

# The lucky consistency of Milton Friedman's science and politics, 1933-1963

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During my whole career, I have considered myself somewhat of a schizophrenic, which might be a universal characteristic. On the one hand, I was interested in science *qua* science, and I have tried – successfully I hope – not to let my ideological viewpoints contaminate my scientific work. On the other, I felt deeply concerned with the course of events and I wanted to influence them so as to enhance human freedom. Luckily, these two aspects of my interests appeared to me as perfectly compatible (Friedman 1993, 189).

Like most of his peers, Friedman (1953a, 4) confidently claimed that he was able to prevent his political beliefs from interfering with his scientific practice. In so doing, he indeed gained the respect of many including political adversaries: “Even those who, like myself, often find themselves in basic political disagreements with him, must greatly admire and indeed envy his fearless and penetrating insight of the deepest roots of economic issues. He has done more directly and indirectly, to train economists in rigorous thinking and in the uncovering of common prejudices than any other teacher in many decades”, wrote Keynesian economist Abba Lerner to the Nobel Prize committee in 1976.<sup>2</sup> This strict separation between science and politics was taken for granted by some historians of the Chicago School such as Van Overtveldt 2007, allowing them to focus

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<sup>2</sup> Lerner to members of the Alfred Memorial Prize committee, undated (probably 1973), Abba Lerner Archives, Box10 folder 7.

exclusively on the internal reasons for success of the Chicago School – excellence and debate culture, mastering of neoclassical price theory and importance of empirical work.

Judging from the stir created by the reception of the Bank of Sweden Prize in Economic Science by Friedman, however, it seems that a number of economists were not convinced by Lerner's statement nor would they agree with Van Overtveldt's history.<sup>3</sup> There was strong suspicion that the consistency between Friedman's market oriented microeconomics and his antistatist political recommendations was not merely accidental.<sup>4</sup> His macroeconomic work has similarly been interpreted as an attempt to shatter the pillars of American Keynesianism, one realized at the expense of reliable theoretical and empirical foundations.<sup>5</sup> On the side of historians of economics and economic methodologists, Friedman's work has been described as an idealized view of the free-market system and as a positive rationalization of his political convictions (Samuels 1976, 7-13 and 363-395; Nelson 2001, Lavoie and Seccareccia 1993, 16). Familiar with the Marx-Mannheim critique of economics, those critics were concerned with hunting down the ideological dimension of economic science. Though the marxist concept of "ideology" has gradually been replaced by references to vision, paradigms, ethics or value judgments, the idea remains the same - the economist is influenced by his social status and his political beliefs and economic theories are not only designed to explain the world, but also to support group interests and legitimize particular course of actions (see for instance Amadae 2003). In this vein Rob Van Horn and Phil Mirowski 2009 have recently argued that "the rise of the Chicago School must be understood as one component of a specific larger transnational project of innovating doctrines of neoliberalism for the postwar world (p 140)," by uncovering the links between some Chicago projects and the establishment of the Mont Pelerin Society (hereafter MPS) and their funding by conservative organizations.

These narratives, however, provide no detailed account of the process whereby political values may have influenced Friedman's science.<sup>6</sup> Yet, given that Friedman, as a statistician, subjected his hypotheses to a demanding process of confrontation with facts, the permanence of

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<sup>3</sup> See for instance Myrdal, (1977, 52) or Kaldor (1978, viii), and Rose Friedman (1977, 20) for an opposite version.

<sup>4</sup> See for instance Bhagwati 1977's depiction of Friedman as an "ideologue" or Rapping in Klamer 1984.

<sup>5</sup> For instance, Keynesian economist Franco Modigliani claimed that "Friedman is driven by the idea that whatever the government does is bad. He has a mission and seems to be willing to sacrifice some intellectual honesty for that." (Modigliani in Klamer 1984, 120), and Culbertson (quoted in Hammond 1996, 117) accused Friedman of a "deliberate misrepresentation of the truth" in his review of *A Monetary History*.

<sup>6</sup> Warren Samuels (1976, 374) suggested that theories are value laden because Chicagoans assumed hypotheses taken from an *a priori* paradigm (the superiority of market solutions) and only paid attention to evidence consistent with these hypotheses. For Lepage 1977, both Chicago's science and ideology were rooted in the same representation of the human being as a rational maximizer.

some private beliefs in his science calls for further explanation. Likewise, Friedman's 1998 claim that it was his science which influenced his politics rather than the reverse is left unexplored. And if it is now widely acknowledged that economic research is inescapably pervaded by ideology, then, says David Colander (2005, 11-12), the consideration must be shifted to "intentional ideological bias...that exceeds that ideological background level of ideology that affects us all."<sup>7</sup> Yet, a framework in which persuasion (see McCloskey 1983), social control and the ideological functions of economic theorizing is emphasized, and in which it is difficult to distinguish between the role of ideology in the *application* of a theory (a case well documented) and in its *formation*, is not suited to the analysis of Friedman's "intentions."

Investigating how Friedman's ideas were initially formed requires a framework in which Friedman's beliefs can be disentangled from his propaganda strategies, and this paper is an attempt to provide one. To that end, we will consider economic research and political activity as just two ways of making sense of the world. Both activities rely on beliefs, views and values built from private experience and from an understanding of the social environment, themselves deriving from religion, readings, professional training, practice and encounters, etc. These beliefs are ethical and political, but also positive (they amount to a view of the working of society and motivation of agents) and methodological (they assume certain methods to reach objective truths and a certain role of science in society). They tend to be combined in an "overview", an overarching picture of the world we will call a "worldview" in the tradition of phenomenology and hermeneutics, and in the process some beliefs are used outside their natural sphere.<sup>8</sup> They are neither necessarily conscious nor consistent with each other, and they are transformed, adjusted, corrected throughout the economist's life. They form an interpretative framework which is used as a heuristic for the discovery of new facts and theories within science and which may provide factual certainties on which to build a political ideology.

The thesis of this paper is that Friedman's scientific and political conclusions were consistent because they were jointly developed from a strong worldview, one that was framed during the thirties and forties and constantly reinforced afterwards. This worldview was based on a

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<sup>7</sup> According to this principle, "it is Friedman, not Kenneth Arrow or Armatya Sen, who would, in my view, be far more likely to be described as crossing the ideological line", Colander (2005, 22) concluded.

<sup>8</sup> We could alternatively have used the words cosmology, patterns, *gestalts*, world picture or others. The purpose here is not to invoke such and such specific theory of understanding and of the possibility of objective knowledge. Nor is it to argue that this concept is systematically distinct from that of ideology; Schumpeter 1949 equated ideology with a "precognitive scheme", a "vision" (see also Heilbroner 1990), and our definition of a worldview echoes Samuel (1977, 470)'s definition of ideology. The somewhat artificial distinction between these two notions is meant to differentiate between an approach based on collective values, social control and propaganda from one based on individual essential beliefs.

vision of society comprised of free independent self-interested individuals cooperating through market mechanisms to exchange goods, ideas, values, this resulting in mutual benefit and global stability. Individual freedom should therefore prevail over other values, Friedman claimed. This sanguine vision of society was paired with a distrust of collective action and a willingness to limit strictly (albeit not suppress) state intervention. The methodological belief that science helped reach consensus among citizens by producing objective knowledge through the confrontation with facts was another crucial feature of Friedman's worldview. Some of these beliefs developed from Friedman's private history and temperament, others from his training, professional encounters and experiences. It is on these same core beliefs that Friedman simultaneously built a scientific and a political system in the fifties and sixties.

Section 1 describes the origins and characteristics of Friedman's worldview, from his student days to the postwar years, when he was hired at Chicago, became interested in monetary economics, and participated in the founding of the Mount Pelerin Society (hereafter MPS). Section 2 shows how Friedman's worldview informed his choice of hypotheses, his understanding of the "facts" (data series and historical evidence) he compared them to, and his econometrics. Section 3 examines the policy recommendations that Friedman produced *qua* scientist and *qua* political activist. In this in-between area where applied economics required reliance on private conceptions of the economic system and where neoliberal doctrines were grounded in academic research, the boundaries of science and politics become blurred.

## **The making of a worldview, 1933-1948**

Friedman's double connection with Chicago (where he studied economics with Jacob Viner, Frank Knight and Henry Schultz) and the NBER (his study of income with Simon Kuznets in 1937-1941 would earn him a PhD from Columbia), his early practice as an economist (at the *National Resource Committee* in 1935-1937 and the *Treasury* in 1941-43) and as a statistician (at the *Statistical Research Group* (SRG) in 1943-1945) is described in Jamie Peck, Tom Stapleford and Dan Hammond's articles in this volume. From the variety of scientific practices and political perspectives he encountered in this formative period, Friedman fashioned the methodological, theoretical and political beliefs that would form the basis of his worldview.

***How to produce scientific knowledge useful in a free society?***

Friedman's methodology was shaped gradually and pragmatically in reaction to the various scientific practices he encountered. His 1933 work on the estimation of demand curves, conducted under the leadership of Henry Schultz, plus his research on the National Resources Committee (see Stapleford, this volume) led to a rejection of rational choice based microeconomics. Building indifference curves empirically was impossible, he argued, because agents rationalize or are confused, and are therefore not reliable (Friedman and Wallis 1942). His encounter with Keynesian theory, through the estimation of the tax rise necessary to curb inflation for the *Treasury* in 1942, the review of Oskar Lange's *Price Flexibility and Employment* in 1946 and of Abba Lerner's *The Economic of Control* in 1947 ended up with similar reservations. Friedman found the income-expenditure and the multiplier theories poor representations of the numerous channels whereby consumption behavior impacted prices and pointed to their lack of empirical content. His NBER study led to his encounter with the theory of imperfect competition, which the NBER director Reynold Noyes wanted him to adopt for the study of medical profession because of differences in abilities and equipments. Instead, Friedman and Kuznets assumed that physicians competed freely, applied Marshallian price theory, and warned that the framework chosen should depend on the practical issue at hand (in their case, emphasizing the similarities *within* each medical profession to analyze income differences *between* them).<sup>9</sup> In fact, Friedman already believed that the perfect competition theory *always* proved "more useful for concrete analysis" (Friedman 1941, 390). By the end of the forties, he realized that these various theories he had challenged exhibited common characteristics: their quest for descriptive accuracy ended up in a "retreat into purely formal theory." He collected them under the label "Walrassian economics" and lamented about the "increasing prestige of speculative analysis" (Friedman to Ruggles 28/06/51, Box 32 Folder 16, MFA).

To this Walrassian economics, he opposed his own "Marshallian" approach, a policy-oriented and concrete problem-solving method he claimed to have found in his reading of Marshall's *Principles*, from which he borrowed his methodological catchphrase that science is "an engine for the discovery of concrete truth" (Friedman 1949, 469; 1953a, 35 See also Hammond, this volume). The economist should not strive to reach realistic statements valid in *all* circumstances, he explained, but theories "analytically relevant" for the issue at hand and judged by their predictive ability (Friedman 1940; 1953a, 9).<sup>10</sup> This policy-oriented approach however raised the threat that

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<sup>9</sup> "Memorandum in reply to Noyes", 1941, p.7-8, Box 37 folder 24, MFA.

<sup>10</sup> The two drafts of Friedman's 1953 methodology essay are tellingly titled "Descriptive Validity vs. Analytical Relevance in Economic Theory" (1948) and "The Relevance of Economic Analysis to Prediction and Policy" (1952) (Hammond 2004).

the economist, as a citizen personally involved in the issues he studied, might blend his positive analysis with ideological beliefs. Friedman had to face these very accusations in 1938, when the Board of Directors of the NBER opposed the publication of the income study on ground it was ideologically laden.<sup>11</sup> Friedman and Kuznets replied with a lengthy report adducing new empirical evidence and tests to their case and concluded that “our interest in social problems (and who doesn’t have such interests?) has stimulated us to make, rather than prevented us from making, an objective and tolerably thorough study of the facts of professional income.”<sup>12</sup> Friedman's early confidence in the scientist’s immunization from ideological bias was indeed firmly grounded in the empirical methodology he had inherited from his statistical training, his NBER affiliation, and his experience at the National Resources Committee, where despite the planning purpose of the institution and the left-wing ideology of most members, economists were committed to objectivity, empirical work, and cultivated relationships across the political spectrum (Stapleford, this volume). It was therefore a pragmatic rather than a philosophical stance, prompted by his observation that people from various political background (free-market oriented businessmen as well as public planners) would accept the same set of quantitative data as a basis for policy, and that he, as a neoliberal, could work with the progressive Kuznets and Mitchell.

A joint outgrowth of the Chicago Marshallian tradition and of his NBER affiliation, Friedman’s methodology was also closely related to his vision of society and of the role of the scientist within it. The preface of his work in professional incomes reveals that even before he became an activist, Friedman already envisioned American society as a combination of free independent, self-reliant and autonomous individuals seeking their material welfare and some agreement on common values through the exchange of goods and ideas : “this approach treats professional activity as taking place in an economy best described as a free enterprise system in which the production of goods and distribution of incomes are regulated primarily by the impersonal mechanism of the market” (Friedman and Kuznets 1945, v). Such vision probably owed to his personal history: the survival of his parents, poor Jewish emigrants, he attributed to the American free enterprise system, and his ability to pay for his studies after his father's death by finding casual jobs in the midst of the Great Depression endowed him with a sanguine view of individuals. The preservation of a “free society in which voluntary cooperation is to be the foundation” required a “framework for settling differences of opinion by peaceful means.” And in

<sup>11</sup> The main bone of contention was Friedman and Kuznets’s explanation of the particularly high average income of physicians as compared with dentists by restrictions of entry in the medical profession.

<sup>12</sup> *Memorandum by Noyes*, undated; Crum to Carlson, 27/08/42; Memorandum to the Directors, by Friedman & Kuznets, undated 1943-1944, p25-26, Box 37 folder 24, MFA. See Also Friedman (1953, 4) especially the comparison with natural sciences.

the wake of his encounter with Jimmy Savage in 1943 at the SRG, Friedman “ha[d] been led increasingly to regard scientific method as a set of conventions for resolving disagreements,” as he later wrote to Friedrich Hayek: “I have been much influenced by the work of... James Savage... [who] claimed that probability judgements are judgments held by individuals separately, that there is nothing objective about any of these, and that the only way to define objective probability is in terms of agreement among different persons’ subjective probabilities... [Savage] argues that the true function of statistics is not to discover knowledge but to resolve disagreement among people on subjective p,” he explained. The kind of “true, though untestable, explanation” advocated by Hayek would however not help settling disagreements between free men “peacefully.” Only the empirical confrontation with fact would convince them, he continued in the same letter<sup>13</sup>

### ***Market efficiency and the “competitive order” as a reality***

Friedman's personal vision of human motives and society was reinforced by his –selective– understanding of the philosophical framework Knight transmitted him at Chicago. Knight’s depiction of a society as a set of rational individuals endowed with diverging preferences and pursuing their welfare and his claim that market oriented systems should be favored because planning presupposed unattainable perfect information struck a chord with the young student. Friedman's optimistic focus on the benefits of cooperation through markets nonetheless superseded his teacher’s insistence on the pervasiveness of value conflicts, his warning that the role of economic motives in human affairs should not be overlooked and that economic science should be supplemented with an ethical reflection (see Emmett 2007 on the legacy of Knight). While Friedman only used utility maximization as an “as if” hypothesis, in line with his 1942 rejection of rational choice theory, individual choices of the economic kind nonetheless formed a cornerstone of his worldview and acted as a heuristic for his research. For instance, his work on income distribution (Friedman 1953b) aimed at demonstrating that income inequality is not so much a product of institutional and legal constraints as the result of free individual choices and taste for risk (Hirsch and de Marchi 1990, 174). His vision of human motives have also overflowed the boundaries of the scientific sphere, so that his whole worldview can be characterized as essentially economic. As argued by Miller (1962, 68), “a major characteristic of the Chicagoans is their

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<sup>13</sup> Friedman to Hayek, 11/09/75, Box 20 folder 19, Hayek papers. Friedman's vision of the role of the scientist within society was also underpinned by his natural inclination for prozelytism and his faith in the power of ideas to change society (Nelson, 2001, 140).

tendency to see and to apply economics all around them.” This is illustrated, for instance, by Friedman and Stigler’s early recreational talks of children as commodities and associated calculation of “the cost of purchasing children in country and in city” to explain the birth rate differential between countries and cities (Friedman to Stigler, 1952, quoted in Hammond & Hammond, 2006, 126). This also explains why economists renowned for their forays in sociology, psychology or political science, such as Gary Becker, emphasized the influence of Friedman on their intellectual development (see Becker 1993).<sup>14</sup>

According to Leonard Rapping (in Klamer 1984, 221), a former Chicago student, Friedman thought the coordination of these free autonomous individuals proceeded unimpeded: “many Chicago people would argue that the world is, in fact, competitive,” he remarked. Van Horn 2009 has extensively documented how this belief grew out of Friedman’s participation to the Free Market Study (FMS), a project designed to demonstrate through objective and impartial empirical investigation that the American economy is competitive. Under the influence of its chairman Aaron Director (Friedman’s brother-in-law), Friedman endorsed the idea that the importance of monopolies in the American economy had been greatly overestimated; since economic forces tended to restore competition on markets, they could be considered minor and transitory. He tied his belief to compelling scientific evidence –the empirical evaluation of the importance of monopolies within the US provided by his student Warren Nutter, and to the personal observations during a 1950-51 journey around Europe.<sup>15</sup> In a letter to colleague, close friend and political ally George Stigler, where we can consequently assume he dispensed with rhetorical flourish, he commented that “by comparison, America is perfectly competitive –you don’t need to measure the degree of monopoly: it’s zero”.<sup>16</sup> Also, it was not only monopolies whose importance was overestimated, but also the other perceived obstacles to market adjustments, such as price rigidities (Friedman 1948) or unions, whose impact on wage he estimated as limited (Friedman, 1951a, 16).

In back-and-forth movements between the scientific and non-scientific spheres, Friedman’s picture of the economic system as free economic agents coordinating through competitive markets, built from a mix of private experiences, intellectual influences and scientific evidence, gradually

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<sup>14</sup> Friedman strongly supported the publication of Becker’s thesis on *The Economics of Discrimination* (1957) against sceptics, and admired his subsequent work on crime and punishment, family, and time allocation.

<sup>15</sup> See also Schliesser, this volume, on the methodological ties between Nutter’s dissertation and Friedman’s methodology.

<sup>16</sup> Friedman to Stigler, 15/01/50 [51], cited in Hammond and Hammond 2006. Friedman attributed the preservation of competition within the US to American’s culture: and the the Sherman antitrust law. (*Free Enterprise : An American View 1950*, Box 41 Folder 2, MFA).

reinforced itself. It created lenses through which he interpreted new evidence, which he therefore saw as supporting his beliefs. For instance, Friedman (1958) retained from a trip to India and other developing countries during the fifties the notion that economic development must rest primarily upon the aspirations and actions of millions of individuals risking their own capital and seeking their own fortunes. His colleague Neil Jacoby, who supported a more active role of the government, made it perfectly clear that their disagreement was rooted in their diverging views of human nature: “I do not have the same faith that you do that there are in underdeveloped countries at the present time millions of ‘able, active and vigorous people’ who are motivated to engage in enterprise and who have the skills and capacities to do so.”<sup>17</sup>

The consistency I have identified may seem overstated (see Hacothen 2007 on the danger of this widespread fault of historians). However, together with the process just described, I attribute the unusual stability of Friedman’s vision to the protective and united Chicago intellectual environment in which he spent his remaining life. Friedman’s closest colleagues, including Stigler, Wallis and his wife Rose, exhibited similar scientific backgrounds (Chicago training with a NBER-style empirical practice) The cross-fertilization of their ideas produced a common paradigm that they succeeded in institutionalizing at the University of Chicago, one they say largely grew out of Friedman’s vision (Miller 1962; Reder 1982, 10, 32-33; Stigler 1963, 1988). The demanding PhD program which “incalculated distinctive habits of thought” in students (Reder 1982, 9; see also Emmett in this volume) and the constant financial support backing their research program enabled Friedman to transmit his vision through the price theory course he taught between 1946 and 1963 (see for instance the testimonies of Becker 1993, 32, and Lucas and Rapping in Klamer 1984), and some of their subsequent contributions (listed in Reder 1982 for instance) eventually came as a scientific legitimization of his worldview.

### ***A liberalism grounded in a distrust of government and lobbies***

Like his positive beliefs, the political convictions of Friedman can be seen as a mix of intellectual

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<sup>17</sup> Jacoby to Friedman, 03/09/58, Box 28 folder 27, MFA. See also his report on a 1962-63 world tour: “What impressed me most was the striking conflict between widely held beliefs and the empirical evidence. It is widely believed that economic progress in underdeveloped countries requires active governmental intervention in the form of central economic planning in order to assure the effective use of limited resources. Yet, if an observer who knew nothing about the ideological disputes on recent decades, or about current governmental policies, were asked to rank the various countries we visited according to the standards of living of ordinary people and the signs of material progress in their living conditions, there is little doubt that he would put the countries that have relied most heavily on central planning at the bottom and those that have relied on it least at the top”(Friedman to Anderson, 27/01/64, Box 23 Folder 3, MFA).

influences and personal observations. The conservatism of Jones and Burns first oriented Friedman toward classical liberalism, a tendency then reinforced by Knight's teaching. His main source of inspiration, however, was Simons's *A Positive Programme for Laissez-Faire* (1934), especially its emphasis on freedom and on the need to organize a large sector of economic life competitively to preserve it (Friedman 1951c, 1955). Friedman found the book "strongly pro free market in orientation," and completely overlooked Simons's condemnation of concentration of economic power in unions and large corporations as well as in the hands of government, a neglect he later attributed to the comparatively "strongly prosocialist" intellectual atmosphere of Chicago (Kitch 1983, 178-179). His permanent contacts with the little group above mentioned, in particular his wedding with Rose Director, who is credited for being the driving force behind Friedman's political zeal (Silk 1976, 68), explained why he remained remarkably immunized from the many progressive influences he encountered in his formative years - the teaching of Douglas at Chicago, Mitchell and Kuznets at Columbia and NBER, the institutionalism of Wisconsin and the Keynesian atmosphere of Washington administrations.<sup>18</sup>

The strength of Friedman's liberalism can also be explained by the several negative experiences of collective action, lobbies and bureaucracy he went through in this formative period. Friedman interpreted the NBER controversy over his work with Kuznets as an excuse to defend the interests of medical lobbies (Friedman 1998, 74). In 1941, he was caught in the University of Wisconsin's internal department fights. Because he taught statistics and because he was Jewish, he was attacked by officials willing to merge the economic department with the business school (Lampman 1993, 118-121). From his stay at the *Treasury*, "[he] concluded that... the frenetic atmosphere of a political capital, with its concentration of day-to-day issues... renders a true scientific attitude almost untenable" (Friedman R. 1976a), Rose remembered. He interpreted a controversy between the *Treasury* and Leon Henderson and John Galbraith from the *Office of Price Administration* over the estimation of the amount of additional taxes necessary to curb inflation as a deliberate strategy by the OPA to secure the legal authority over the determination of prices (Friedman 1998, 111). Finally, upon publication of *Roofs or Ceilings* (1946), a pamphlet against rent controls, Friedman and Stigler were asked by Leonard Read and O. Watts, directors of the very conservative Foundation for Economic Education (FEE) who commissioned the book, to delete some paragraphs ironically considered as "collectivist

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<sup>18</sup> Rose later undertook the editorial work for *Capitalism and Freedom* 1962, coauthored the bestseller *Free to Choose* 1981 and insisted that her husband accept to write chronicles for *Newsweek* (Friedman 1998, 356).

propaganda" (Skousen 1998).<sup>19</sup> The two authors firmly refused to do so.<sup>20</sup>

These experiences made Friedman aware that public intervention was plagued by multiple biases and inefficiencies, but he was also conscious that in some cases government help was unavoidable.<sup>21</sup> The diagnosis that the expanding role of the state and the present-day social and economic issues required an in depth modernization of the principles of liberalism was one that he shared with the other American and European intellectuals (including Director, Knight and Stigler), businessmen and journalists who gathered in 1947 at the initiative of Hayek to create the MPS.<sup>22</sup> Friedman's participation in the society identified him as a neoliberal (Mirowski 2009, 429), but whether MPS *influenced* Friedman's political thinking rather than *reflected* it is however difficult to document, for reasons touched upon in the last section. Whether Friedman's liberalism preexisted or not, it was nevertheless this founding meeting that initiated his activism.

That Friedman's positive, methodological and political beliefs jointly developed from a range of private and scientific experiences explain why they were inextricably interwoven. This common set of beliefs built both his monetarism and his brand of neoliberalism in the following decades.

## How Friedman's beliefs influenced his science

Between 1948 and 1962, Friedman produced his major works: *The Theory of the Consumption Function* (1957), and his theoretical and empirical work on money found in *A Restatement of the Quantity Theory* (1956) and *A Monetary History of the United States* (1963) with Anna Schwartz. For both subjects, Friedman told a story of his intellectual development which emphasized that "the sequence of problems dealt with is determined by the internal structure of the work and not really by these kinds of external influences..."<sup>23</sup> Still, his invariably anti-keynesian and anti-interventionist conclusions suggests otherwise.

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<sup>19</sup> Friedman and Stigler's incriminated statement was that they, like most economists, were seeking the most efficient means to attain "even more equality than there is at present, not alone for housing but for all products" (Friedman and Stigler 1946, 6).

<sup>20</sup> Friedman to Read, undated 1946, Box 38 Folder 6, MFA.

<sup>21</sup> « Even if we had completely free access to different employments and to capital, there would still be the problem of poverty », he remarked in his introduction to a 1947 MPS session on « taxation, poverty and income distribution » (Box 5, folder 12, MPS archives)

<sup>22</sup> On the history of the MPS, see among others Hartwell 1995 and Mirowski and Plehwe 2009.

<sup>23</sup> Friedman to Breit, 22/06/67, Box 21 folder 26, MFA.

## ***The underlying quest for stable behaviors and self-stabilizing mechanisms***

In 1967, Friedman claimed that “it was a sheer accident that led me to undertake as my first major project the study of incomes from independent professional practices. I was simply offered a job to work on that. It was there that the ideas about the importance of a regression toward the mean about the notions of permanent and transitory elements of income developed.” Indeed, the Permanent Income Hypothesis is often described as the outgrowth of Kuznets’s paradox - that the marginal propensity to consume remained stable across time despite steady increase in aggregate income, in contradiction with the Keynesian theory-, and of the postwar attempts by several economists including Rose Friedman to explain current consumption by variables other than current income (see Hynes 1998). Thanks to his permanent-transitory income framework and ingenious identification hypotheses, Friedman was able to reconcile these opposing empirical conclusions within a single explanatory model. This model implied that the multiplier associated with government spending was very low unless the associated additional income was seen as permanent by households, thereby making stabilization policy inefficient.

This anti-interventionist conclusion, however, suggests that Friedman was influenced by more than just the technical developments in the field. Friedman’s main motive for devising a new consumption theory was indeed to defeat the “secular stagnation thesis” and the idea that there could be long run underemployment: “the doubts about the adequacy of the Keynesian consumption function raised by the empirical evidence were reinforced by the theoretical controversy about Keynes’s proposition that *there is no automatic force* in a monetary economy to assure the existence of a *full-employment equilibrium position*,” Friedman (1957, 5; emphasis added) wrote in the introduction of his consumption book. Since the existence of a “full employment equilibrium,” a consequence of the assumption that markets *are* competitive, was a cornerstone of the Chicago view (Reder 1982, 11-12), Friedman sought to build a dynamic theory that would supplement static Marshallian theory. His participation in a subgroup of the Committee on Public Issues of the American Economic Association devoted to the problem of economic instability in 1947-1949 and his 1948 reflection on monetary policy made him aware that the touchstone of his disagreement with Keynesians was the issue of stability. And the long run stability and full-employment that he believed characterized capitalist societies meant that the free coordination of self-seeking individuals through markets necessary embodied self-stabilizing mechanisms, such as the real balance effect (Friedman 1948). As a result, Friedman's consumption theory assumed a relation between consumption and wealth, both with stable characteristics: wealth consisted in the attribution of a constant annual income (except for a transitory component) on an infinite horizon,

the so-called permanent income. Likewise, the consumer was said to consume his resources at a stable rate. Friedman's worldview therefore imposed preconceptions on what should be looked for in the data.

Friedman's account of his work on money is similarly thin and linear: "Once having started studying in the field of money, the internal logic of that development led a life of its own," he wrote to William Breit. His monetarism developed from the re-examination of the historical evidence for the 1929-1933 period, he claimed, and from the "Chicago Oral tradition" that made it natural for him to turn to the quantity theory as a theoretical framework (Friedman 1956, 3).<sup>24</sup> Yet, the conclusions he had reached by 1963 –the stability of the velocity of circulation of money, the responsibility of the Fed in the worsening of the 1929 crisis, the variations in the stock of money as a major cause of business cycles and the existence of "lags in transmission" prohibiting the use of discretionary policies again sounded anti-Keynesian and fit perfectly with his neoliberal values. The coincidence was striking enough so that Hirsch and De Marchi (1990, 205) provided the following reconstruction of Friedman's intellectual development:

In retrospect it looks as if he had seen by the late forties that he could not make an impression on the dominant Keynesian thinking merely by expressing methodological reservations about the system and its concepts. Something had to be offered in its place, an alternative that bypassed what he had long recognized as the key Keynesian element, the multiplier, and which performed better on the prediction front than it did. He chose the quantity theory as his candidate and probably began the NBER study with the aim of collecting data appropriate to testing the adequacy of a simple quantity theory hypothesis.

If not a deliberate strategy, Friedman's focus on a reformulated quantity theory may be explained again by his quest for stable behaviour and self-stabilizing mechanisms. He came to the study of money with an *a priori* opposition to the Keynesian representation of the economic system, in which, according to Modigliani (1977, 27), "a private enterprise economy using an intangible money *needs* to be stabilized, *can* be stabilized, and therefore *should* be stabilized." For Keynesians, instability arose from the erratic move of interests rates, which in turn determine investment and the demand for money. Because of the possibility of liquidity traps caused by low interest rates, policy makers must resort to fiscal policy to restore equilibrium. The hypotheses *a contrario* selected by Friedman (see Hammond 1996, 59-63 for details) -money is an asset used by households to adjust their portfolios, the velocity is a consequence of individual choices rather than

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<sup>24</sup> The existence of a Chicago oral tradition has been one of the most debated claims by Friedman. The huge literature initiated by Patinkin's denial of such tradition is collected in Leeson 2003.

technical factors- were consistent with his belief that economic outcomes resulted from the interactions of autonomous agents through competitive markets. His quest for stable behaviors influenced both the kind of data he collected and the pattern he found in them, e.g. the procyclical and stable behavior of the velocity (Friedman 1952b, 621). He then explained it by a reformulation of the quantity theory where the demand for money was determined by permanent income (Friedman 1956) but not by interest rates (see Friedman's 1959 empirical study and Tobin's 1965 critique). Such reformulation allowed for a self-stabilizing mechanism through portfolio allocation, making government stabilization policy unnecessary (Modigliani 1977, 27). This framework in turn influenced Friedman and Schwartz's subsequent interpretation of new historical evidence. In the *Monetary History*, they explained the downward trend of velocity prior to World War two by the rise of income and the fact that money was a luxury good, and attributed the trend reversal after the war to enhanced expectations of economic stability, an explanation that their Keynesians opponents condemned as *ad hoc* (Tobin 1970).

It however appears that Friedman's opponents were less critical of his theoretical hypotheses than of his empirical practice. At first, they called the latter “naïve” and “deceptively pretentious empiricism” (Culbertson, quoted in Hammond 1996, 115-118, Modigliani and Ando 1965, Karenken and Solow 1963), but when monetarism gradually came to be seen by economists and political figures as a valuable alternative to Keynesian economics, the tone hardened. Friedman was accused of “chicanery” and “charlatanism” (Johnson 1971, Kaldor 1982) and of distorting his results (Desai 1981, Hendry and Ericsson 1983). The violence of the attacks reached a climax when Frank Hahn (1984, 326; 1985) derided the “sheer bravado of reducing the beautiful structure of general equilibrium theory to one or two log-linear equations” and compared Friedman's econometric methods to ancient Roman Greek oracles. Friedman had succeeded in engaging his Keynesian opponents in an “econometric race”, which eventually left the hegemony of the income-expenditure theory seriously undermined (see Leeson 2000, ch3). Yet, the choice of econometric weapons by both sides was not merely driven by alleged technical superiority and scientific purity; it also indicated opposite visions of the usefulness of these results for the formation of economic policy.

### ***How visions of government informed econometric choices***

The monetarist-Keynesian econometric race brought to the fore diverging conceptions of empirical

work, epitomized by Koopmans's famous "measurement without theory" critique of Burns and Mitchell's *Measuring Business Cycles* (1946).<sup>25</sup> Cowles researchers tried to model the economy as an exhaustive set of interdependent, simultaneous and stochastic relationships of the Walrassian kind, and believed that the data would speak only once such exhaustive structural model be imposed on them.<sup>26</sup> Friedman found such undertaking unduly optimistic given their little of knowledge of economic behavior and therefore doomed to failure, as he explained to Joseph Willits from the Rockefeller Foundation in 1947.<sup>27</sup> But Cowles econometrics was also informed by the political agenda of its members. That Jacob Marschak, director of research between 1943 and 1947, titled the blueprint for his directorship "Statistical Foundations of Rational Economic Policy," is telling of what he believed was his and his fellow economists' social role: "I hope we can become 'social engineers'" he had stated a few years earlier (Marschak 1941, 448). All this reflected the socialist background he shared with several other European emigrés from the Cowles (see Mirowski 2002, ch.5), or more precisely their desire to ground government intervention/ planning in scientific research. The specification-identification-estimation-testing procedure was designed with this purpose in mind, so that Cowles-style econometrics embodied a certain conception of the state from the beginning.

Friedman's econometrics belonged to a different tradition. The methodology of his study of the monetary history of the United-States, including his emphasis on dynamics, understood as lags in adjustments (Friedman 1951b,114), his work with time rates of changes rather than variable levels, his first difference detrending technique and his breaking up of times series in permanent and transitory components all bore the mark of the NBER. Likewise, his preference for single equation models owed to the NBER tradition and to his past work on alloys at the SRG (Friedman 1998, 143). Unlike Cowles econometrics, the NBER methods, created by progressive economists such as Mitchell and Kuznets, did not embody a distinctive policy orientation, and remained pragmatic and adaptable enough to accommodate Friedman's own worldview. His concern for self-stabilizing mechanisms and the disturbing effect of government intervention prompted him to borrow some mathematical patterns he had encountered years before in non-economic work at the SRG. While preparing a report of shotgun errors in 1943, he was introduced to the nascent research on control engineering. In the models he studied, time lags and feedback loops implied that external action upon systems can be destabilizing (Klein 2007, 22). The NBER tradition of looking for peaks and

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<sup>25</sup> Friedman helped Rutledge Vining's draft a reply to Koopmans, but he refused to be publicly acknowledged (Vining to Friedman, 01/11/48, MFA Box 34 Folder 33).

<sup>26</sup> For a detailed exposition of the Cowles method, see Christ 1994 and Epstein 1987.

<sup>27</sup> Friedman to Willits, 26/09/47, Box 35 Folder 16, MFA, see also Mirowski 2002, 215-220).

lags patterns in the data helped Friedman transplant these mathematical models into economics, thereby enabling him to highlight the potential destabilizing character of discretionary policies. Also, since he had no desire to improve government interventions he believed were inefficient, there was no need that the variables in his equations be controllable nor that their endogeneity or exogeneity be precisely specified.

It was precisely these features of Friedman's methods that drew fire from his Keynesian opponents. Nothing ensured that Friedman's variables, exogenous in his single equation models, would remain so in a model of a whole economy, Modigliani and Ando (1965, 693) complained. "The Friedman and Meiselman game of testing a one-equation one-variable model.... cannot be expected to throw any light on such basic issue as how to our economic systems work, or how it can be stabilized", they worried.<sup>28</sup> In the same vein, Karenken and Solow 1963 challenged the definition of money Friedman used in his 1961 estimation of the lags in monetary policy and recommended that the money variable be something that the Federal Reserve controlled directly. They insisted that *caeteris paribus* conditions be met and that the identification of money supply and money demand factors be tackled. They also warned that a bare leading correlation between the peaks and troughs of monetary and income series could to no avail stand for a demonstration that changes in the money supply *cause* business cycles (foreshadowing Tobin's 1970 famous *Post Hoc Ergo Propter Hoc* critique). As pointed out by Hammond 1996, Friedman was satisfied with a grey judgment on the "largely independent role of money" in lieu of the clear-cut causality Keynesians needed to implement their interventionist agenda.

### ***The crucial role of historical evidence and its dependence upon private beliefs***

What Friedman's challengers reproached him with, finally, was his inconsistent position toward econometrics. In a 1940 review of *Business Cycles in the United States of America*, Friedman had already faulted Tinbergen for selecting the explanatory variables he knew would provide the highest correlation coefficient, thereby rendering the statistical tests of significance meaningless. Instead he proposed to follow Mitchell's requirement that empirical work "must be judged, not by the coefficients of correlations obtained within the period for which they have manipulated the data, but

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<sup>28</sup>On the Friedman-Meiselman versus Ando-Modigliani controversy, Modigliani (1989,578) later noted: "I must acknowledge that the difference in parameters is partly the results of prior beliefs of ideology...There is obviously an ideological bias in assessing the values of the parameters...we end up with somewhat different estimates of the same thing."

by the coefficients which they get in earlier on later periods” (Friedman 1940, 660). Yet, as pointed out by Mayer (1972, 59), in the *Theory of the Consumption Function* Friedman assumed the PIH was true and then adjusted the data to fit his hypothesis. Furthermore, the key prediction of the PIH – that the income elasticity is zero for transitory income and unity for permanent income, was never put to test. Diesing (1985, 65-69) also noted that, depending of the results of the tests for the PIH, Friedman resorted to a range of data adjustments tactics He sometimes exaggerated the fit with theory, invented ad hoc explanations for divergences or rejected some data as unreliable or expressed puzzlement for the discrepancies. Several reviewers similarly pointed out the lack of modern tests other than on goodness of fit in his other works (see for instance Klein 1958,543). When, in 1951, Karl Christ used W. Marshall's new technique of out-of-samples testing to assess Lawrence Klein's macrostructural model of the US economy, Friedman was one of the few in the profession to take notice in a review of Christ's paper (see Qin 1993, 138-9). It is therefore surprising that he did not subsequently make use of those tests which apparently fitted his methodology perfectly.

These inconsistencies manifest Friedman’s ambiguous position toward empirical work (exhaustively documented in Leeson 2000, ch.2). In spite of his advanced statistical training and influential contributions to the field, he did not believe that econometrics could corroborate tentative hypotheses and yield causal relationships. Statistical evidence could be misleading, he warned, and high t statistics and R<sup>2</sup> were “a test primarily of the skill and patience of the analyst” (Friedman 1951b, 108). Friedman’s faith that the confrontation with fact brought scientific progress rather relied on the close analysis of historical evidence, and it was through historical narrative and counterfactual evidence rather than hypothesis testing that he intended to demonstrate the importance of money in business cycles through. As he later put in a letter to Robert Leeson:

“I would never have been comfortable with the conclusions reached if the only basis for them had been the statistical correlations we were presenting. However...I felt very confident in the evidence from history independently of the evidence from the statistical correlations, and hence regarded these as confirmatory rather than decisive evidence” (quoted in Leeson, 2000, 38).

For Friedman the varied character of economic history was a substitute for the lack of controlled experiments in economics: “the student of economic change... can observe monetary experience under sufficiently disparate conditions to sort out what is common from what is adventitious and to acquire considerable confidence that what is common can be counted to hold under still other

circumstances” (Friedman & Schwartz 1963, 676). Hence his interest in wartime and depression periods – they provide “precisely the kind of evidence that we would like to get by 'critical' experiments if we conduct them” (Friedman 1952b, 612). The crucial role ascribed to historical evidence, proceedings of central banks meetings or events such as the 1928 death of the governor of the New-York Federal reserve, one that reflected his ties with Mitchell's institutionalist vision (see Stapleford, this volume), thus provided an ultimate channel whereby his distrust of the state informed his science. Indeed, his interpretation of such evidence was itself influenced by his worldview. For instance, the record shows that Friedman began his study of the monetary history of the US with a clear picture of the role of the Fed in the Great Depression already in mind. During a 1947 Mont Pelerin conference, Friedman approved a fellow economist' remark that “the history of the 1920s shows monetary discretion at its best” with an explicit statement: “I agree. The big error in Fed policy was that of 1931.”<sup>29</sup> After spending the summer of 1948 reading of monetary theory and history, he then wrote to Walter Stewart, chairman of the Rockefeller Foundation Board of trustees, that “the one general conclusion I came up after going through this material and also some of the more recent material on the Federal Reserve, was the hunch that the Federal Reserve System had on the whole made matters worse rather than better. This is of course a pretty dogmatic statement, and I don't by any means feel utter confident in it. It is also a statement entirely at variance with what I would have said in advance, so at least it derives from the evidence.”<sup>30</sup> No wonder that Peter Temin, an economic historian and author of a history of the Great Depression, subsequently noted that “their [Friedman and Schwartz's] narrative...assumes the conclusion and describes the Depression in terms of it; it does not test it or prove it at all” (Temin, 1976, 15-16). Much later, in 1977, Modigliani also concurred that the source of his disagreement with Friedman ultimately was their different views on government intervention:

« Value judgments end up by playing a role in your assessment of parameters and of the evidence we consider... And there is no question that Milton and I, looking at the same evidence, may reach different conclusions as to what it means. Because, to him, it is so clear that government intervention is bad that there cannot be an occasion where it was good! Whereas, to me, government discretion can be good or bad” (Modigliani, 1977, 10).

Not only were Friedman's theoretical hypotheses informed by *a priori* beliefs about the stability of the economic system and the harmful effects of government intervention, but the very process whereby he confronted these hypotheses with facts -the collection of data, the choice of

<sup>29</sup> *Proceedings*, Box 87 Folder 10, MFA. This archive may have been misfiled and may be part of the proceedings of a 1948 conference organized within the Free Market Study.

<sup>30</sup> Friedman to Stewart, 12/01/49, Box 33 Folder 35, MFA.

discriminating historical events, the modelling and the interpretation of tests - also appeared undetermined and inconclusive enough to allow Friedman's values to enter the inquiry.

## **Friedman's policy recommendations between science and politics**

Friedman reflected on policy both as a scientist (see for instance Friedman 1948; 1952a; 1959), in line with his view that positive economics should comprise "the art of economics" (Friedman 1953a, 5; see Stapleford, this volume, for a discussion of the status of the "art of economics" in the 1953 essay), and as a political activist – such as in *Newsweek* chronicles, books like *Capitalism and Freedom* (1962) or his *Free to Choose* TV show in the eighties. And it is when Friedman reflected on economic policy that the consistency between his science and politics was the most striking; whatever the sphere they belonged to, his proposals systematically favored free market solutions over government intervention. If the boundaries between the two spheres become blurred in that in-between area, it is, we argue, both because the art of economics entailed the use of extra-scientific hypotheses and because Friedman sought to ground his neoliberalism in his scientific expertise.

### ***How scientific policy discussions make room for private values***

Friedman considered the "art of economics" as directly deriving from positive analysis because he believed that the economist did not need to choose the social "ends" to be pursued and could therefore remain detached when discussing policy proposals: "differences about economic policy among disinterested citizens derive predominantly from different predictions about the economic consequences of taking action... rather than from fundamental differences in basic values", he argued (Friedman 1953a, 5). He accordingly got into the habit of stating the "agreed ends" in the title or the first sentences of his articles. For instance, "A Monetary and Fiscal Framework for Economic Stability" (1948) began with the statement that: "the basic long-run objectives, shared I am sure by most economists, are political freedom, economic efficiency, and substantial equality of economic power... I believe-and at this stage agreement will be far less widespread- that all three objectives can best be realized by relying, as far as possible, on a market mechanism within a "competitive order" to organize the utilization of economic resources"(Friedman 1948, 246). Friedman seemingly saw no contradiction between ends such as price stability and employment, or efficiency and equality, and his 1948 paper precisely aimed at showing that, if price and wages were

flexible, a policy proposal built for long term stability *also* provided the best short run regulation of the cycle.<sup>31</sup> Yet, most of his colleagues found the idea of a general agreement on values extremely naïve. Albert Hart, a Columbia Keynesian economist trained at Chicago, remarked in a letter to Friedman that “giving priority to “price flexibility” measures over employment strikes me as self-defeating through intensifying the forces which make prices misbehave.” Possibly influenced by such comments and by Rose Friedman (1976b,22)’s opinion that “it is possible to predict an economist’s positive view from my knowledge of his political orientation,” Friedman later acknowledged a possible “inconsistency” between alternative ends and a “difference in time perspective” as regards their selection (Friedman 1977, 12, Friedman 1968). And his own preference for “the long view” over the short run undoubtedly rested on his belief in individual rationality and the system’s stability.

Reflecting on his lifelong policy disagreement with Franco Modigliani, Friedman (1977, 12) also conceded a second source of disagreement: “our judgment of the way in which policy is formed, operates, and develops.” Indeed, a policy proposal includes the choice of the institutional arrangement in which the causal sequence will take place, one that is rooted in the economist’s vision of current institutions and the level of changes acceptable by public opinion. With regards to institutional matters, Friedman’s position was ambiguous. In a 1953 rejoinder to Henry Oliver on *Economic Advice and Political Limitations*, he insisted that “economists should put politics aside in discussions of public policy” (see also Reder 1982, 26). As a matter of fact, the policy proposals outlined in his “Monetary Framework” were, by his own admission, designed as “a rational economic program for a free enterprise system” (1948, 254) in which prices and wages are flexible, a restriction his reviewer John Neff (1949, 947) saw as a major weakness. In the same vein, when Hart derided Friedman’s 100% reserve proposal as “utopian blueprints,” the latter replied that “one should set down the broad outline of the appropriate social framework without compromise. The compromise will come anyway.” At the same time, Friedman (1953c, 252) also claimed that the economist should take into account “the realities of political life” since they “in part determine what the effects of a policy will be.” His policy recommendations systematically took as a premise the belief that government intervention is an inefficient means to achieve whatever ends are considered, including employment and equality. Accordingly, the stumbling block of his disagreement with Neff on “The Monetary Framework” was “the relative merits of automatic and discretionary action” (Friedman 1949b, 942). Against the possibility of discretionary intervention, Friedman (1948, 255)

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<sup>31</sup> In a famous 1977 public debate with Modigliani again, he categorically refused to attribute their disagreement to the weight they respectively attached to inflation and unemployment (Friedman, 1977,12).

held that government intervention was plagued by a recognition lag, an action lag, and an effect lag. Inspired by Simon 1934, he consequently advocated a system in which these lags would be reduced through the operation of automatic stabilizers -a predetermined countercyclical deficit funded by money creation, coupled with a 100% reserve requirement in order to eliminate private creation of money and the discretionary control by the central bank. His opposition to fixed exchange rates was similarly underpinned by a wide range of anti-interventionists arguments. In 1948, he pointed to the possibility that the government makes larger mistakes with respect to the exchange rate level than private market operators, he also warned that government officials could display speculative tendencies. And he fought Lionel Robbins' skepticism toward free exchange rates with the observation that government would never “submit to the harsh discipline of any standard involving rigid exchange rates when it conflicts when the demand of full employment,” because of the pressure of public opinion.<sup>32</sup>

The content of Friedman’s proposals also derived from the influence of the *zeitgeist* of the neoliberal community. The sessions Friedman attended at the founding meeting of the MPS included one on “Contra-Cyclical Measures, Full Employment and Monetary Reform”, another on “Free Enterprise or Competitive Order”, and those devoted to the drafting of the “Statement of Aims.” Its final version included the redefinition of the functions of the state with the underlying idea that the conditions for the existence of a free society and a competitive order should be constructed, the idea that government intervention should be limited by “the rule of law,” that it should be possible to establish minimum living standards by means not inimical to the functioning of markets, that the misuses of history for collectivist argumentation should be challenged, and that MPS members should work to design an international order safeguarding liberty (Harwell 1995, 49-50; Van Horn and Mirowski 2009, 159-160). Five months later, Friedman presented a draft of “A Monetary Framework” to the Econometric Society. The coincidence between it and the MPS manifesto is striking: “(1) Government must provide a monetary framework for a competitive order since the competitive order cannot provide one for itself. (2) This monetary framework should operate under the ‘rule of law’ rather than discretionary authority. (3)... general fiscal measures (as contrasted with specific intervention) are the most desirable non-free market means of decreasing

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<sup>32</sup> Hart to Friedman, Friedman to Hart, 4/08/1947, Box 38 Folder 13. MFA. “1947 Proceedings”, Box 87 Folder10, Mont Pelerin Archives. Friedman to Robbins, 22/02/52, Box 32 folder 6. The comparison of these arguments with those given to Barry Goldwater, the Republican Party's nominee for the 1964 presidential election, illustrates how Friedman’s scientific and political reflections eventually converge in the same policy proposals: “Such a measure is a direct restriction on individual freedom, no less so if done in the name of saving dollars than if done, as the Russians do, to keep their citizens from contract with the rest of the world.... [it was] invented by Schacht and first introduced in Germany in 1934.”, Friedman wrote (Friedman to Goldwater, 12/12/60, Box 27 folder 24, MFA).

inequality” (Friedman 1948, 246). The proximity in time suggests that Friedman’s worldview was developed within the broader intellectual and political movement that gave birth to the MPS, but also makes it difficult to assess whether Friedman was influenced precisely by what he heard at the sessions.

In policy discussions also, Friedman’s worldview operated as a heuristic for the defense of new institutional arrangements, for the reshaping of the relationships between the state and economic individuals. But the record of Friedman’s participation to the rebirth of liberalism also suggest that, within the political sphere, his (and Chicago’s) vision of the task specifically relied on scientific expertise, another characteristic shared by many MPS members.

### ***A scientific demonstration of the superiority of neoliberalism?***

From the beginning, the MPS was divided on what strategy to adopt to modernize and spread liberalism (Hartwell, 1995, 103), an intellectual division intensified by the cultural/geographical one (Mirowski 2009, 442). While the old European guard--led by the Swiss businessman Albert Hunold, a financial benefactor of the society--saw the MPS as a platform for political lobbying, Friedman and the other Chicago economists involved in the society insisted that the defense of neoliberalism should be grounded in scientific analysis. Friedman’s viewpoint largely contributed to blur the boundaries between his science and politics.

The connection between science and neoliberalism was already implicit in his first talks on the subject, given in Paris and London during his journey around Europe in the early fifties. In them, he explained that socialists and collectivists had failed on the means advocated rather than the ends pursued, on which there is wide agreement on ultimate values in western societies:

The major fault of the collectivist philosophy that has dominated the western world is not in its objectives – collectivists have wanted to do good, to maintain and extend freedom and democracy, and at the same time to improve the material welfare of the great masses of the people. The fault has rather been in the means. Failures to recognize the difficulty of the economic problem of efficiency coordinating the activities of millions of people led to readiness to discard the price system without an adequate substitute and to a belief that it would be easy to do much better by a central plan. Together with an overestimate of the extent of agreement on detailed objectives, it led to a belief that one could achieve widespread agreement on a “plan” couched in precise terms and hence avoid those conflicts of interests that could be resolved only by coercion. The means collectivists seek to employ are fundamentally inconsistent with the ends they seek to attain.<sup>33</sup>

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<sup>33</sup> “Neoliberalism and its prospects”, Box 42 Folder 8, MFA, later published in the Mont Pelerin’s journal *Farmand*.

If Friedman disagreed with socialists and interventionists about the means, then such disagreement could be solved through scientific inquiry. The opportunity to connect scientific research and political thinking more explicitly came in 1955 when John Van Sickle and Benjamin Rogge from Wabash College asked Friedman to give a series of conferences on the principles of economic liberalism and on its policy applications. In reaction to the success of Kenneth Galbraith's *The Affluent Society*, Rose Friedman later gathered these lectures in a book; *Capitalism and Freedom* (hereafter C&F) would sell more than one million copies.<sup>34</sup> Unembarrassed with giving his thinking elaborated philosophical foundations, Friedman directly began his opening lecture by setting the preservation of freedom as the main end for policies, one that, in accordance with his past experience of being part of a racial, intellectual and political minority, he defined as "freedom from coercion."<sup>35</sup> This negative definition of freedom (see Smith 1988) received no further justification nor elaboration. He then argued that the best protection from the coercion of majority is the market, since it works as a "system of proportional representation" which provides coordination without standardization and a "check" to political power. He offered as an example the case of McCarthyism, where the academic market of labor had succeeded in protecting scientists against discrimination. However controversial, this example was more than a rhetorical device designed to shock the audience and genuinely reflected his understanding of the society he lived in. His choice seemingly derived from a lengthy private exchange with fellow economist, MPS member and friend Fritz Machlup. When the latter confessed his refusal to vote for Eisenhower because he received McCarthy's support. Friedman claimed Eisenhower's election would turn McCarthy into a "harmless snake."<sup>36</sup> By assigning a political function to the market, Friedman brought his political analysis to rely on his economic expertise. And by pointing out that "a necessary condition for individual freedom is the organization of the bulk of economic activity through private enterprise operating in a free market—a form of organization I shall refer to as competitive capitalism" (chapter 1 of C&F), he brought the debate into his area of expertise. In a last twist, he further narrowed his concern to the question of the redefinition of the role of the government in relation to markets (chapter 2 of C&F): "how can we keep the government we create from becoming a Frankenstein that will destroy the very freedom we establish it to protect," he asked (Friedman,

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<sup>34</sup> Friedman identified Galbraith as one of his main opponents and devoted much energy to counter his views. He wrote a pamphlet entitled *From Galbraith to Economic Freedom* (1977), and counterattacked Galbraith's 1977 TV series *The Age of Uncertainty* with the *Free to Choose* show, broadcasted in 1981.

<sup>35</sup>"All of us here are accustomed to being members of an intellectual minority, to being accused by fellow intellectuals of being reactionaries or apologists or just plain nuts", he characteristically emphasized in a 1976 MPS speech (quoted in Frazer 1988, 1972).

<sup>36</sup> Friedman to Machlup, 28/10/52, Box 38 Folder 25.

1962, 2).

Turning the case for liberalism into a problem of weighing the benefits of markets against those of government intervention allowed Friedman to switch most of the argumentation from the political to the scientific level. In C&F, he first and foremost intended to restore “true facts,” by relying on the evidence uncovered in his and his colleagues’ empirical research, and to derive policy proposal from this adequate representation of the economic world. Almost each chapter surveyed an issue previously dealt with in some academic articles. Friedman for instance used his monetary research to argue that “*the fact is* that that the Great Depression, like most other periods of severe unemployment, was produced by government mismanagement rather than by any inherent private instability of the private economy” (Friedman, 1962, 38, emphasis added), and then referred to his 1948 “Monetary Framework” article to support his proposal of a rule of action. The chapters on “international financial and trade arrangements”, “fiscal policy”, “occupational licensures”, “the distribution of income” echoed his work on floating exchange rates (1953d), excise and income tax (1942, 1952c), and incomes from independent professions (1945). The chapter “Monopoly and the Social Responsibility of Business and Labor” made extensive use of Nutter’s 1951 study to insist that “*the most important fact* about enterprise monopoly is its relative unimportance from the point of view of the economy as a whole”(Friedman, 1962, 121-25, emphasis added).

On the basis of the ideas that monopolies tend to be suppressed by competitive forces, that businessmen have no social responsibility and that corporate income tax should be abolished, all contained in C&F, on the basis of the Free market Study, and of the funding of both projects by the Volker Fund, Van Horn 2009 and Mirowski and Van Horn 2010 assert that Friedman’s (and Chicago) liberalism was “nominally anti-statist but in practice pro corporate.” Our impression is that, on the contrary, Friedman’s stance was anti-statist rather than pro-corporate, if only because Friedman's view of monopolies was paired with the symmetrical belief that the impact of unions on wages was overestimated. Also, while Friedman believed that public monopolies were more harmful than private ones, he harshly condemned both. It is indeed doubtful that Volker officials endorsed Friedman's denunciation of government support of the railroad, truck, gas, communication, radio and television, aeronautic and building industries through regulations, subsidies and tariffs (Friedman, 1962, 123-129), or his recrimination against the New-York Stock Exchange, A.T.&T. and American Oil companies in his *Newsweek Chronicles* (Friedman 1972, 285; 308-310). As Samuelson remarked in a 1976 *Newsweek* column, “the adjective ‘conservative’ does not do proper justice to a thinker who would refuse the steel industry its import quotas, strip Texas of its oil subsidies and deprive the railroads and the trucking interests of their protective

regulations” (cited in Friedman R. 1977, 27). As made clear by all his writings on the subject, Friedman had an interest in dealing with corporations and monopolies only inasmuch as they epitomized the drawbacks of government intervention. As for the repeated financial support of the Volker Fund, funding does not necessarily mean influence, and the *Roofs or Ceilings* dispute mentioned in previous sections fully testify to Friedman’s intellectual independence with regard to funding bodies.

Friedman’s political writings thus displayed a strong economic bent and a willingness to ground his arguments in scientific empirical research, one that can account for the consistency of his academic and political policy proposals. This scientific flavor was also apparent in subsequent projects and even became an asset for supporting funding bodies. To publicize their free society ideals, Friedman, Stigler, Hayek, Rogge and Van Sickle created a collection of books entitled the “Principles of Freedom” series. With the help of journalist and petroleum consultant Ruth Sheldon Knowles, by 1963 they had secured funds from several big companies including Shell, the Continental and the New Jersey Oil companies, U.S. Steel, General Electric, Shell Oil, DuPont, Ford Motor Cnie, some of which Friedman had criticized in C&F and would later attack again in *Newsweek*. What appealed to these subscribers was that their ideological leanings could be supported by scientific expertise. The members of the advisory committee were recruited among well-known intellectual, including among others Maurice Allais, Knight Gottfried Haberler, John Jewkes, Karl Popper and L. Von Mises, and the brochure designed to raise funds which Friedman extensively contributed to draft, emphasized scholarly orientation:

We propose to enlist the collaboration of distinguished scholars in the preparation of a series of books dealing with important, controversial and misunderstood economic and political issues of the days....The series will be designed for a number of audiences: young undergraduate students, businessmen, labor leaders, politicians, journalists, and the informed public. In order to reach these audiences, every volume should combine the highest quality of scholarship with lucidity and brevity. To this end we plan to bring together, when needed, the skills of scholars and professional writers.<sup>37</sup>

All the participants to the “Principles of Freedom” project but Knowles were MPS members. Yet, the influence of the society of Friedman's political thinking is again difficult to assess. Friedman was a background character in the establishment of the society. He only intervened during general sessions to emphasize “that liberalism has a humanitarian aim and is a progressive

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<sup>37</sup> RSK to Rogge, December 1963; RSK to Committee 04/03/64, Box 88 folder 5; RSK to Van Sickle, 08/08/65, Box 98 Folder 5, MFA. Knowles also characteristically remarked that “they [underwriters] feel our project is what should be done because of the scholars on the Committee.”

philosophy.”<sup>38</sup> And he remained so in the first decade of existence of the MPS. He attended only 3 of the first 10 meetings (the founding one, Bloemendaal 1950 and Princeton 1958), and only became a regular participant in 1960.<sup>39</sup> Friedman, Stigler and others took over the leadership of the society in 1961 after the tumultuous resignation of Hunold and the withdraw of Hayek (see Hartwell 1995). Admittedly some of Friedman's political ideas matched those of the MPS, such as the insistence that the functions of the state should be redefined, not destroyed (Mirowski 2009, 436). However, some of his other political ideas, such as his exclusive focus on the economic dimension of political issues, conflicted with the Hayekian/Austrian wing of the society. In sum, I view MPS as a catalyzer rather than a source of influence in Friedman's intellectual development.

## Conclusion

This account shows that the consistency between Friedman’s science and politics cannot be reduced to a deliberate or unconscious distortion of assumptions, models, results and policy advice to fit one’s political convictions. Our explanation highlights the complex interplay between the positive, ethical and methodological beliefs which developed from Friedman's private experiences and observations, intellectual influences, and empirically tested scientific hypotheses. His faith in the benefits of a free individualistic society, in the stability of market oriented systems, and his distrust of most forms of state intervention are the most salient features of his worldview. While these beliefs provided *substantive* hypotheses on the world that influenced his research, his methodological choices, in particular his reliance upon historical evidence rather than econometric procedures shaped the *channels* whereby these hypotheses survived the confrontation with facts (on which he grounded his scientific objectivity). Friedman’s worldview was in turn strengthened by these scientific confirmations, and therefore appeared as a solid basis in which he could ground his neoliberal ideology, thereby blurring the boundaries between his scientific activity and his political propaganda.

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<sup>38</sup> « Statement of Aims; Monday April 7th » (Box 5, folder 12, MPS archives). See also « discussion of Agenda, April 4th » (Box 5, folder 13, MPS archives)

<sup>39</sup> The triggering factor was Hunold's opposition to the membership of Seldon Harris and his discontent at the increasing role played by Harris's Institute of Economic Affairs in the management of the society.

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