scrutiny and criticism by those whose interests are affected, e.g., political authorities and citizens. A key to establishing the authority of science emerges from the construction of distance from any interests (Porter 1997)[16] Consensus (on the value of a theory, the relevance of the methods used in the research, the definition of a problem, the scope of a contribution, or the treatment of a problem, to give only a few examples) is a feature of knowledge regularly summoned to establish this distance and, more concretely, to claim the status and authority of science. In the interdisciplinary field of studies of science, consensus is indeed ubiquitous as a demarcation criterion and as a principle of hierarchization between disciplines. It is regularly presented as the locus of progress and rationality. Two sources are at the origin of this special epistemological status: on the one hand, its conceptualization as a rational end point of episodes of change and scientific controversy; and, on the other hand, its presentation as a necessary condition for the accumulation of knowledge.[17]

In 1949, Milton Friedman participated, with Emile Despres, Albert G. Hart, Paul A. Samuelson, and Donald H. Wallace, on the Stability subcommittee, one of the Consensus subcommittees. The result was the publication of "The Problem of Economic Instability," a report aiming to summarize economists' knowledge on the nature of the problem of economic instability and its treatment. While important disagreements were noted, the authors of the report did not give up and claimed a privileged position in the policy-making process—as the last paragraph of the report reveals: "This diversity of views and programs is significant in indicating that the problem of prices, wages and profits is as yet largely unsolved. Difficulties in this field should not … prevent us from using fiscal and monetary controls to do the best we can in limiting fluctuations in total income, expenditure, production, and employment" (Despres et al. 1950, p. 538).

The Stability subcommittee, along with the Agricultural Price Supports and their Consequences and the Functions of the Government in the Post War Economy consensus subcommittees, reported "major differences among economists and major gaps in [their] knowledge" (Despres et al. 1950, p. 503). Nevertheless, the project of using consensus to claim the authority of science in the policy arena transcended the AEA's consensus committees. Milton Friedman later became famous for his defense of the role of predictions in achieving professional consensus on policy issues. According to Friedman, since the differences about economic policy derive predominantly from different predictions about the economic consequences of taking action, the agreement between economists on policies to be chosen would come with the report of more and more accurate results. Yet, consensus did not arrive. In the years to come, Friedman

[16] Eric Schliesser (2015) takes a different approach to this question. Analyzing a broader period of time (from the end of the eighteenth century to the beginning of the 1970s), Schliesser studies Friedman's (but also Stigler's) commitment to a technocratic conception of politics and science as related to a long lineage including Henry Sidgwick, J. N. Keynes, Max Weber, Lionel Robbins, Talcott Parsons, and Robert Merton.

[17] For a summary of this literature, see Miriam Solomon (2001). Solomon also presents a critique of the epistemological virtues of consensus. For a sociological analysis of how consensus evolves and is maintained in the natural and social sciences, see Stephen Cole (1983). For an analysis of this process in economics, see Camila Orozco Espinel (2017).

turned out to be a controversial economist, whose predictions did not produce much of the desired consensus among his peers—in spite of their purported accuracy. Later, in his *Memoirs*, Friedman almost suspected "ideological bias" as being responsible for the lack of acknowledgment of the success of any adversary's predictions (Friedman and Friedman 1998, p. 219). We can offer an alternative explanation for Friedman's ideological lack of trust and relate the limits of predictions as consensus devices to his approach to empirical research in economics and, more concretely, to his methodological prescriptions concerning the criteria to define theoretical categories.[18] Indeed, David Teira (2009) has shown how, by relaxing coherence, Friedman's methodological prescriptions concerning the definition of categories hindered consensus. In other words, under Friedman's methodological prescriptions, it was always possible to argue that another classification of data was possible when disagreements existed.

While Friedman's methodological prescriptions ultimately failed in establishing predictions as consensus devices, he did succeed in imposing them as neutral instruments for economic policy. In this case, instead of connecting scientific knowledge with consensus, Friedman exploited another source of scientific authority frequently used by scholars: the association between empirical knowledge and value-free knowledge. Indeed, when scholars present their work to the public, political authorities, or their colleagues, claiming the authority of *science*—hoping to enlarge their material and symbolic resources—empirical reasoning is often stressed as an essential, and even unique, characteristic of scientific knowledge.[19] This strategy is driven mostly by positivistic principles, an epistemology that was in vogue among mid-twentieth-century US social scientists.[20] As George Steinmetz (2005, p. 5) has shown, the period between World War II and the mid-1960s represented a rather coherent era, "characterized by a relatively homogeneous regime of social science and an epistemological predilection for positivism."[21]

For Friedman, predictions are value-free instruments of economic policy (Friedman 1967, 1986), and thus economists could serve as neutral *scientists* who merely predicted the outcome of various actions, as opposed to partisan advisers.[22] Predictions are tested by their empirical accuracy and make progress possible in *positive economics*—a body of systematized knowledge concerning what is. In *normative economics*—a body of

[18] On the criteria to define theoretical categories, see Friedman's heuristic rule number 3 in section IV of this paper.

[19] See, for example, Gieryn (1999, pp. 115–183). Gieryn analyzes the competition between phrenologists and anatomists for the chair of Logic and Metaphysics of the University of Edinburgh during the early 1800s as a case of boundary work, where one group of scientists excluded another group, also claiming to be scientific, on the basis of their lack of empirical testing of their results. The *Methodenstreit* is an example of the use of empirical methods to claim the authority of science in economics. See, for example, Mirowski (1991) and Louzek (2011).

[20] It is important to highlight that there is no consensus on the association of Friedman with positivistic principles. For instance, Eric Schliesser (2011), Peter Galbács (2019), and others associate Friedman with a Weberian tradition.

[21] We must note how far Friedman was from the Vienna Circle as to the proper definition of theoretical terms. Nevertheless, as the works collected by George Steinmetz (2005) show, positivism persisted in the US sciences during the second half of the twentieth century in guises that differ from the versions that were popular in Europe during the 1920s and 1930s.

[22] This idea is systematically presented in the first section of Friedman's 1953 essay "The Methodology of Positive Economics": "The Relation between Normative and Positive Economics."

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systematized knowledge discussing criteria of what ought to be—predictions are used by policy-makers to reach their political objectives. We may relate Friedman's emphasis on empirical testing through predictions to his statistical training (Teira and Zamora 2009). As mentioned in section I, Friedman was well acquainted with the statistical methods introduced in the 1920s by Ronald Fisher. Throughout his career, he applied mostly Fisherian statistical methods and thus regression techniques as the means to obtain predictions.[23] Regression analysis, as Fisher taught it, relied explicitly on a classificatory device: the contingency tables invented by Karl Pearson (Porter 1988). These tables collected observations in their rows and columns and delivered, in the bivariate case, an intuitive test of their fitness to the distribution pattern required by correlation analysis, so that further predictions could be derived through a regression (Teira 2007).

Of course, during the aftermath of World War II, Friedman was not the only one in the discipline to believe that economics was, or could be, normatively neutral and that the discipline should be valuable for policy decisions. Furthermore, the positive economics/normative economics distinction dates back to the late nineteenth century with John Neville Keynes's *The Scope and Method of Political Economy* ([1891] 1955), as Friedman (1953) explicitly acknowledged.[24] Likewise, while Friedman did not refer to Lionel Robbins in his 1953 essay, the positive/normative distinction is fundamental to the arguments developed in Robbins's famous *Essay on the Nature and Significance of Economic Science* (1935).[25]

Yet, during the aftermath of World War II, Friedman pushed the neutrality of economics further, introducing it into the political sphere. He argued that economics could predict outcomes for concrete policy problems and thus provide a normatively neutral analysis of means, so that policy-makers could focus narrowly on ends. Certainly, Robbins's definition of economics through *scarcity* assumes a clear demarcation between means and ends (Robbins 1935, p. 15). Indeed, before Friedman, Robbins's *Essay* played an important role in establishing the separation between positive and normative propositions in economics. In the discipline between the early 1930s and the late 1940s, the controversies raised by Robbins's *Essay* showed that the distinctions were no closer to conventional knowledge (Backhouse and Medema 2009a, 2009b).

The ways in which debates around Robbins's definition changed over the years are an indication of the transformations that economics experienced. The shift toward concerns related to predictions and testability during the late 1940s and early 1950s was significant. For Roger Backhouse and Steven Medema (2009b, p. 809), Paul Samuelson's *Foundations of Economic Analysis* (1947) and his focus on the importance of deriving

[23] Friedman's article, coauthored with David I. Meiselman (1963), is an example of how he tested a hypothesis (in this case a stable money-income relation) through the empirical accuracy of its predictions. For an analysis and other examples, see Ericsson, Hendry, and Hood (2016).

[24] It is worth mentioning here that while Friedman quotes J. N. Keynes's discussion of a three-part distinction (between positive and normative economics and the art of economics), Friedman's essay discusses only the methodology appropriate for positive economics. See Colander (1992) for an analysis of the consequences for the discipline of economics of Friedman's focus on the methodology of positive economics and his dismissal of the art of economics.

[25] Friedman and Robbins were well acquainted before the publication of "The Methodology of Positive Economics." They first met in 1947 in London, either on the trip to or on the return from Switzerland for the Mont Pelerin Conference. Later, in 1950, when Friedman gave two lectures at the London School of Economics, Robbins hosted him in his home (Howson 2011, pp. 664–665, 743).

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"operationally meaningful theorems" was important and accelerated the shift.[26] Karl Popper's work was also important in the process of bringing economists closer to predictions and testability. This process took place primarily at the London School of Economics (LSE) during the 1950s and early 1960s, in a group led by Richard Lipsey and Chris Archibald (de Marchi 1985).

Yet, the acceptance of Friedman's conception of the role of economists in the policy-making process was not smooth, not even at the University of Chicago. During the 1930s, Frank Knight's certainty of the impossibility of far-reaching predictions, based on constraints coming from imperfect information and the impossibility of predicting human action, distanced Friedman from his colleagues. Later, during the 1960s, George Stigler's skepticism about the interest of politicians in *scientific* advice continued the controversy (Reder 1982, pp. 25–32).

In a wider context, Friedman's views were not less controversial. In a paper entitled "Economic Research in Relation to Public Policy," Robert Calkins, president of the Brookings Institution (1952 to 1967) and a former director of the New York Federal Reserve Bank, presented an almost diametrically opposed version of the role of economists in the policy-making process and of the policy-making process itself. For Calkins, in policy-making "the closer the research analyst is to the decision-making process, the greater is his concern with those immediate specifications and considerations that make a policy proposal acceptable" (Calkins 1953, p. 433). Conspicuously, in 1953, during the 65th AEA Annual Meeting, Calkins presented his view of the policy-making process, as opposed to Friedman's distinction between the positive and normative in economics. For Calkins, confining economists to offer advice concerning means but not to recommend the choice of ends may be a valid approach. But this approach can hardly be taken as a reliable guide to professional conduct, until it has been re-examined and brought into harmony with the growing need for economic advice and with the nature of ends and means: "ends are so often the means to higher goals and means so generally define the intermediate objectives that serve such goals" (Calkins 1953, p. 439).

The shift that took place during the late 1940s and early 1950s toward concerns related to predictions and testability has already been mentioned. We may also connect Friedman's role in separating means and ends in economics and establishing predictions as neutral instruments of economic policy to the long-term process, which took place in the United States at the beginning of the twentieth century, of transformation of the role of social science scholars, in Mary Furner's (1975) words, from "advocacy" to "objectivity." Moreover, the rapidity with which politics became a controversial issue among economists is also related to the broader social environment, within which the production of economic knowledge was carried out in the United States. The two key elements here are the absence of a tradition of autonomous academic guilds and the lack of support of a strong state (Fourcade-Gourinchas 2001). In the United States, where expert knowledge is not a technocratic arm of the state itself and thus where top civil service positions have traditionally been filled with outsiders, Friedman's neutral economist, who merely predicted the outcome of various actions, was crucial to assuring

[26] Backhouse and Medema (2009b) also briefly discuss how reviewers of *Foundations* in the late 1940s related Samuelson's work with Robbins's claims.

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a place for economics in the policy-making process. For Marion Fourcade, the intervention of economists in public arenas in the United States has been shaped "not only by their own 'scientific' capabilities but also by the particular expectations emanating from the institutions that request such expertise in the first place" (Fourcade 2009, p. 128). The suspicions of the ideological underpinnings of knowledge from the social sciences (Porter 1997), which characterized American political culture during the mid-twentieth century, encouraged the use and reinforced the acceptance of Friedman's conception of the policy-making process.[27]

Placed at the top of the profession and teaching in one of the most influential university economics departments in the United States, Friedman and his methodological prescriptions have had a lasting impact on the relationship between economics and the process of policy-making (Colander 1992). Yet, Friedman's institutional prestige, his future trajectory foretold by the Clark Medal that he received in 1951, and the social environment of the United States do not by themselves explain the long-term influence that Friedman's methodological prescriptions exerted. Indeed, while methodology offers itself as a regulative metadiscourse for economic practice, economists are generally dismissive of methodological writing and its relevance for their work. As Daniel Breslau (2005, p. 461) stated, "reception, acceptance, and citation of economic work do not proceed according to rules or fixed methodological criteria." The practical demands of a scientific field do not correspond perfectly to any formal and independent methodology.

The analysis of Friedman's role as an administrator, as an educator, and as an adviser in the Economics Department of the University of Chicago provides insight into how his methodological prescriptions and, more generally, his approach to empirical research in economics and his views on economists' role in the policy-making process influenced the field. Through this exercise, we can see how the crystallization of his methodological prescriptions in the curriculum and practices of the Economics Department set the guidelines for a long-term project that reinforced the character of economics as an applied policy discipline, while simultaneously expanding its frontiers.

IV. ECONOMICS AS AN APPLIED POLICY DISCIPLINE: FRIEDMAN'S METHODOLOGICAL PRESCRIPTIONS INSTITUTIONALIZED

Friedman's methodology was originally intended as an *a posteriori* exposition of the principles that guided his own research (Hammond 1999a). If it was subsequently adopted by many others in the discipline, this was not because economists required a parallel evaluative methodology to do their work.[28] Yet, methodological debates are as heated, and the participants have just as much at stake, as theoretical controversies. This can be related to the social function of methodology for collective long-term research projects. Friedman's 1953 essay, "The Methodology of Positive Economics,"

[27] It is worth mentioning here Walter Lippmann's vision of technocracy in a democracy and how it helped in shaping the post-WWII state. On this point see Schliesser (2019).

[28] On the ambivalences of the relationship between scientific practice and methodological discourses, see Gaston Bachelard (1934).

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read in conjunction with the other essays in the volume (in which the methodological piece was the preamble), illustrated this point. We can start from the hypothesis that Friedman's methodological work can be seen as a set of heuristic rules that structured graduate education in the Economics Department of the University of Chicago. Graduate study at Chicago was a process of immersion in those practical rules, so that they became so intuitive that, in combination with new empirical investigation, they opened the door to novel fields of application, some of them conspicuously outside the traditional jurisdiction of economics. In other words, we can interpret these heuristic rules as *habitus* in Pierre Bourdieu's sense, a socially ingrained habit, skills, and disposition that determine the way in which individuals intuitively act (Bourdieu 1998).[29]

I identify four different practical rules in Friedman's methodological writings:

1. Avoid general equilibrium analysis and focus on single markets

From the very beginning of his career, Friedman defended the use of partial equilibrium in economics, opposing it to a general equilibrium approach by its empirical emphasis.[30] For Friedman, general equilibrium analysis could not be used for numerical calculation. As he stated in reviewing *The Marshallian Demand Curve*, "[t]he analyst who attacks a concrete problem can take explicit account of only a limited number of factors" (1949, p. 469). In the same early methodological piece, Friedman explicitly stated what single-market analysis should be useful for: "predicting the consequences of changes in the economic environment" (1949, p. 490).

2. Find empirical data and generate predictions with simple statistical techniques (fewer variables are better than more)

An example of how Friedman achieved predictions is his NBER joint monograph with Simon Kuznets (1945).[31] The idea of the study was to identify the determinants of the number (and hence the income) of workers from different professional groups. Friedman and Kuznets identified several statistical indexes that provided a classification of data suitable for predictive purposes.[32]

3. Interpret your economic categories in a way flexible enough to grasp the actual data you are dealing with

With Friedman's chosen statistical techniques, the more mathematical constraints are imposed by the definition of theoretical categories, the less value they will have for the organization of empirical data. Thus, instead of searching for an empirical counterpart that corresponds to the theoretical definition of the categories, Friedman opted for proxy

[29] See Bourdieu and Wacquant (1992), for a synthesis of Bourdieu's theory of action.
[30] For an analysis of Friedman's divide between the Marshallian and Walrasian approaches, see de Vroey (2009).
[31] For his doctoral dissertation at Columbia University, Friedman submitted chapters 3 and 4 of this study.
[32] Later empirical works by Friedman, for instance his 1957 *A Theory of the Consumption Function*, do not apply this rule. For an analysis of Friedman's selection, treatment, and uses of data, see Ericsson, Hendry, and Hood (2016, pp. 99–101).

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variables (statistical indexes).[33] For instance, in his joint monograph with Kuznets, Friedman restates the categories of demand theory to correspond with the statistical variables at hand, e.g., prices and expected income would approximate the commodities offered by professionals (Friedman and Kuznets 1945, p. 155).

4. *Whoever has the best predictions has the best theories*

For Friedman, predictions were, as seen in section II, neutral elements of economics policy. The empirical accuracy of predictions was the mechanism chosen by Friedman to settle debates between concurrent approaches. He proposed concrete tests: naive models (Friedman 1951).[34] As Marcel Boumans (2016, pp. 559–602) has pointed out, these kinds of tests came to be his only, albeit lasting, contribution to the Cowles's econometrics program.[35]

To understand the implementation of these four practical rules, studying Friedman's implication in the doctoral program is decisive. It explains how, together with new empirical investigation, the incorporation of these heuristics made the University of Chicago Economics Department a field well befitting for economics to secure its credentials as an applied policy discipline. Before presenting the crystallization of Friedman's heuristic techniques in the graduate program, it is important to stress the polysemy of the term *applied*, and thus of the notion of *applied economics* and, more generally, of the notion of *applied sciences.* While the dichotomy of pure (or basic) and applied is common in many boundary-work activities, the distinction is often difficult to sustain in practice.[36] However, the practices of economists, as well as those of other natural and social scientists, do not fall into these neat compartments. For instance, in their description of the twenty-first century as the age of the *applied economist*, Roger E. Backhouse and Béatrice Cherrier (2017) introduce three different meanings that the word *application* can have in economics: 1) tailoring a theory to the study of specific issues, by creating a model that is relevant to a specific set of circumstances; 2) using a theory to explain or analyze a specific set of data; 3) participating in the design of public policies or business strategies. Milton Friedman's approach to empirical research in economics belongs in the second group. However, this is not the point that I want to stress. As Backhouse and Cherrier highlight, economists employ a broad use of the term *applied* and each of the three categories will group together very different types of research.

[33] For an analysis of the link between the classification of data and the definition of theoretical categories, see David Teira (2009).

[34] A naive model is a model in which minimum amounts of effort and manipulation of data are used to prepare a forecast. The naive models most often used are random walk (current value as a forecast of the next period) and seasonal random walk (value from the same period of prior year as a forecast for the same period of forecasted year).

[35] See Pinzón-Fuchs (2016) for a historical account of Carl Christ's (1951) test of Klein's model with the naive models suggested by Friedman as standards of comparison.

[36] For a historical account of the distinction between *pure* and *applied*, see Paul Erickson (2017). Erickson shows how the distinction, developed by natural scientists during the nineteenth century, was used to achieve two seemingly irreconcilable goals immediately after World War II: preserving the independence of scientists, and convincing those in a position to provide financial support that "pure" science lay behind many useful inventions and was therefore worth funding.

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Between 1940 and 1960, the Economics Department underwent a transformation of the curriculum and research concentrations. Friedman's methodology for empirical research in economics was central in the process of setting up the structure and content for a PhD program that integrated faculty research and graduate education in ways that encouraged applied economics. The workshop system is the institutional structure that created the environment in which this process took place, allowing continuity and a unified long-term research program over the years. The implementation of the workshop system is part of a wider process: a transformation in the orientation of research in the social sciences at the University of Chicago. The idea was to institutionalize a disciplinary organization of social sciences while, at the same time, building an appropriate framework to support interdisciplinary, empirically oriented studies. The construction of the Social Sciences Research Building (SSRB) illustrates the direction of these changes. Newly built rooms were specially designed to host research groups and seminars, and there were physical spaces dedicated to the analysis and storage of statistical data. The aim was to create the necessary infrastructure to emulate the production and transmission of knowledge in the natural sciences. The fact that, in the SSRB, several workspaces have been identified as "laboratories" embodies the character of the project. In these laboratories, technical and human resources for data processing (especially calculations) were made available to researchers at different stages of their careers who could meet in these spaces to carry out joint projects. As Ross Emmett (2011) has shown, the roots of the workshop system are found in these "laboratories."

Indeed, during the late 1940s, D. Gale Johnson presented a proposal that today is recognized as the first version of a Chicago-style workshop in the form of a laboratory.[37] The first workshop conceived as such, the Workshop in Money and Banking, was directed by Milton Friedman.[38] The presentation of Friedman's workshop in the 1952 *Announcements* of the university captures the essence of the project: "an experiment in combining training in research and learning of the subject matter organized around a continuous investigation into monetary factors in business cycles" (cited in Emmett 2011, p. 104). Students, whose presence was central for the project, were expected to participate during the sessions and to undertake individual projects toward the preparation of a report of publishable quality. Guidance was provided through readings and informal seminars held on a non-regular basis. For Karl Brunner (1976, p. 596), "The [Money and Banking] workshop produced, under Friedman's guidance, some of the best studies on inflation and the role of money on inflation."

At the Economics Department, the implementation of the workshop system was part of a phase of reorientation of its teaching practices. At this point, Friedman's presence was central, leading the department toward greater homogeneity. His charisma, the critiques that he addressed to the work in progress of his colleagues and students (deemed energetic), and his vigor during debates quickly made him the

[37] Johnson's proposal was part of a project funded by the Rockefeller Foundation (Emmett 2011).
[38] Like most workshops during the 1950s, Friedman's was first funded by a grant from the Ford Foundation and then by the Rockefeller Foundation.

center of gravity in the department.[39] As a participant-observer remarked in the *Journal of Economic Literature*, summarizing the permanence and change at Chicago:

> On a wide variety of issues, one was led—drawn into—defining one's own position by reference to Friedman's views. Agree or disagree, one had to come to terms with his ideas given the power of these ideas, his debating skill, and the force of his personality, it was not surprising that students and colleagues tended to agree with him more often than not. And even when managing to disagree, or continue in disagreement, they came away with a very firm impression of the case for the opposing view. (Reder 1982, p. 32)

Until his retirement in 1976, Friedman led the Economics Department with his workshop and his own work but also through his administrative responsibilities and his supervising and teaching activities. Friedman played a central role in designing and implementing the restructuring of the graduate curriculum. He directed the commission that reviewed the standards for the evaluation of dissertation proposals, one of the first steps toward program reform, and supervised the research of seventy-five students (Hammond 1999a, p. xix). The Workshop in Money and Banking was the main place where Friedman supervised his advisees.[40] However, it was with the Price Theory course that he taught, first between 1946 and 1964, and then again between 1972 and 1976, that Friedman participated most in the direction of the Economics Department.[41] He taught the course from a partial equilibrium perspective, with Alfred Marshall's *Principles of Economics* as a textbook. During Friedman's tenure, almost all of the students in the department took his course.[42] The main idea was that a wide variety of problems could be studied using price theory. Contrary to Knight's approach in the previous version of the course, Friedman coupled empirical analysis of statistical data and price theory. Friedman began the course by collecting applications of theory. Then, rather than separating the theories of perfect and imperfect competition—as was the convention in other price theory courses—he intermingled the coverage, according to which was relevant to the problem under discussion (Hammond 2010, p. 10).

Gary S. Becker, who took the graduate Price Theory course under Friedman, has written on what he found distinctive about Friedman's approach:

> The emphasis in his course on applications of theory to the real world set the tone for the department. It was considered necessary to have a strong working command of basic price theory, especially so-called partial equilibrium supply and demand analysis. Yet the theory was not an end in itself or a way to display pyrotechnics. Rather, the theory became worthwhile only insofar as it helped explain different aspects of the real world. (Becker 1991, p. 142)

Friedman's approach to empirical research in economics and its applications to policy issues had a very important institutional function. It provided a long-term guide that

[39] There are many testimonials about the voracity of Friedman's criticism of papers presented in his workshop. See, for example, Reder (1982, p. 32).
[40] It is important to mention Aaron Director's role advising early, data-driven PhD students at the University of Chicago, both in the Economics Department and at the Law School, as discussed by Schliesser (2012).
[41] In 1962, Friedman's notes for this course were published in Friedman (1962).
[42] For a history of the Price Theory course in Chicago, see Hammond (2010).

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made possible a process of knowledge accumulation. This method of producing knowledge contrasts with how researchers at the Cowles Commission were organized. As does Friedman's "Methodology of Positive Economics," Tjalling Koopmans's (1957) *Three Essays on the State of Economic Science* operates as a long-term route. The postulational method that Koopmans proposed in his methodological manifesto was thought of as an engine for the cumulative refinement of models, so that they could take into account more and more aspects of reality. Friedman's methodology for empirical research in economics works quite differently. By broadening the range of applications to explain not only the production and consumption of commodities but also subjects beyond the traditional jurisdiction of economics, Friedman's methodological prescriptions contributed to broaden the frontiers of economics, pushing the discipline to colonize other domains, such as legal theory, political science, and sociology. Conspicuously, Friedman's approach to empirical research in economics influenced the structure and orientation of the University of Chicago Law School.[43] As Ross Emmett (2010a, pp. 2–3) put it: "post-war Chicago economists became a scholarly group who used a common economic approach to analyze all aspects of life."

Gary Becker's work is the canonical example of Chicago's imperialistic method of accumulation of knowledge. In *The Economics of Discrimination* (1957), starting with racial discrimination, a very concrete question, rather than with a theoretical problem, Becker expanded the range of application of the theory.[44] As Becker suggested, Chicago's imperialism is rooted in a deep belief that no one stands outside of economics rationality and that, furthermore, money is the primary medium through which economics rationality expresses itself.[45] The expansion of economics to problems related to crime (Becker 1968) and marriage (Becker, Landes, and Michael 1977) exemplifies the mechanism of knowledge accumulation driven by Friedman's approach to empirical research in economics.

V. CONCLUSIONS: FRIEDMAN'S VIEWS IN THE POLICY ARENA

During the second half of the twentieth century, economists at the University of Chicago played a prominent role in their discipline. As Ross Emmett (2010b, p. 269) has said, "What happened in Chicago did not stay in Chicago." Indeed, postwar economists at Chicago participated in changing the discipline. This paper focused on the role of Milton Friedman's approach to empirical research in economics during this process. I started by relating Friedman's early training in statistics to his view of what counts as truly

[43] On Chicago law and economics, see the work of Steven Medema, for example, Medema (1997) and Medema and Mercuro (2000).

[44] Both Friedman's positive/normative distinction and economics imperialism can be connected to Lionel Robbins's methodological work. For Robbins, defining economics through scarcity "had the implication that, insofar as it deals with the influence of scarcity, any kind of human behavior falls within the scope of economic generalizations" (Robbins 1935, p. 16). Yet, Gary Becker's combining use of maximizing behavior and rationality involves a narrower definition of economics. Moreover, the expansion of the boundaries of economics is also connected with the shift from scarcity to choice as the main element defining economic actions. See Backhouse and Medema (2009a, pp. 227–230).

[45] For an analysis of Chicago's totalizing claims of economics rationality, see Fourcade (2009, p. 93). See also Becker (1993).

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scientific work in economics. I analyzed how Friedman's 1953 methodological piece sought to establish the normative neutrality of (positive) economics through predictions, specifically through an empirical test of its accuracy. In other words, for Friedman, if economists could serve as neutral scientists who merely predict outcomes and, concomitantly, policy-makers could focus narrowly on ends, this was due to the empirical and thus scientific test of theories through the accuracy of its predictions.

If we track back economists' beliefs about their participation in the policy-making process as neutral scientists—so as their success in spreading this idea in the policy sphere—certainly Milton Friedman is but one agent in a larger constellation of processes taking place. Yet, the way in which he turned his methodological prescription into a long-term research project at Chicago is specific to Friedman's approach to empirical research and his view of economics as an applied policy discipline. The institutionalization of Friedman's methodological prescriptions at the Economics Department is, as well, an important part of the explanation of his lasting influence in the discipline. I contend that Friedman's methodological prescriptions were crystalized in the Economics Department's curriculum and practices. More concretely, I claim that Friedman's methodological prescriptions can be read as a set of heuristic rules that structured graduate education in economics at the University of Chicago. Throughout this process, a particular approach to empirical research was developed at Chicago, an approach that reinforced the character of economics as an applied policy discipline, while simultaneously expanding its frontiers.

By paying close attention to how the context, where Friedman was trained and developed his career as a professional economist and scholar, structured his practice of economics, the questions addressed in this paper remind us that scientific practices are always embodied in specific social and institutional contexts. By focusing on the reciprocal influence of Friedman's academic and political trajectories, his methodological prescriptions, and his role structuring the development of economics at the University of Chicago, this paper highlighted the importance of the links among the political, methodological, and practical dimensions of knowledge production. While individually these matters have received extensive scholarly attention, the intertwined reading proposed in this paper expands our knowledge of Friedman's 1953 piece, the view of economics as an applied policy science that led to it, and how these views shaped the Economics Department at the University of Chicago.

Whether we think that people worldwide have benefited or suffered from Friedman's ideas, the processes studied here are pieces to understand the effectiveness of his views in the policy arena, as well as his influence in academia.

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