

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

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“I believe that Friedman had a crusade that he was pushing all over the world, not just in the profession. He saw the big picture, and the big picture was right for him. He didn't really want to be bothered with these little technical problems. (Tobin in Klamer 1984, 105-106).

1. Introduction

“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 private values from interfering with his scientific practice. In public speeches, in scholarly and newspapers articles, as well as in private correspondence with colleagues, political figures and businessmen, Friedman always strove to distinguish between scientific and policy claims. And in so doing he 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 the Keynesian Abba Lerner to the Nobel Prize committee in 1976.¹

Judging from the stir created by his reception of the Bank of Sweden Prize in Economic Science, however, it seems that a number of economists were not convinced by Friedman’s detachment.² There was even suspicion that the consistency between his free-market scientific framework and his antistatist political recommendations was not merely accidental. While some economists simply pointed out that the positive and normative dimensions of the Chicago school of economics he is associated with are “distinct but closely related” (Reder 1982, 31), others have explicitly condemned the neoliberal character of Friedman’s market-oriented microeconomics.³ Likewise, his macroeconomic work has been interpreted as an attempt to shatter the pillars of American Keynesianism and its progressive implications, one realized at the expense of reliable theoretical and empirical foundations.⁴ 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; Wilber and Wisman 1976, 79; Nelson 2001, Lavoie and Seccareccia 1993, 16). Familiar with the Marx-Mannheim critique of economics, those critics have been more willing than economists to denounce the ideological dimension of economic science. Though the marxist concept of “ideology” has gradually been replaced with references to vision, paradigms, ethics, 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 Hodgson 2001 and Amadae 2003). In addition, the view of economics as a discourse has contributed to focussing attention primarily on the persuasion and rhetoric dimension of scientific achievements (for instance McCloskey 1984, Klamer and McCloskey 1994).

¹ Lerner to members of the Alfred Memorial Prize committee, undated (probably 1973), Abba Lerner Archives, Box10 folder 7.

² On the troubles surrounding the Nobel Prize, see Friedman’s wife account (Rose Friedman 1977, 20). On the side of critics one finds Kaldor (1978, viii) and Myrdal, (1977, 52).

³ See for instance Bhagwati 1977’s depiction of Friedman as an “ideologue” or Rapping in Klamer 1983.

⁴For instance, the Keynesian economist Franco Modigliani stated 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). See also Culbertson (1964, 375)’s accusation of a “deliberate misrepresentation of the truth” in his review of *A Monetary History*, and Modigliani 1977, Kaldor 1981, Desai 1981, Hendry & Ericsson 1983, Hahn (1984, 1985).

The above critics provide no detailed account of the process whereby political values may have influenced Friedman's science.⁵ Given that Friedman, as a statistician, subjected his hypotheses to a demanding process of confrontation with facts, the permanence of his private values in his science calls for further explanation, especially when it is remembered that the empirical part of his work was the most criticized.⁶ Likewise, the possibility that scientific conclusions and political values are consistent because of the influence of the former on the latter, as Brunner (1976) or Friedman (1998) have claimed, is left unexplored. The focus on political beliefs at the expense of cognitive and methodological emphasized the persuasion, social control and ideological functions of economic theorizing at the expense of its goal of understanding the world. As a result, it becomes difficult to separate between the role of ideology in the *application* of a theory (a case well documented) and in its *formation*. Also, as David Colander (2005, 11-12) has noted, if it is now widely acknowledged among economists or at least historians that economic research is inescapably pervaded by ideology, then the consideration must be shifted to "intentional ideological bias", "bias that exceeds that ideological background level of ideology that affects us all."⁷ Assessing the interdependence of values and science in Friedman's work may thus require that special attention be given to his intentions.⁸ This calls for investigation of the way the ideas he would untiringly advocate during a great part of his career were initially formed. And this investigation calls for a framework enabling to disentangle Friedman's true beliefs and his propaganda strategies.

In this paper, we consider economic research and political activity as just two ways of making sense of the world. They yield some beliefs, political and ethical, but also cognitive (on how people actually behave and society actually works) and methodological, which may be used in other activities. Reciprocally, the economist's research is likely to be influenced by convictions deriving from his psychology, religion, art, encounters, readings, life experiences, reactions to collective history. An account of the consistency of Friedman's science and

⁵ Warren Samuels suggests that theories are value laden because Chicagoans assume hypotheses taken from an *a priori* paradigm (the superiority of market solutions) and only pay attention to evidence consistent with that hypothesis (Samuels 1976, 374). Wilber and Wisman (1976, 85) and Reder (1982,13) outlined a similar treatment of empirical findings

⁶ Mayer 1972 and Diesing 1985 closely investigated Friedman's hypothesis testing and data interpretation procedures, but they carefully refrained from drawing conclusions on the value-ladenness of Friedman's results.

⁷ 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 crossing line", Colander (2005, 22) concludes.

⁸ The doubts levelled against Friedman's intellectual honesty were so serious that his best friend and colleague George Stigler (1988, 154) had to hammer out in his autobiography that "Milton is in no way a trickster. He believes what he says and says what he believes."

politics should therefore bring into the story this wide array of beliefs. These beliefs are neither necessarily conscious nor consistent with each other, and they are transformed, adjusted, corrected along the economist's life. In line with phenomenology and hermeneutics, we will call this set of beliefs a worldview.⁹ The thesis of this paper is that Friedman's scientific and political conclusions were consistent because they were jointly developed from a strong and consistent worldview, one that was framed early in his life and constantly reinforced afterwards.¹⁰ This worldview relied on the idea that individual freedom should prevail over every other value, but also on cognitive and methodological beliefs that proved crucial in shaping Friedman's scientific and political outlook. Friedman *saw* his own life experiences and historical events as factual evidence that people are rational and autonomous, that economic and social systems are inherently stable and self-stabilizing. His optimistic belief in individual rationality was paired with a deep distrust in all forms of collective action, especially governmental intervention. The idea that science helped reach consensus among citizens and that objective knowledge was achieved through the confrontation with facts were also crucial in the development of his worldview. Some of these beliefs originated from his private history and temperament, others from his studies, professional encounters and experiences. A peculiar feature of this worldview is the extent to which it drew on economic concepts.¹¹ It is on these same core beliefs that Friedman simultaneously built a scientific and a political system in the fifties and sixties.

This paper spans the years 1933 through 1963.¹² Section 1 describes the origins and characteristics of Friedman's worldview, from 1933, when he entered Chicago as a graduate

⁹ We could alternatively have used the words mental lenses, ultimate cosmology, patterns, forms of thoughts, *gestalts*, world picture (Wittgenstein), forms of consciousness (Hegel), fore-structure (Heidegger), or simply vision. 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 distinction between the concepts ideology and worldview simply helps distinguishing between an approach based on collective values, social control and propaganda from one based on a wide range of individual essential beliefs.

¹⁰ The overall consistency of Friedman's worldview does not preclude specific inconsistencies between his beliefs, or his words and deeds. For instance, note his defence of a public money supply despite his dislike of government intervention, his fight against labour unions despite their purported limited impact on wages, his disinterest in the threat McCarthyism was for the academic freedom he cherished.

¹¹ By his own admission, Friedman "lived, breathed and slept economics" (Friedman 1998, 149). In particular, he *viewed* every kind of behaviour, not merely economic, through the lenses of supply and demand categories.

¹² Stopping in 1963 left some of Friedman's major contributions -the reformulation of the Phillips curve and the natural rate of unemployment hypothesis- and some major controversies -on his role in Chile, on the Hendry-Ericsson paper- untouched. Some evolutions in the seventies and eighties however make it more difficult to discriminate the substance from the rethorique and propaganda. Friedman's strategic attempt to translate his marshallian theories in the walrassian framework of his opponents, his disappointment with Nixon's policy and

student, to 1946, when he returned there as a teacher. It was in this formative period that he built his methodology, his economic-based conceptual framework and his political liberalism. The paper then concentrates on the period ending up with the publication of *Price Theory: A provisional text*, *A Monetary History of the United States*, and *Capitalism and Freedom*. Section 2 studies the influence of Friedman's worldview on his scientific thinking. It is shown that Friedman's values informed his choice of hypotheses, his understanding of the "facts" (data series and historical evidence) he confronted with these hypotheses, and eventually his kind of econometric modelling built and testing procedure. Since part of the claim that economics is value-laden arises from its proximity with the political process, section 3 examines Friedman's "art of economics", the domain that stood between science and politics. Friedman reflected on policy matters partly as a scientist, in scholarly journals and conferences, and also as a political activist, in public speeches, newspapers chronicles, and Mont Pelerin Tribunes. We show that, although they were never confused in Friedman's writings, these two kinds of policy recommendations were closely intertwined.

2. The making of a worldview, 1933-1948

Friedman was born in 1912 in Brooklyn, the son of poor Jewish emigrants who worked in sweetshops until they could open a small store.¹³ His father died in 1928, and the sixteen year-old boy found himself compelled to work to pay for his studies at Rutgers University. His self-supporting situation in the middle of the Great Depression, his sanguine character and his self-confidence imparted him a thorough intellectual independence, not to say irreverence, and a sense of autonomy.¹⁴ At Rutgers, he was naturally drawn to Homer Jones, a graduate student from Chicago, whose free enterprise bent echoed his private experience. Friedman specialized in mathematics and statistics and also attended a few courses in economics and business cycles taught by Arthur Burns, then PhD student at Columbia with Wesley Mitchell. A devotee to the pursuit of truth and objectivity, Burns introduced Friedman to the empirical

his neoliberal radicalisation, the beginning of the Reagan-Tatcher era and hardening of keynesian critiques against monetarism all cloud the picture.

¹³ The biographical material is taken from Friedman 1998, Hammond 1993, Silk 1976, and from the material and vitae found in the Milton Friedman Papers (hereafter MFA), Hoover Institution, Box 5 Folder 11. Friedman later interpreted his parents' history as an example of the benefits of the American free enterprise system (see Friedman 1998 and the script of the first *Free to Choose* TV episode, Box 61 Folder 9, MFA).

¹⁴ "Friedman may not have believed in God, but he believed in man. If the word misanthrope had an antonym, that would describe Friedman", Dan Hammond (2007, 25) argued in a paper on the transcendental commitments of economists. See also Frazer (1998, 19-20).

method that was the hallmark of the NBER and to marshallian price theory. This twofold connexion with Chicago and the NBER Friedman entertained for his whole life. He attended the courses of Jacob Viner, Frank Knight and Henry Schultz at Chicago, but he also spent one year studying statistics and mathematics at Columbia with Harold Hotelling and Wesley Mitchell. Friedman's PhD dissertation consisted of a NBER study of the income determinants of independent professional practice, conducted in 1937-1940 under the leadership of Simon Kuznets. As a consequence of his training, Friedman initially saw himself as a statistician as much as an economist.¹⁵ During his formative period, Friedman encountered a wide range of scientific perspectives: Marshallian price theory in Viner's courses, statistics and empirical work at the NBER and the National Resources Committee in 1935-37, the institutionalism at Columbia and Wisconsin, policy oriented research at the *Division of Tax Research* at the *Treasury* in 1941-43, and applied mathematics and statistics at the *Statistical Research Group* in 1943-1945. The political opinions encountered also spread over a broad spectrum, from Knight's pessimistic liberalism to Simons's qualified laissez-faire position, and from Mitchell and Kuznets' progressivism to Washington economists' interventionism. From these various perspectives, Friedman gradually fashioned the methodological principles, the Chicago style conceptual framework, and the liberal political leanings that later formed the cornerstone of his scientific work and political commitment.

2. 1. Friedman's vision of science within society.

The release of Friedman's famous essay "The Methodology of Positive Economics" (1953a) initiated a fifty years inexhaustible stream of literature. For Friedman however, it was the final point of fifteen years of methodological thinking, which developed in close association with his vision of society as a whole.¹⁶ Following Knight, Friedman viewed society as cooperation between rational individuals endowed with diverging preferences and disagreeing about social goals and the means to achieve them. More optimistic than his teacher, Friedman did not retain his insistence on the pervasiveness of value conflicts and on the necessity to

¹⁵ Until the end of the war, some positions he held relied exclusively on his statistical training (such as his SRG job) or at least required a strong command of statistical techniques. During the year he spent at Wisconsin University in 1940-1941, he taught statistics, and later contributed to a statistical textbook (Friedman, Freeman and Mosteller 1948).

¹⁶ By the end of Friedman's formative era, upon return to Chicago in 1946, his methodological views were already almost settled: he wrote the first draft of the essay, *Descriptive Analysis vs Analytical Relevance* during the summer 1948 (Hammond 2006, 155). After the release of his essay in 1953, Friedman would never publicly return to methodology or answer criticisms.

supplement economics with an ethical reflection (see Emmett 2007 on Knight's Chicago legacy). Friedman was indeed faithful in the power of ideas to change society, and exhibited a natural inclination for prozelytism and political activism (Nelson 2001, 140).¹⁷ He believed that scientific inquiry was the best means to solve disagreements among citizens, one that would preserve individual freedom, the key principle of a liberal society. Friedman's encounter with the statistician Jimmy Savage and his subjective probability theory in 1943 at the SRG fleshed these beliefs with a philosophical structure. As he later wrote to Friedrich Hayek,

[Your] repeated reference... to a "true, though untestable, explanation" impressed me with what seems a contradiction between such a statement and the praxeological principle it reflects and our belief in a free society in which voluntary cooperation is to be the foundation. We recognize that such a voluntary society rests on the establishment of a framework for settling differences of opinion by peaceful means....

I myself have been led increasingly to regard scientific method as a set of conventions for resolving disagreements by work in a different though related field, namely that of probability. In particular I have been much influenced by the work of an Italian, Bruno de Finetti, not directly but indirectly through his influence on James Savage... They 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 proceeded to use this concept as the basis for his book on The Foundations of Statistics. He argues that the true function of statistics is not to discover knowledge but to resolve disagreement among people on subjective p.¹⁸

Economic science was thus inherently policy oriented, Friedman concluded in the late thirties. But since the economist was also a citizen personally involved in the social issues he studied, this policy orientation entailed a possible confusion between scientific analysis and normative judgment. This threat surfaced in 1938, when the board of directors of the NBER, led by Reynold Noyes, opposed the publication of Friedman and Kuznets's work on the income of independent professional practice.¹⁹ The main bone of contention was Friedman and Kuznets's explanation of the particularly high average income of physicians as compared with dentists by the restrictions of entry in the medical profession. Noyes suspected that this

¹⁷ See also Niskanen's comment of *Capitalism and Freedom*: "one is struck by the sense of optimism about the ultimate power of ideas—a faith that closely reasoned arguments, an accumulation of evidence, and a leavening of wit will persuade most people and that our governmental processes will be responsive to their preferences" (quoted in Nelson 2001). Friedman's optimism can be contrasted with Stigler's pessimism that science could influence society: "from Smith to Keynes to Friedman, reformers have all be deluded", Stigler argued (quoted in Nelson 2001, 155).

¹⁸ Friedman to Hayek, 11/09/75, Box 20 folder 19, Hayek papers, Hoover Institution.

¹⁹To warrant their objectivity, NBER publications were allowed only if a "special reading" committee of directors granted their agreement (Rutherford (2005)).

statement was dictated by the authors' political beliefs. "The purpose of the Bureau is not to propose social reforms", he thus reminded them. By way of defence, Friedman and Kuznets argued that the economist's personal concern for the issue investigated did not challenge his ability to separate positive from prescriptive analysis:

Our study is... a factual investigation. But it has a distinct bearing on questions of policy. That is as it should be. Facts derive significance from the problems they help to solve... The present writers, for example, testify to a strong interest in the efficient allocation of resources and in the role that incomes play in such an allocation. We believe that our investigation has gained in significance from this fact. 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.²⁰

"Positive economics is in principle independent of any particular ethical position or normative judgment", Friedman (1953a, 4) concluded.²¹ There was indeed a scientific safeguard against the conflation of science and values, the confrontation with facts. Friedman's experience as a statistician, his endorsement of the NBER empirical method, and the example of the meticulous efforts of Henry Schultz to test demand theory all explained Friedman's insistence on empirical validation.²² During the 1940s, he refined his empirical requirements. The success of a hypothesis was not to be judged by its realisticness, he argued, but by the usefulness and relevance of its results, evaluated by conformity of its predictions with factual evidence and the discovery or prediction of new facts (Friedman 1940; 1953a, 9, 14-15).

Friedman's methodological principles matured alongside his confrontation with alternative theoretical frameworks. During the NBER affair, he was compelled to defend his

²⁰ *Memorandum by Noyes*, undated; Crum to Carlson, 27/08/42; Memorandum to the Directors, by F&K, undated 1943-1944, p25-26, Box 37 folder 24, MFA. See Also Friedman (1953, 4) especially the comparisons with natural sciences.

²¹ On the contrary, his wife Rose has always been convinced that it was possible "to predict an economist's positive view from my knowledge of his political orientation, and I have never been able to persuade myself that the political orientation was the consequence of the positive views" (Friedman R. 1976b, 22). By the later seventies, she seems to have won over her husband.

²² Friedman was Henry Schultz's research assistant in 1933-34. His methodological views stem from a peculiar combination of Chicago marshallian price theory and NBER empirical method. The same combination is found in Stigler, who also exhibited this dual institutional membership to Chicago and the NBER and their close friend Allen Wallis, recruited at the University of Chicago Graduate School of Business in 1946, and appointed its Dean in 1956. This explains Friedman's claim for a collective methodological stance: "In discussions of economic science, 'Chicago' stands for an approach that takes seriously the use of economic theory as a tool for analyzing a startlingly wide range of concrete problems, rather than an abstract mathematical structure of great beauty but little power; for an approach that insists on the empirical testing of theoretical generalizations and that rejects alike facts without theory and theory without facts." (Friedman 1974,11).

and Kuznets's use of the supply-demand framework to account for income differences between professions. Noyes interpreted this choice as an *a priori* theoretical bias in favour of the perfect competition hypothesis and, because of differences in abilities and equipments, he contended that the monopolistic competition framework was more appropriate for the study of medical professions. Friedman and Kuznets acknowledged that medicine and dentistry were characterized by product differentiation, but remarked that when it came to analyse the differences between professions, "so far as the specific problem is concerned, differences among the products of different physicians are less important than the similarities –else, why distinguish physicians from dentists?...For these problems we must use tools that emphasize the similarities rather than the differences among products, i.e., until better tools are developed, the Marshallian supply and demand curves". Friedman's early opposition to the theory of monopolistic competition was thus far from an *ex post* neoliberal justification of free markets. Coined with the anything but conservative Kuznets, it stem from a pragmatic view of economic theories - they depended on the problem under study, and the conditions under which they held had to be systematically specified.²³ Noticeably however, when Friedman took up the argument in a 1941 review of Robert Triffin's Harvard PhD dissertation on monopolistic competition, he conceded no "specific problem" in which the imperfect competition framework would be "more useful for concrete analysis" (Friedman 1941, 390).²⁴

It was at about the same time that Friedman was confronted with the Keynesian income expenditure theory.²⁵ At the *Treasury*, his work consisted of estimating the consequences of a tax rise on aggregate consumption and inflation. Although he performed his estimation in the framework of the Keynesian income-expenditure theory, Walter Salant, an economist from the *Office of Price Administration*, reproached him for not taking into account the effects of the Keynesian multiplier in his estimates. In his reply, Friedman 1942 argued that Keynesian theory was a very poor substitute for the numerous channels through which consumption behaviour impacted prices and pointed out its lack of empirical content. He gradually extended his charge of empirical emptiness and practical uselessness to

²³ "Memorandum in reply to Noyes", 1941, p.7-8, Box 37 folder 24, MFA. This vision of economic theories Friedman attributed to Marshall. His catchphrase that science is "an engine for the discovery of concrete truth" is borrowed from the *Principles* (Friedman 1949, 469; Friedman 1953, 35).

²⁴ Although Stigler's became Chicago's leading opponent of Chamberlin with his LSE Lecture "Monopolistic Competition in Retrospect" (1949), he appears to have been initially more favourable to monopolistic competition than Friedman. A graduate student believed that Chamberlin's work would revolutionize economics, and he substantially discussed the matter in his mid forties works (Leeson 2000, 62-63).

²⁵ He had already been introduced to Keynesian monetary theory by James Angell at Chicago, but he found neither originality nor heresy in that approach (see Steindl 2004).

Walrassian equilibrium theory, incidentally targeting leading advocates of progressive reforms and state intervention such as Oskar Lange and Abba Lerner (Friedman 1946, 1947). He eventually he collected the various theories he was critical of under the label “Walrassian economics”, which he contrasted with his “marshallian economics” purportedly in the NBER tradition and Marshallian price theory. Upon discussion with Stigler on imperfect competition in 1947, he started putting his various objections together (Hammond and Hammond 2006) and found the common denominator of his targets, namely the “retreat into purely formal or tautological analysis” (Friedman 1953a, 11).²⁶

Although his methodology aroused from the gradual confrontation with a wide range of theories, his various criticisms met in the defence of marshallian price theory and the perfect competition framework that would become the trademark of Chicago microeconomics. Friedman’s methodological thinking was indeed closely intertwined with the development of a positive vision of individual behaviour and of how societies and economic systems work. Grounded in Viner and Knight’s teaching, his conceptual framework quickly overflowed the scientific sphere and thoroughly informed Friedman’s outlook on the world. Individual rationality, market efficiency, economic stability and perfect competition appeared to him as facts rather than working hypotheses or ideals.

2.2. Market efficiency and the “competitive order” as facts

“I was impressed with his ability to get so much mileage out of an extremely simple view of the world”, Leonard Rapping, former student of Friedman, recalled (Rapping in Klammer 1983, 220). The core economic concepts underlying his worldview were initially taken from Viner’s modern and rigorous presentation of marshallian economics. These he articulated in the wider philosophical framework provided by Knight (see Knight 1933). From the latter, he retained the idea that laissez-faire and the preservation of freedom were to be favoured because alternative organizations worked worse. In particular, economic planning was doomed to failure since it presupposed unattainable perfect information. Friedman however rejected his teacher’s warning that the capitalist system was not the panacea, that the role of economic

²⁶ A 1952 letter to Richard Ruggles, a PhD student in methodology show how he put all his opponents in the same basket: “the increased prestige of speculative analysis, in turn, doubtless resulted from the apparent triumph of Keynesian analysis, a particular speculative analysis”, he wrote, mentioning as offspring of this success the growth of walrassian economics, monopolistic competition, institutionalism, and anti-marginalist studies (Friedman to Ruggles 28/06/51, Box 32 Folder 16, MFA).

motive in human affairs should not be overstated, and that the maximization hypothesis was unrealistic since preferences actually were emotional and unstable. Influenced by his early experience of autonomy and rationality, Friedman considered the maximization hypothesis as a good approximation of reality, one he applied to every situation, cultural and institutional difference notwithstanding.²⁷ For instance, in a paper on “Foreign Economic Aid: Means and Objectives” 1958 relying on the impressions gathered in a previous trip to India, Friedman argued 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 was in favour of a more active role of the government, made it 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.”²⁸

Another belief characteristic of Friedman’s vision was that economic forces tended to restore competition on the markets. Thus, monopolies could be considered minor and transitory exceptions and the impact of unions on wage was clearly overstated (Friedman 1951a, 16).²⁹ In his reminiscences of Friedman’s teaching, Rapping remembered that “many Chicago people would argue that the world is, in fact, competitive. They tend to believe their own pragmatic myth...I [too] believed that it was more than just a convenient assumption. I believed that it was descriptively accurate” (Rapping in Klamer 1983, 221). Again, this belief conditioned his view of the US economy. From France in 1951, he wrote Stigler that “by comparison, America is perfectly competitive –you don’t need to measure the degree of monopoly: it’s zero.”³⁰ Since competition was a matter of facts, Friedman thought that economists could demonstrate through objective and impartial empirical investigations that prices are flexible, that employment is within reach, that is, that the American economy

²⁷ As put by Samuels (1976, 7), “all are describing the world as they see it, and they sincerely believe that their ideal is...practicable.”

²⁸ Jacoby to Friedman, 03/09/58, Box 28 folder 27, MFA.

²⁹ This claim is difficult to reconcile with his recurring attacks on Unions, as pointed out by Friedman’s colleague Martin Bronfenbrenner: “If, as you maintain, it [union power] increases neither the aggregate relative share of labor nor the relative share of union versus non-union labor, and if it is not a force for inflation, then in God’s name what harm does it do?” (Bronfenbrenner to Friedman, 03/11/52, Box 21 folder 35, MFA).

³⁰ Friedman to Stigler, 15/01/50 [51], cited in Hammond 2006. Friedman attributed the preservation of competition within the US to American’s culture: “I believe...that the freedom to establish an enterprise is the fundamental element of the American belief in free enterprise. This fact is demonstrated very clearly by the American legal framework. Under the Sherman anti-trust law and later enactment, agreements in restraint of trade are per se illegal and unenforceable” (*Free Enterprise : An American View 1950*, Box 41 Folder 2, MFA).

displays the basic premises of a liberal economy. This was precisely the purpose of the “Free Market Study”, made up of members of the Law school and economic department among which Friedman, Knight, Henry Simons, and Aaron Director as its chairman. Established in 1946 on a grant from the conservative Volker Charity Fund, it intended to counter with scientific arguments the growing socialist critique of capitalism stemming from unprecedented waves of mergers, the enormous profits of the Standard Oil, the power of the railroads and the Steel industry (on the history of the ‘Free Market Study’, see Van Horn 2005).³¹ Friedman did not contribute much to the study, but he participated to the meetings and directed students associated with the project, among which Warren Nutter on an empirical evaluation of the importance of monopolies within the US.

If the American economy was competitive, Friedman thought, then coordination through market prices ran efficiently, and supply and demand categories were the most relevant conceptual framework to explain a wide range of phenomena. This framework was not merely methodological but genuinely ontological in Friedman’s worldview, and the range of issues he tackled with it far exceeded the usual boundaries of economic science.³² For instance, Friedman and Stigler early talked on considering children as commodities, and accordingly calculated “the cost of purchasing children in country and in city” in 1952, so as to explain the birth rate differential between countries and cities.³³ The pervasiveness of the supply-demand framework in Friedman’s thinking is best seen in his teaching. The testimonies left by Friedman’s students, such as Gary Becker, Karl Brunner, Georges Lucas, and Rapping all pointed to Friedman’s tremendous influence through the price theory course he taught between 1946 and 1963 (see Klamer 1983 and Hammond 1993). When asked what made Friedman a great teacher, Becker reminded that “he... gave numerous illustrations and applications...[which] helped students absorb Friedman’s vision of economics as a tool for

³¹ See Hammond (1993, xix). Relying on the nature of the funding and other evidence including Hayek’s will to set up a conservative American community of scientists, Mirowski and Van Horn 2005 assert that the motives behind the free market study are essentially ideological. However, that the kind of work undertaken supported liberal theses does not preclude a sincere belief that the American economic was indeed competitive and that this could be demonstrated scientifically.

³² This tendency may be attributed to the influence of Director: “Aaron Director had a genius for high powered application of relatively low powered economic theory that later became the hallmark of Chicago law and economics” (Hammond 1993, xi).

³³ Friedman to Stigler, Winter or Spring 1952, quoted in Hammond and Hammond 2006. Friedman’s wide application of the supply-demand framework inspired later applications of economic tools to other disciplines such as sociology, psychology and, political science, such as Gary Becker’s. Friedman strongly supported the publication of his thesis *The Economics of Discrimination* 1957 against sceptics, and admired his subsequent work on crime and punishment, family, and time allocation. Becker related how he had lost interest in economics at Princeton and considered switching to sociology, until he attended Friedman’s price theory course (see Becker 1993).

understanding the real world, not as a game played by clever academics” (Becker 1993, 32). This feature put Friedman and the Chicago School apart: “A major characteristic of the Chicagoans is their tendency to see and to apply economics all around them” (Miller 1962, 68).

Another core notion in Friedman’s worldview was equilibrium, which was supposed to describe the usual state of markets and was seen a consequence of rational economic behaviour (consumption, money demand, etc.) (Reder 1982, 11-12). Friedman (1942, 1953a) nonetheless thought that this static account of economic systems should be supplemented by a theory of monetary dynamics, and felt deeply dissatisfied with the Keynesian interpretation he had encountered at the *Treasury*. His dynamic counterpart to the notion of equilibrium was that of long term stability, long term full employment and real-monetary dichotomy. From his joint work with Kuznets, Friedman had learned to distinguish permanent, semi-permanent and transitory causal factors in the determination of income, a formalization he subsequently used in almost all his macroeconomic works. But it was more than a mere mathematical modeling device for him, it genuinely reflected his understanding of economic behaviour: “a fundamental hypothesis of science is that appearances are deceptive and that there is a way of looking at or interpreting or organizing the evidence that will reveal superficially disconnected and diverse phenomena to be manifestations of a more fundamental and relatively simple structure” (Friedman 1953a, 33).

By the mid 1950s, Friedman’s emphasis on the Marshallian framework –he presented his idiosyncratic view of Marshallian demand theory in a 1949 paper- and his application of economic analysis to a wide range of phenomena had become unusual in a profession that had been swept along by the general equilibrium revolution.³⁴ In 1948 and 1953b, he extended of the rational decision paradigm to situations with risk with help of Savage, but on the whole his substantive contributions to Chicago style microeconomics remained scarce. It is however from the worldview he transmitted in these few papers and his methodological essay and especially through his teaching that the Chicago research programme developed (Miller 1962, Reder 1982, 10 and 32-33, and Stigler 1963, 1988): “the emphasis in his [Friedman] course on applications of theory to the real world set the tone for the department. It was considered

³⁴ That Kenneth Galbraith, Friedman’s major opponent, used Marshall to build and diametrically opposed *Theory of Price Control* (1952), show how intellectual influences are conditioned by the worldview in which they are assimilated.

necessary to have a strong working command of basic price theory, especially so-called partial equilibrium supply and demand analysis”, Becker reminded. The research programme grown out of Friedman’s vision in turned strengthened it by providing scientific warrant (these contributions are detailed in Reder 1982). Friedman’s worldview was also strengthened by the unity of the intellectual environment he, Stigler, Allen Wallis and Director had succeeded in building.³⁵ Friedman and Wallis had been brought back in 1946; Director became head of the law department, and Wallis of the Business School; the Cowles Commission and its walrassian and socialist members departed for Yale in 1953; eventually, Wallis succeeded in bringing back Stigler from Columbia in 1956. The spreading of the Chicago paradigm was further enhanced by the establishment of a demanding PhD programme which “incalculated distinctive habits of thought” to students (Reder 1982, p. 9. See also Emmett 1997). The community was also backed by a constant financial support, at the beginning from the Rockefeller foundation, and especially from several conservative think tanks and foundations.³⁶ Friedman was to spend the remaining of his academic career in this protective environment, and therefore never saw many reasons to question his worldview.

Had Friedman’s worldview depended only on his beliefs in the rationality of human behaviour, the competitive nature and the stability of the economic system, it would not have been so idiosyncratic, however. There was a more distinctive belief at the roots of Friedman’s worldview, one that distinguished him not only from economists outside Chicago, but also from his teachers. His belief was that collective action, in particular state intervention, was inefficient and unfair.

2. 3. A liberalism grounded in a distrust of government and lobbies.

³⁵ The situation was far from secured at the end of the war. A stream of progressive and walrassian economists had arrived in Chicago due to Jacob Marschak’s energetic direction of the Cowles Commission, and the situation was hardly better within the economic department. In 1946, an offer had been made to Samuelson instead of Stigler, as Friedman had hoped (see Hammond 2006,46)

³⁶ The Free Market Study Program was funded by the Volker fund (Van Horn 2005), Friedman’s workshop on Money and Banking was started on a three year Rockefeller grant. The conservative Earhart fondation set on a fellowship program for economic PhD students (for which Friedman recommended Friedman recommended Becker and Roger Weiss in 1953, Marc Nerlove and Lawrence Fisher in 1955, Richard Robb and Robert Snyder in 1956 (MFA Box 26 folder 1). We however think that this ideologically loaded funding mirrored rather than influenced the type of research conducted at Chicago. During the *Roof of Ceiling* episode, Friedman had shown that he would never be willing to sacrifice his intellectual independence to string together with his fund providers’ political opinions. He used the liberal orientation of his research to gather fund and conducted his research as it pleased him.

The conservative political leanings of Jones and Burns early oriented Friedman toward classical liberalism, a tendency then reinforced by Knight's influence. His main source of inspiration however was Simons's *A Positive Programme for Laissez-Faire* (1936) (Friedman 1950; 1951; 1955 from archives).³⁷ Simons chose individual freedom as the queen value and argued that its preservation entailed that a large sector of economic life be organized competitively. Yet, he feared as much concentrations of economic power in unions and large corporations as in the hands of government, and he defended income taxation as a mean to reduce distribution inequality. Friedman found the book "strongly pro free market in orientation", and later attributed his neglect of Simon's qualification to the newness of the book in the "strongly prosocialist" intellectual atmosphere at Chicago at the time (Kitch 1983, 178-179).

These intellectual influences alone cannot explain the strength and orientation of Friedman's political beliefs. Why wasn't Friedman drawn to more progressive Chicago teachers, such as Douglas? How could he have been in repeated contact with progressive Keynesian and institutionalist beliefs at Columbia and Wisconsin, worked with the almost communist Kuznets, be immersed in the Keynesian atmosphere of Washington public administration during the war, without at least being attracted by these ideologies?³⁸ In fact, Friedman was immunized from the progressive influence of his working environment by his continuous contact with a small group of friends with similar political beliefs, whom he had met at Chicago. The group included Wallis, a fellow undergraduate, who, like Friedman, worked for National Resource Committee, for the NBER and for the government during the war. Director, who had been trained with Hayek also spend his war effort in governmental organizations. His sister Rose, also a staunch libertarian, became Friedman's wife in 1938. She is often credited with being the driving force behind Friedman's liberal zeal (Silk 1976, 68). She 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). Eventually, Friedman's officemate at the SRG happened to be Stigler, also a classical liberal.

³⁷ Hayek's *The Road to Serfdom* (1944) made a lasting impression on him (Friedman to Breit, 22/06/67, MFA, Box 21 folder 26), but by the time he read it his political views were settled.

³⁸ It should be reminded that there was a significant political diversity within Chicago in the interwar (Reder 1982). Other Chicago students, for instance Kenneth Boulding and Paul Samuelson, did not embraced liberalism.

The strength of Friedman's liberalism is also explained by the numerous bad 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 a pretext to defend the interests of medical lobbies.³⁹ 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). The position he subsequently held at the *Treasury* then offered a privileged standpoint from which to observe the policy making process. As his wife Rose put it, «[he] concluded that a brief stint in Washington was vital for a putative economist, but that a long stay was exceedingly harmful. The frenetic atmosphere of a political capital, with its concentration of day-to-day issues, destroys perspective, makes it difficult to disentangle abiding principles from distorting froth, and renders a true scientific attitude almost untenable» (Friedman R. 1976a?, 18?). There again, he experienced what he saw as the contamination of scientific analysis with private interests. The *Treasury* was engaged in a controversy with the *Office of Price Administration*, led by Leon Henderson and John Galbraith, on the amount of additional taxes necessary to curb inflation. The OPA initially presented lower estimations than the Treasury, only to come up with much higher revised estimated after being given the legal authority to fix wages and prices. Friedman saw this change of mind as a deliberate strategy by the OPA to secure the legal authority over the determination of prices: "crass promotion of their own power? Or necessary tactics to achieve what they firmly believed was a public good? Whatever the answer, the manipulation of the estimates seemed to me then, and still does, dishonesty pure and simple", he concluded (Friedman 1998, 111). Friedman was again caught into controversy in 1946, when he published *Roofs or Ceilings*, a pamphlet against rent controls co-written with Sigler and subsidized by the very conservative Foundation for Economic Education (FEE).⁴⁰ The book of course was criticized as ideologically-laden with progressive economists (see Bangs 1947,482, and Samuelson cited Silk 1976, 84) but more surprisingly, it was also shouted down by conservatives, on grounds it was "collectivist propaganda" and "the most pernicious thing ever issued by an avowedly conservative

³⁹ In his autobiography, Friedman slyly noted in passing that Reynold Noyes "was in the pharmaceutical business" (Friedman 1998, 74)

⁴⁰ Friedman considered the book as a piece of scientific work: "I have repeatedly experienced attacks on what I regarded as scientific findings by economists who seemed driven more by their values than their objective judgments", he later wrote. "The attacks on George Stigler's and my *Roofs or Ceilings* was an early and mild example" (Friedman 1998, 219).

organization” (Rand 1946 cited in Skousen 1998).⁴¹ Pressured by the real estate lobby, the FEE directors, Leonard Read and O. Watts asked the authors to delete some sentences, which they categorically refused: “I am not willing to sacrifice an iota of intellectual honesty in order to have it published”, Friedman replied.⁴²

These various experiences would explain Friedman’s peculiar definition of freedom as the preservation of minorities from all kinds of coercion and his insistence on academic freedom. A more immediate consequence was Friedman’s public endorsement of liberalism and the beginnings of his political activism. In 1947 he participated in the foundation of the Mont Pelerin Society (MPS), an international grouping of American and European liberal intellectual, businessmen, journalists, at the initiative of Friedrich Hayek.⁴³ Despite his adhesion to the famous libertarian organization, Friedman’s liberalism however remained idiosyncratic. Nelson, for instance, described it as “a mixture of progressive and libertarian values” (Nelson 2001, 140). Friedman was indeed willing to fight all kind of lobbies and attacks against individual freedom and competition, even when it ran counter business interests. This intellectual independence is acknowledged by Samuelson in his *Newsweek* column on Friedman’s Nobel in 1976: “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).

Friedman’s above-mentioned experiences also informed his vision of the world “as it is”: the government was an inefficient means to attain whatever goals the American society favoured, he thought, because it was bureaucratic and driven by a coalition of vested interests. Therefore, either the goals attained did not reflect those chosen through a democratic process, or resources were wasted in the process (ref??). This outlook is seen in the theory of lags in state intervention and his advocacy of policy rules rather than discretion Friedman developed in scholarly journals at the end of this troubled period (Friedman 1947; 1948) In the years to come, this fundamental distrust of state intervention found a scientific legitimacy in the work of Stigler in economics, of Director and Posner in the Law and

⁴¹ 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).

⁴²Friedman to Read, undated 1946, Box 38 Folder 6, MFA.

⁴³ For a history of the Mont Pelerin Society, see Hatwell 1995. His closest friends, Director, Wallis, and Stigler, also joined the society.

Economics field, of the Public Choice school. They opposed the idea that government officials acted “in the public interest” and showed that their behaviour actually maximized their private interests as for private economic agents. His belief that state intervention was inevitably harmful grew so strong that it gradually came to shape his outlook of every alien economic system, including Japan, Germany, and the Third world.⁴⁴

By 1946, Friedman’s worldview was thus definitely settled. It relied upon belief that the economic system and society were inherently stable and on the antinomy between individual rationality and the inefficiency and unfairness of government intervention. These beliefs conditioned his hypotheses, but also the kind of models he built, his testing procedures, and his outlook on statistical and historical evidence.

3. How Friedman’s beliefs influenced his science

In 1948, Burns, then director of research of the NBER, asked Friedman to head a study of monetary factors in business cycles. The outcome of the ensuing fifteen years spent gathering and testing data, interpreting historical evidence, and resurrecting the quantity theory within the Chicago Workshop on Money and Banking was the 1962-63 publication of “Money and Business Cycles”, *A Monetary History of the United States* in collaboration with Anna Schwartz and “The relative stability of monetary velocity and of the multiplier”, written with David Meiselman.⁴⁵ A few years before he had articulated the knowledge of budget studies and consumption data accumulated since his work at the *National Resource Committee* in his *Theory of the Consumption Function* (1957). With regard to these works, Friedman had always claimed that “the sequence of problems dealt with is determined by the internal

⁴⁴ For instance, after a year spent visiting development countries in 1962-63, he related in his report to the Carnegie Foundation, fund purveyor of the trip: “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).

⁴⁵ The importance of these works for the development of monetarism is illustrated by a recollection of Tobin: “I think that people started to take Friedman seriously as soon as his stuff with Meiselman on the explanatory power of money came out. I also recall attending a conference on the Friedman-Schwartz volume when it was just coming out” (in Klamer 1983, 105).

structure of the work and not really by these kinds of external influences...”⁴⁶ Still, the invariably anti-keynesian and anti-interventionist character of his conclusions suggests otherwise. The choice of hypotheses seems influenced by Friedman’s quest for stable behaviours and self-stabilizing mechanisms in the economy. Likewise, his distrust of government intervention have informed the type of econometric models he built and his interpretation of the historical evidence that was so central to his empirical practice.

3.1. The quest for stable behaviours and self-stabilizing mechanisms

In 1967, Friedman gave the following account of his Permanent Income Hypothesis (PIH): “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 which were later to come to a more important fruition.” Indeed, the PIH appeared as the outgrowth of Kuznets’s paradox - that the marginal propensity to consume remained stable across time despite the steady increase in aggregate income, in contradiction with the Keynesian theory-, and the attempts to explain current consumption by variables other than current income and to account for biased budget studies estimations of the marginal propensity to consume carried between 1947 and 1953 by Dorothy Brady and Rose Friedman, Ruth Mack, Margaret Reid and other economists in contact with Friedman (see Hynes 1998 for an early history of the PIH). The idea behind the PIH was this in the air, as exemplified by the Life Cycle Hypothesis (LCH) presented by Franco Modigliani and Richard Brumberg in 1954. The power of Friedman’s contribution was in his statistical inventiveness: he used his permanent-transitory income framework and devised identification hypothesis that enabled him to reconcile these opposing empirical conclusions within a single explanatory model. Unlike Modigliani’s LCH, Friedman’s PIH implied that the multiplier associated with government spending is very low unless the additional income it brings is seen as permanent by households, which renders stabilization policy inefficient. This anti-interventionist conclusion suggests that Friedman was not only influenced by the technical developments in the field.

⁴⁶ Friedman to Breit, 22/06/67, Box 21 folder 26, MFA.

From the introduction of the book, it appears that Friedman's main motive for devising a new consumption theory was to defeat the "secular stagnation thesis" and the idea that there could be long run underemployment. He hoped for a theory in line with his beliefs in long term equilibrium and stability:

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. A number of writers, particularly Haberler and Pigou, demonstrated that this analytical proposition is invalid if consumption expenditures (C) is taken, not only to be a function of Y, but also of W, or, to put it differently, if the average propensity to consume is taken to depend in a particular way on W/Y. This dependence is required for the so-called Pigou effect (Friedman 1957, 5).

Friedman thus resorted to a relation between consumption and wealth and, unlike Modigliani and Brumberg, assumed that the life of the representative individual had an infinite duration. In each year of his life, the individual expected to receive a constant income-the so-called permanent income- except for a transitory component. Likewise, he was said to consume his resources at a stable rate that was not systematically linked with his permanent income. For Modigliani (2001,70) this "difference in the starting point between the LCH and the Permanent Income Hypothesis (PI) of Friedman... was to prove crucial as regards their macroeconomics implications".

Friedman's account of his monetary work similarly put emphasis on the internal dynamic of the field: "It is very hard for me to say that there was any philosophical reason which led to my undertaking studies in the field of money. Once having started studying in the field of money, the internal logic of that development led a life of its own" (Friedman to Breit). His monetarism derived from the re-examination of the historical evidence for the 1929-1933 period, Friedman 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).⁴⁷ With regards to methodological orientations, his emphasis on dynamics, understood as lags in adjustments (Friedman 1951b,114), the importance he attributed to datation, his work with time rates of changes rather than variable levels (Boland and Frazer 1983, 134), his first

⁴⁷ The existence of a Chicago oral tradition has been one of the most debated claims by Friedman. Patinkin 1969 launched the controversy by claiming that no such tradition existed and that Friedman's monetary work was in the tradition of Keynes. The huge literature on the nature of the "tradition" transmitted by Mints, Simons, Angell among others and on the evolution of Friedman and Patinkin's positions is collected in Leeson (2003).

difference detrending technique, his breaking up of times series in permanent and transitory components, all resulted from the influence of Hotelling, Mitchell and Kuznets. Yet, the theoretical conclusions he had reached by 1963 –the stability of the velocity, 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 perfectly fit in his neoliberal values. The coincidence was striking enough so that Hirsch and De Marchi (1990, 205) concluded:

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, to the effects that changes in money produce changes in activity.

Without assuming 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. According to Modigliani (1977, 27), the essence of Keynesianism was that “a private enterprise economy using an intangible money *needs* to be stabilized, *can* be stabilized, and therefore *should* be stabilized.” The economic system is inherently unstable because investment and the demand for money are determined by the erratic move of interest rates, with the possibility of a liquidity trap appearing when interest rates are low. In such a situation, there is no possibility to restore long term equilibrium other than expensive fiscal policy. Friedman came to the study with an *a priori* opposition to this vision. This preconception was combined with his view of money as an asset rather than merely as a means of payment, in line with his fisherian microeconomic framework. Friedman was especially concerned with the potentially devastating consequences of household’s portfolio decisions during currency panics, where the velocity of money could become infinite. Because he viewed the velocity as a reflect of individual decisions rather than technical factors, he insisted in his 1948 “Preliminary Plan for the Compilation of Data” that special consideration be given to the measure of cyclical variations in the rate of use of the circulating medium (Hammond 1996, 59). A year later, he noted that the preliminary data gathered seemed to feature a regular procyclical velocity, which “differed widely from the assumptions

about the behaviour of the circulating means” (Hammond 1996, 63). Such results suggested an alternative to the income expenditure model, namely the quantity theory which “asserts in essence that the velocity of circulation of money is the empirical variable that behaves in a stable and consistent fashion” (Friedman 1952b, 621). In this 1952 paper, Friedman studied the behaviour of prices, income and money series during wartime periods and established that prices and income were related to money in a stable manner (*check*). The theoretical explanation for these empirical findings came in 1956 with the reformulation of the quantity theory as a demand for money, one depending on permanent income in a stable fashion and allowing for a self-stabilizing mechanism through portfolio allocation. Interest rates were present in the initial equation as the prices of alternative assets but Friedman quickly rule their explanatory power out in a 1959 empirical study. He thus ended up with a theoretical framework where government stabilization policy was unnecessary (Modigliani 1977, 27). This framework in turn influenced Friedman and Schwartz’s interpretation of 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. In the eye of Keynesian opponents, this *ad hoc* explanation based on unobservable variables was unacceptable. In his review of the book, James Tobin 1965 faulted the authors for neglecting the effects of interest rates, despite the fact that they provided a single explanation for both trends.

The charges against Friedman did not primarily focus on his hypotheses, however, but on his empirical practice. At first, they found it “naïve” or “deceptively pretentious empiricism” (Culbertson 1960, Ando and Modigliani 1965, Karenken and Solow 1963), but when monetarism gradually came to be seen as a valuable alternative to Keynesian economics and ultimately threatened to be implemented, the tone hardened. Friedman was accused of “chicanery” and “charlatanism” (Johnson 1971, Kaldor 1982) and -more or less explicitly- 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 Rome 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 reflected 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, a gap previously epitomized by Koopmans's famous "measurement without theory" critique of Burns and Mitchell's *Measuring Business Cycles* (1946).⁴⁸ Keynesian economists used the structural estimation method developed at the Cowles Commission. They believed that the economy could be represented as a set of interdependent, simultaneous and stochastic relationships of the walrassian kind, and that the data would speak only once such exhaustive structural model has been imposed on them (see Klein 1955).⁴⁹ On the contrary, Friedman concentrated on single equations with variables, a choice he was extremely criticized for. Although he recognized that the outlook on data was inevitably conditioned by a theoretical framework (see the sentences of Friedman and Schwartz 1963), he thought that the economist should start with "the collection of data to provide something to generalize from". Only then could hypotheses be derived and tested.⁵⁰ Uncovering empirical regularities and identifying peaks and lags in the series were the inescapable first steps of empirical research. Friedman expressed his disagreement with Cowles researchers in a 1947 letter to Joseph Willits, director of the division of social sciences at the Rockefeller: "it might, in decades ahead, be possible to construct a general model of the economy based on a general theory of economic fluctuations, but... we do not have enough knowledge of the behaviour of components segments now to discriminate between alternative models", he argued.⁵¹

⁴⁸ Friedman was very helpful in the writing of Rutledge Vining's reply to Koopmans, but he refused to be publicly acknowledged and asked Vining to drop the thank-you footnote introducing the reply (Vining to Friedman, 01/11/48, MFA Box 34 Folder 33).

⁴⁹ For a detailed exposition of the Cowles method, see Christ 1994 and Epstein 1987.

⁵⁰ Friedman to Stigler, 04/10/48, quoted in Hammond and Hammond (2006). The use of simple equations he later explained by a work on the resistance of alloys conducted at the SRG. The alloy he had built from an explanatory equation that fared statistically extremely well happened to rupture far before the forecasted time limit. "Ever since, I have been extremely sceptical of relying on projections from a multiple regression, however well it performs on the body of data from which it is derived; and the more complex the regression, the more sceptical I am », Friedman (1998, 143) concluded.

⁵¹ Friedman to Willits, 26/09/47, Box 35 Folder 16, MFA, see also Mirowski 2002, 215-220, Rutherford 2005, 124-5.

But differences of political agenda were looming behind the methodological disagreement. For the socialist Jacob Marschak, research director of the Cowles between 1943 and 1948, as for most members, the ultimate aim of the structural equations method was “social engineering”. His initial 1943 schedule for his five years directorship was called “Statistical foundations of Rational Economic Policy”. If theories of the behaviour of various classes of economic agents could be translated into a unique set of structural equations, then economic policy could be cleared from “preconceived ideas affected by emotional preferences”, Marschak optimistically hoped (Epstein 1987, 62-65). Cowles researchers’ insistence on correct model specification, identification and estimation techniques derived from their desire to picture exhaustively causal relations and the exogenous variables on which the government could act. This objective surfaces in their criticisms of Friedman’s empirical practice. Suspicious of governmental intervention as he was, Friedman was certainly not interested in the control of the variables in his equations, nor did he feel the need to characterize straightforwardly their exogeneity or endogeneity. Ando and Modigliani criticized Friedman and Meiselman’s specification of their income expenditure and quantity theory equations. Nothing ensured that the explanatory and explained variables were simultaneous, they noted, nor that Friedman’s variables, exogenous in his unique equation, would remain so in a model of a whole economy. “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 concluded (Ando and Modigliani 1965, 693).⁵² Likewise, Karenken and Solow 1963 challenged the definition of money Friedman used in his 1960 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, especially since the use of a rate of change as the money variable and a level as the income variable created a spurious lead-lag pattern (as also pointed out by Tobin 1970 in his 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” where Keynesians

⁵²On 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.”

economists needed a clear-cut causality as a basis for policy implementation. Following Clower 1964, Hammond (1996, 112) concluded from his extensive study of the reactions to the *Monetary History* that the disagreement with Friedman and Keynesian could not have been settled by factual evidence, because they had different objectives in mind: “his was to explain historical experience, and theirs, to answer counterfactual questions regarding current policy options.”

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

Cowles economists especially blamed Friedman for not submitting to the very methodological principles he had appealed to against their forerunner, Jan Tinbergen. In a 1940 review of *Business Cycles in the United States of America*, he had pointed out Tinbergen’s statistical tests of significance were meaningless, since he had selected the explanatory variables he knew would provide the highest correlation coefficient. 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 by the coefficients which they get in earlier or later periods” (Friedman 1940, 660). The tests performed in the *Theory of the Consumption Function* nonetheless were merely confirmatory rather than discriminating, Mayer (1972,59) remarked. He pointed out that Friedman assumed the truth of the PIH and then adjusted the data to fit the hypothesis, the very procedure Friedman 1940 had blamed Tinbergen for. The tests confirmed certain characteristics of the empirical model, but non test the key predictions of the PIH – that the income elasticity is zero for transitory income and unity for permanent income. Likewise, Diesing (1985, 65-69) found different data adjustments tactics according to the results of the tests of the PIH. He pointed out that Friedman sometimes exaggerated the fit with theory, invented ad hoc explanations for divergences, such as higher social security payment in Great Britain to explain the lower saving rate. Or he adjusted the data until they fit, and transformed slightly the definition of permanent income. In last resort, he rejected the data as unreliable or expressed puzzlement for the discrepancies.⁵³ Several reviewers pointed out the lack of modern tests other than on goodness of fit in his other works (see for instance Klein (1958,543) on Friedman and Becker 1957). In 1950, W. Marshall had laid out the principles

⁵³ While such a practice is not specific to Friedman, it provides room for *a priori* judgments in the testing process.

for out-of-samples tests, and Karl Christ had used some the following year in his assessment of Klein macrostructural model of the US economy. The undertaking went unnoticed in the profession, except for Friedman who commented Christ's paper, interpreting the rejection of many equations in Christ's test as evidence that the economic knowledge of dynamics was not mature enough to build structural models (see Du Qin 1993, 138-9). It is therefore surprising that he did not subsequently make use of tests that apparently fitted his methodology perfectly.

This inconsistency reflects 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 yield causal relationships and test the validity of tentative hypotheses. Statistical evidence could be misleading, he warned, and high t statistics and R² were "a test primarily of the skill and patience of the analyst (Friedman 1951b, 108)." Friedman and Meiselman's use of the size of the correlation coefficient to prove monetarist models' superiority was a strategic argument, Leeson (2000, 34) explained. Friedman's faith that the confrontation with fact yielded scientific progress rather relied on the close analysis of historical evidence. As exemplified by the *Monetary History*, Friedman wished to demonstrate the importance of money in business cycles through historical narrative and counterfactual evidence rather than hypothesis testing. 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, by 1963, the bulk of the *Monetary History* book has been written. 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, data as well as proceedings of central banks meetings or seemingly minor events such as the 1928 death of the governor of the New-York Federal reserve, thus provides an ultimate channel through which his distrust of the state could enter his science. As regards monetary history, the records show that Friedman started the study with a clear picture of the role of the Fed in the Great Depression in mind. During a 1947 Mont Pelerin conference (that is, before Friedman was hired to lead the NBER study), when Hardy remarked that “the history of the 1920s shows monetary discretion at its best”, Friedman immediately approved: “I agree. The big error in Fed policy was that of 1931.”⁵⁴ 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.”⁵⁵ 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. I'm quite open-minded about that, and am therefore willing to take the point estimate; it will have to be a very biased estimate, before he will accept it” (Modigliani, 1977, 10).

Friedman's theoretical thinking was thus conditioned by his *a priori* view of the economic system as inherently stable. The confrontation procedure and the “facts”, statistical and historical, underpinning Friedman's value-neutrality claim in also appears to have been informed by his distrust of government intervention. His resulting conclusions, in particular

⁵⁴ *Proceedings*, Box 87 Folder 10, MFA. *Caution. This archive may have been misfiled. Possibility that it is the proceeding of an early 1948 conference part of the Free Market Study. Check.*

⁵⁵ Friedman to Stewart, 12/01/49, Box 33 Folder 35, MFA.

the existence of variable lags in monetary policy and the responsibility of the Fed in the Great Depression in turn provided a scientific legitimization for this belief, and enabled him to draw his famous policy recommendations limiting the discretionary power of monetary authorities. Consisting of tying together ends and means, abstract causal relations and concrete institutional situations, the art of economics may create other incentive to use private values than in theoretical and empirical science, and therefore should be treated separately.

4. Friedman's art: between science and politics

Friedman (1953a, 5) began his methodological essay by distinguishing positive economics, normative economics, and the art, thereby attributing a distinct existence to policy discussions. If he did not confuse policy recommendations with positive economics, he nonetheless considered them as a part of his science and published several papers on stabilization, monetary and fiscal policy (see for instance Friedman 1948; 1952a; 1959), and sustained by empirical work. When it came to champion specific ends, Friedman moved to other forums, such as the MPS's conferences, his chronicles in *Newsweek* and other newspapers, or dedicated books such as *Capitalism and Freedom* (1962). Whether scientific or political however, Friedman's thinking systematically led to favour free market solutions over government intervention. The two spheres seemed closely interconnected. On the one hand, Friedman's political defence of a free-society relied heavily on his scientific conceptual framework and his empirical findings. On the other hand, his scholarly discussion of "means" presupposed a temporal trade off between the various "agreed ends" and the choice of a proper institutional setting, which, according to the criticisms it raised, did not reflect values shared by the whole profession.

4. 1. The defence of political liberalism: mixing private values and economic concepts

The founding meeting of the MPS was devoted to questions such as "What is the nature of liberalism? Why has it declined? What is the "competitive order" and how can it be maintained? What should governments do in economic affairs (about money, employment, trade unions, poverty, agriculture?)" (Hartwell, 1995, 27). The discussions Friedman attended convinced him that classical liberalism provided no convincing alternative to government

intervention with respect to present-day social and economic issues. Modernizing its principles was a prerequisite to the formulation of a consistent set of free-markets policies, and this implied a break with the continental tradition. In a conference given in 1950 in Paris, “La France ne sait plus ce qu’est la liberté d’entreprendre”, he contrasted European *laissez-faire*, wherein the government had no role and enterprises were free to do whatever they could to defend their interest, including cartels and price agreements, and the American-style liberalism he called “neoliberalism”. In this American “competitive order”, the Sherman Act had entrusted the State with the protection of consumers and enterprises against monopolist tendencies. The European neglect of the government’s role ultimately led to a centralized, cartelized, and rigid industrial structure, while the American system fostered adaptability and innovation. The following year, he conceded that “the collectivist belief in the ability of direct action by the State to remedy all evils it itself, however, an understandable reaction to a basic error of the 19th century individualist philosophy...[which] underestimated the danger that private individuals could through agreement and combination usurp power and effectively limit the freedom of other individuals”. At the beginning at least, Friedman’s suspicion of state intervention did implied its systematic rejection. Rather, he undertook a careful delimitation of its boundaries, and restricted it to public work, the preservation of freedom and of a stable monetary framework, and the struggle against poverty.⁵⁶

Friedman’s political thinking matured within an American neoliberal community centered on Wabash College scholars John Van Sickle, former associate director of the Rockefeller bureau in Paris, Benjamin Rogge.⁵⁷ In 1955, they secured funds from the Volker funds and asked Friedman to give a series of conferences on the basic principles of economic liberalism and its policy applications. This gave him the opportunity to bring his ideas together in a unified framework: “though his basic libertarian philosophy goes back to his student days and probably earlier, it was in the course of preparing the lectures for the various conferences and even more in the discussions that followed that the principles of his philosophy were spelled out and that the principles were then applied to a various set of special problems”, Rose Friedman remembered (Friedman R. 1976b, 26).

⁵⁶ “Neoliberalism and Its Prospects”, Box 42 Folder 8, MFA, later published in the Mont Pelerin’s journal *Farmand*.

⁵⁷ Van Sickle was a staunch classical liberal: while in Paris during the thirties, he has subsidised the foundation of the Mises institute.

In preparing the speeches, Friedman came out with the thesis that would become the hallmark of his philosophy: “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”, he opened his first conference with. He strived to build bridges between his economic conceptual framework and his liberal values by entrusting the concept of “market” with normative content. Markets protect minority groups from the coercion of majority, he reflected, and should therefore be seen as a “system of proportional representation” that enabled coordination without standardization, and a “check” to political power. This focus on minority and freedom echoed his unfortunate Wisconsin experience and his Jewish origins, and reflected more widely his experience as an heterodox thinker permanently thrown into controversies.⁵⁸ He nonetheless remained strikingly undisturbed by the main threat to freedom of the time, McCarthyism. When, in 1952, the classical liberal Fritz Machlup told him that he would vote against Eisenhower because of the support he received from McCarthy, Friedman derided his friend’s comparison with interwar Germany: “I believe you are seeing ghosts under the table where there are none.” Two years later, when Maurice Allais asked whether foreign scholars should bring public support to Paul Sweezy, a marxist economist from Harvard put on trial for refusing to testify on the content of his courses, Friedman declined with rather muddle consideration on the balance between the violation of freedom of speech and Sweezy’s lack of strategy in his defense.⁵⁹ During the 1964 presidential elections, Tobin again questioned Friedman’s idea that the major threat to freedom is the size of the government rather than “know-nothings, Mitchell Palmers, McCarthys, Klu Kluxers, and the like. It is not the big Federal government that intimidates libertarians, textbooks writers, broadcasters, civil right advocates in the South”, he underlined.⁶⁰

4. 2. A political activism relying on scientific expertise

⁵⁸ “All of us here, he complained, 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 said in a 1976 MPS speech (quoted in Frazer 1988, 1972). In a 1972 conference on “Capitalism and the Jews”, given as president of the MPS, Friedman expressed his puzzle that the Jews are generally anti-capitalist. Since it was precisely in free market societies that they had been allowed to develop their talents and to prosper, “the ideology of the Jews has been and is still opposed to their self interest” (see Frazer 1988, ch.6).

⁵⁹ His analysis of the situation was that the American free-market system had on the whole protected individuals against the witch hunt by enabling them to keep earning a living no matter of their political values. (“Capitalism and Freedom”, Box 44 Folder 1, MFA).

⁶⁰ Friedman to Machlup, 28/10/52, Box 38 Folder 25; Friedman to Allais, Box 18 Folder 21; Tobin to Friedman, 07/12/64, Box 34 Folder 13, MFA.

Galbraith's *The Affluent Society* was released in 1958 and it immediately became a best seller. In his book, he bemoaned that most national resources were used by the private sector to produce bare consumer goods at the expense of necessary public services such as education and infrastructures.⁶¹ This success increased the urge to have the ideas developed in the Wabash conferences brought to a larger public. Aware that criticisms against socialism had been growing since the forties (Nelson 2001, 143-145), he began thinking of a book for a wider audience. It was Rose who finally put the lectures in a publishable form (Friedman 1998, 340-341). *Capitalism and Freedom* heavily drew upon Friedman's scientific research to provide the reader with a corrected version of facts in chapters on money, international, financial and trade arrangements, fiscal policy, etc. Friedman expounded the principles of modern liberalism and the role attributed to the state in this framework in two introducing chapters, and devoted the rest of the book to concrete policy proposals. They included the replacement of various social security advantages with a negative income tax, the replacement of the state schooling system with school vouchers, the suppression of the minimum wage legislation, of the draft, of rent control, of public industries, public housing, compelling retirement programmes (Friedman 1962, 32). These proposals he would fight for from the mid sixties onward through his support of Barry Goldwater for the 1964 presidential elections, his *Newsweek* chronicles (1966-1984), his numerous radio and public speeches, the *Free to Choose* TV series and the Friedman foundation for school vouchers.

The 1947-1963 period was rather devoted to the establishment of an institutional niche from which to launch later attacks on interventionism. A striking feature of Friedman's strategy was the combination of political values and scientific expertise. Friedman's appeal to his scientific identity was not only for the sake of rhetoric. It reflected how science and liberalism were tied together in his worldview. In 1951, he made it clear that the case for liberalism had to be fought on the scientific level because the major failure of socialists lied in the means advocated rather than the ends pursued:

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

⁶¹ The book sold more than one million copies. In a MPS conference on "Adam Smith's relevance for 1976", Friedman fiercely noted that "Galbraith, who is currently doing a BBC series on economists through the ages, has a very different view than I of Adam Smith's relevance today. Perhaps this attitude is simply Galbraith's defensive reaction to Smith's devastating 1776 review of Galbraith's 1958 *Affluent Society*" (Box 55 folder 21, MFA). In the year ahead, he identified Galbraith as his main opponent and devoted much energy to counter his views. He wrote a pamphlet entitled *From Galbraith to Economic Freedom* (1977), and counterattacked his 1977 TV series *The Age of Uncertainty* with the *Free to Choose* series, broadcasted in 1981.

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.⁶²

This idea of a scientific way to liberalism was shared by many other scientists, in particular Hayek. When he founded the MPS, Hayek had in mind an intellectual community devoted to the scientific *analysis* of a free society rather than a lobbying group devoted to *political propaganda* (Hartwell 1995, 103). His views clashed with the plans of the old European guard, including Wilhelm Röpke, Karl Brandt, and Jacques Rueff and led by the Swiss businessman Albert Hunold who funded the society.⁶³ Hayek thus gradually turned to American classical neoliberals, mainly economists, who shared his scientific approach. The disagreement broke into an open crisis in 1959, when Hunold issued a circular denouncing the increasing role of his Institute of Economic Affairs in the management of the society. He also opposed the membership of its director Seldon Harris, who fully shared Hayek’s scientific perspective. Friedman’s reaction shows how these two visions of liberalism had become irreconcilable: “If Hunold stays... this means turning the society over to him, which means the most reactionary and statu quo influence in the society, which have already gone far to give us a name that those who are liberals in the nineteenth century sense and like Knight and Harry Gideonse believe the state does have a positive function, albeit not without dangers, have been trying to combat as much as we could. Hence our determination to make a fight this fall to get Hunold out”, he fulminated in 1960. The dispute finally ended with the tumultuous resignation of Hunold and Röpke in 1961 (which Friedman had been instrumental in provoking) and the American faction swiftly took over the leadership of the Society.⁶⁴

⁶² “Neoliberalism and its prospects”, Box 42 Folder 8, MFA.

⁶³ Brandt visited Chicago in April 1949 and wrote Hayek that « Friedman seemed to me to be most positive and energetic of the trio. We talked of course also about the policy of the Society and here I found what I had sensed for a long while. [Director] is strictly opposed to any activity that would try to influence policy directly or have any practical implications. To a lesser extent, Frank Knight is also doubtful about the possibilities of such action.” (Hartwell 1995, 86). The events agitating the MPS in 1960-61 proved Brandt to be false in his assessment of Friedman. Nonetheless, his impression shows that Friedman was more willing than his fellow scientist to get involved in the political process.

⁶⁴ Friedman to Gideonse, 08/08/60, Box 86 Folder 2, MFA.

Other projects of Friedman and his colleague reveal the close connection between their defence of liberalism and their scientific achievements within their worldview. In the mid fifties, Friedman, Stigler and Van Sickle began thinking of a book collection of books that would publicize their free society ideal. The latter came in contact with Ruth Sheldon Knowles, a journalist and best seller author who had pursued her career as petroleum consultant and historian of the oil and gas industry. By 1963, she had secured funds for the projects from virtually all the biggest American companies, including the Shell, the Continental and the New Jersey Oil companies, U.S. Steel, General Electric, Shell Oil, DuPont, Ford Motor Cnie, Firestone Tire & Rubber, Eli Lilly, etc. The “Principle of Freedom” project was launched, with an organizing committee composed of Friedman, Hayek, Knowles, Rogge, Stigler, Nutter, and Van Sickle as president. The members of the advisory committee were recruited among MPS members. It included Maurice Allais, Stanley Dennison, Gottfried Haberler, John Jewkes, Frank Knight, Karl Popper, Jacques Rueff and L. Von Mises among others. From the brochure designed to raise funds, which Friedman extensively contributed to draft, it clearly appeared that the promotion of liberal ideas was to be fulfilled with reliance upon scientific expertise:

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 day.... These volumes will provide what we do not have and urgently need: a comprehensive and modern statement of the positive case for free enterprise and limited government as means for the achievement and preservation of personal liberty....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.

From the beginning, the project was thus was designed to win the battle of ideas, an orientation which eased the funding: “they [underwriters] feel our project is what should be done because of the scholars on the Committee”, Knowles remarked. The project ultimately amounted to a semi-failure, due to the difficulty to find reputed scholars to write for the collection. Friedman did not contribute; he had published *Capitalism and Freedom* just before the beginning of the project, and subsequently got involved in the presidential campaign of

Barry Goldwater, the republican candidate. And above all, he was busy with his scientific research as another path leading to market-oriented policies.⁶⁵

4. 3. How Friedman's private values influence his art

“Friedman arrived at his conclusions by thorough systematic application of economic theory, and the graduate students witnessed policy positions derived from economic analysis”, his former student David I Fand (1993, 18) remembered. Friedman’s teaching reflected his inner conviction that “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” (Friedman 1953a, 5). Policy discussions dealt exclusively with means, he concluded, and therefore could claim for scientificity. In line with this belief, he got into the habit of stating the “agreed ends” in the title or the first sentences of his articles and then considering the matter settled. For instance, “A Monetary and Fiscal Framework for Economic Stability” (1948) begins with the following sentence: “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).⁶⁶ Friedman seemingly saw no contradiction between ends such as price stability and employment, or between efficiency and equality.⁶⁷ Indeed, his 1948 paper aimed at showing that a policy proposal built for long term stability *also* provides the best short run stability available.

Friedman later gave two explanations for his policy disagreement with Keynesian: “the discount rate we use in judging future events relative to current events relative to current events”, which he called “a difference in time perspective”, and “our judgment of the way in

⁶⁵ 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.

⁶⁶ There was certainly both substantial belief and rhetorical flourish in this “shared-end strategy” he constantly used in policy controversies. His private exchange with Martin Bronfenbrenner over housing policy following the publication of *Roofs or Ceiling* is archetypal of such persuasion technique: “As you know I have strong egalitarian leanings and it is largely on a egalitarian ground that I object to the ad hoc proposals like yours. It seems to me that they largely dissipate the egalitarian sentiment and strength in achieving special measures that do not accomplish the objective we share.” (Friedman to Bronfenbrenner, 18/07/48, MFA, Box 21 Folder 35)

⁶⁷ In a famous 1977 public debate with Modigliani, he categorically refused to attribute their disagreement to the weight they respectively attached to inflation and unemployment (Friedman, 1977,12).

which policy is formed, operates, and develops” (Friedman 1977, 12, see also Friedman 1968). His own temporal trade-off favoured “the long view”, a choice which rested on his belief in individual rationality and the system’s stability. And progressive economists could not subscribe to such beliefs, as exemplified in his private correspondence with the Columbia economist Albert Hart on the *Monetary and Fiscal Framework* (1948). Albeit trained at Chicago, Hart was a Keynesian economist and naturally disagreed with Friedman’s temporal tradeoff: “giving priority to “price flexibility” measures over employment strikes me as self-defeating through intensifying the forces which make prices misbehave. I opine that if we try to pursue employment goals we’ll before long strike a spot where pricing practices are an impediment. Then will be the time to worry –but not so much about instilling “flexibility” as about avoiding inappropriate increases.”

With regard to institutional feasibility, 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). Indeed, 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.” In the 1953 rejoinder, however, Friedman also claimed that the economist should take into account “the realities of political life”, inasmuch as they influenced the institutional setting and “in part determine what the effects of a policy will be” (Friedman 1953c, 252). A systematic premise in his policy discussion indeed was that government intervention is an inefficient means to achieve whatever ends are considered, including unemployment and equality, and that prices could be considered flexible. A first reason he gave was the existence of lags in governmental response of three kinds: the recognition lag, the action lag, and the effect lag (Friedman 1948, 255). Friedman consequently defended a system that would reduce the lags through the operation of automatic stabilizers. It consisted of 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 central bank authority. This proposal was inspired by Simons 1934. Around 1959, having demonstrated empirically that the lags are long and variable, he replaced the idea with a monetary growth rule.⁶⁸ Another reason why state

⁶⁸ This evolution has been given several explanations. To Allais who was wondering about this switch, Friedman replied that he was “stressing a more immediately feasible reform”(Friedman to Allais, 06/12/71, Box 18 Folder 21, MFA). Laidler (2007) argues that as Friedman’s work progressed, he became increasingly

intervention was doomed to failure were that government mistakes were of greater impact than individual ones. For instance, when the issue of free vs fixed exchange rate was touched upon in a session of the first MPS conference, Friedman argued in favour of free rates by pointing out 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, a judgment echoing his previous Washington experience. And to convince Lionel Robbins of the dangers of the fixed exchange rates system he supported, Friedman argued that the 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.⁶⁹

On the whole, there was a two-pronged rationale between Friedman’s refusal of stabilization policies: governmental intervention was inefficient because of lags in reaction, mistakes, and vested interests, but also unnecessary since the economic system was capable of self stabilization. We have seen that these two conclusions are infused with his fundamental belief in individual rationality and distrust of the state. These same beliefs also translated in a political activism in favour of a “free society” and in a correlative set of anti-statist policy proposals designed to preserve individual freedom.⁷⁰ These common roots explain why, however never confused, Friedman’s policy proposals issued in scientific forums and those issued in political ones are so consistent.

5. Conclusion

This account above shows that the process whereby science and political values interact cannot be reduced to the distortion of hypotheses, models, results and policy advice to fit one’s political convictions. If the later played a role in Friedman’s science, it is only in

convinced of the inherent stability of an economy that was not subjected to monetary shocks and devised an alternative institutional framework that would prevent their occurrence.

⁶⁹ Hart to Friedman, Friedman to Hart, 4/08/1947, Box 38 Folder 13; Friedman to Robbins, 22/02/52, Box 32 folder 6, MFA. “1947 Proceedings”, Box 87 Folder10, Mont Pelerin Archives.

⁷⁰ Friedman often defended a policy proposal with different arguments depending of his interlocutor. Compare for instance the above-mentioned case for free exchange rate expounded to Robbins and that used to convince Barry Goldwater: “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.... Invented by Schacht and first introduced in Germany in 1934. Almost without exception, wherever introduced it has been a prelude to a wide range of further direct controls over personal and business transactions.” (Friedman to Goldwater, 12/12/60, Box 27 folder 24, MFA).

combination with some cognitive and methodological values that enabled the survival of these subjective elements in his scientific conclusions. Friedman's distrust of the state and confidence in the stability of economic system would not have influenced his macroeconomic so much had they not been combined with his Marshallian orientation, his NBER type of empirical research, his scepticism about econometrics's and his resulting emphasis on historical evidence such as the Great Depression. That Friedman saw his conclusions as warranted as facts contributed to the strengthening of his neoliberal convictions. Also, despite a clear separation of his scientific and political activities, Friedman's scholarly discussion of policy means relied on private trade-offs between ends and preference for market oriented institutional settings.

Methodological choices of mathematical formalisation, econometric design, testing, etc take up a broader significance when they are considered in combination with the economist's other values. More precisely, while cognitive and political values provides *substantive* hypotheses on the world that influence research, methodological choices shape the *channels* whereby these values actually enter the theorizing process and survive the confrontation with fact. Such conclusion is in line with Roger Backhouse's 2005 comment that the role of "intellectual" or methodological values in the relationships between science and ideology is worth studying. While Friedman's example suggests that methodological values convey private values, other case studies may support the economists' claim that methodological innovations rather enhance the immunisation against ideology.

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