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INSTITUTO DE ECONOMIA

JONAS TEIXEIRA COUTO CAMPOS

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Resumo

Resumo: Milton Friedman e George Stigler foram dois dos mais importantes economistas de uma das mais importantes escolas de pensamento econômico do século XX, a Escola de Chicago. Apesar disso, eles discordaram notadamente sobre qual deveria ser o relacionamento apropriado entre economistas e políticas econômicas. Esta Dissertação é uma investigação de sua divergência neste tópico. Primeiro, observamos o nascimento da Escola de Chicago, destacando o espaço para divergências e desacordos intelectuais que ela deixava aberto. Então investigamos a divergência de Friedman e Stigler como ela se desenvolveu historicamente, chegando no que parece ser seu aspecto essencial: enquanto Friedman entendia legítimo e eficiente intervir diretamente sobre a formulação de políticas, Stigler entendia que era legítimo e eficiente fazê-lo mediado pela comunidade científica. Por fim, tentamos uma explicação de sua divergência fazendo recurso ao conceito do mercado de ideias, que pretende servir como uma epistemologia dos economistas. Este último esforço é bastante mais tentativo devido ao estado ainda subdesenvolvido do mercado de ideias como conceito, mas chegamos à conclusão de que diferenças quanto à permissibilidade para comportamentos não racionais e quanto ao lugar atribuído ao mercado de ideias científicas em particular podem ser fatores explicativos de sua divergência.

Palavras-chave: Milton Friedman; George Stigler; Escola de Chicago; Papel da Economia/dos Economistas.

Abstract

Abstract: Milton Friedman and George Stigler were two of the most important economists of one of the most important schools of economic thought in the twentieth-century, the Chicago School of Economics. Nonetheless, they notably disagreed on what should be the appropriate relationship of economists to economic policies. This Dissertation is an investigation of their divergence on this topic. We first delve into the birth of the Chicago School, highlighting the space for intellectual divergence and disagreement that it left open. We then investigate Friedman and Stigler's divergence as it unfolded historically, arriving at what seems to be its essential aspect: Friedman thought it legitimate and efficient to intervene directly on policies, whereas Stigler thought it legitimate and efficient to do so mediated by the scientific community. Finally, we attempt an explanation of their divergence by resorting to the marketplace of ideas concept, which is meant to serve as the economists' epistemology. This last effort is much more tentative due to the marketplace of ideas' still underdeveloped state as a concept, but we reached the conclusion that differences in the allowance for nonrational behavior and in the place allotted to the marketplace for scientific ideas in particular may be explanatory factors of the their divergence.

Key-words: Milton Friedman; George Stigler; Chicago School; Role of economics/economists.

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Tables

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Introduction

I began my undergraduate studies in economics right after Brazil took a neoliberal turn. In my first couple of years in university, the Brazilian Congress approved a number of neoliberal reforms, which profoundly transformed the country's institutional environment. The transformation was not complete, as many social forces opposed the movement, but some important advances for the neoliberal project were made.

What caught my attention at the time was the strength of neoliberal ideas. Many of the reforms, such as those in the labor market, social security, and fiscal policy, appeared in the mainstream media as unavoidable facts, imposed by the economy. As an economics student in a heterodox school, I was firmly convict neoliberal reforms were not inavitable: a variety of economic views existed, and those reforms incorporated only one of them. But the single economic view the reforms incorporated seemed to have a sort of intellectual monopoly, both on the media and on public and political debates more generally. But why were things like this? How could people not know alternative ideas existed?

Those questions led me to study neoliberalism. I first tried to understand what it was, but after a while reading those who made neoliberalism a subject of (generally critic) investigation, I wanted something more. I wanted to get to know neoliberals themselves, those who had build the movement and led it to dominance. In particular, I wanted to know what they thought about the place of intellectuals in advancing their own political project.

This Dissertation is an attempt to understand how two neoliberals in particular thought intellectuals should act to advance political positions. Due to the scientific nature of this investigation, I had to narrow down to a very specific subject, and so I did. I deal specifically with what Milton Friedman and George Stigler, two of the most important members of the Chicago School of Economics, thought about the economists' proper role when influencing policies. I hope this specific investigation, beyond its own merits in the history of economic thought, serves also as a means to understand the role of ideas and intellectuals in political movements in general, and in neoliberalism in particular.

Dealing with neoliberalism, even if it is mostly in the background due to the specificity of my chosen subject, I should define it briefly. I conceive neoliberalism as do the major works in the history of neoliberal economic thought: neoliberalism is a political

movement, organized primarily around the Mont Pèlerin Society, but connecting individuals from a number of thought collectives, among which is Chicago. More will be said regarding the Society and thought collectives in Chapter 1, and I refer the reader to it for further details. Here, I will note two things about neoliberalism that do not appear in any of the following chapters. I owe those considerations to professor Manuel Luz, who emphasized their importance to me.

First, neoliberalism has a rhetoric of spontaneity, but it is very much something deliberately built. Free markets are portrayed as things which emerge naturally from human societies, but neoliberals have engaged in deliberate, concerted, and extremely laborious efforts to produce free markets along the twentieth and twenty-first centuries. For evidence on this, the reader may look into the very good histories of neoliberalism, some of which are referenced in the bibliography section.

Second, neoliberalism and neoclassicism are different things. Neoclassical economics is a particular way of doing economics, which is characterized by an emphasis on rationality and utility maximization; by an emphasis on equilibrium; and by a rejection of all strong forms of uncertainty, particularly fundamental uncertainty (Dequech, 2007; see also Dequech, 2011). Neoclassical economists may be neoliberals; Friedman and Stigler certainly were. But they may not be; Oskar Lange and Abba Lerner, who are discussed in Chapter 3, certainly were not. Neoliberals, on the other hand, are often not neoclassical economists. Friedrich Hayek and Ludwig von Mises, for example, were Austrian economists, who frequently reject the notions of rationality and of utility maximization and accept the existence of strong forms of uncertainty. There is an intersection between neoliberalism and neoclassical economics, then, but they are different things.

The history we will tell has two neoliberal neoclassical economists as protagonists. They are important members of the postwar neoliberal political movement, and are important neoclassical economists as well, having both received Nobel Prizes for their contributions to economics.

To see how they conceived the role of intellectuals in advancing political positions, we first look at where they were producing those conceptions: the Chicago School. The latter is a neoclassical school of economic thought, but also one of the most important neoliberal thought collectives. We sought to highlight the particularity sucha a combination creates in the

School in Chapter 1, where we discuss it. But above all we sought to emphasize the intrinsic intellectual variety of the Chicago School, within which Friedman and Stigler could divergence on their conceptions of how economists should relate to policies.

How they differed in those conceptions is the subject of Chapter 2. We map their conceptions of the economist's proper policy role, and show their historical evolution from the moment the Chicago School is born until the time Friedman and Stigler begin to diverge not only in their conceptions, but in the actual policy roles they played as economists. After having mapped their differences, we try to relate each of their personal positions to deeper epistemological conceptions the two had. We attempt this in Chapter 3 using the concept of a marketplace of ideas which, although a tricky concept, was useful in making sense of their divergence.

Chapter 1 – An overview of the origins of the Chicago School of Economics[2]

1. Introduction

Traditionally, the literature has separated the Chicago School of Economics into a prewar "old" and a postwar "new" Chicago. Recent literature, however, has come to identify a proper Chicago School only in the postwar period (Emmett, 2010a; Irwin, 2018; Stapleford et al., 2011). In the old view, Chicago was understood as a style of thought founded and maintained by Jacob Viner, Frank Knight and Henry Simons, and continued by Gary Becker, Milton Friedman, George Stigler and their associates. In the new view, however, the former three people are considered important but insufficient influences on the tradition of economic thought that emerged after World War II around the newer generation of Chicago-educated economists. The latter came to constitute an effectively original school of economic thought.

The core of this transition was the demise of pluralism. In its place, a distinct school of economic thought emerged[3, 4]. The prewar Department of Economics was characterized by institutionalist as well as neoclassical elements in terms of the theoretical and methodological approaches of its faculty and students (Rutherford, 2003; 2010; Fiorito, 2012). The coexistence, and moreover the possibility of combining various positions was the hallmark of its pluralism. After World War II, when neoclassicism came to dominate it, institutionalist faculty and students, and positions more generally, would mostly fade[5], erasing this pluralist character.

What we argue in this chapter is that this transition created in Chicago a neoliberal thought collective adapted to the American context. We do not intend this to be an original claim. That Chicago has profound relations with neoliberalism, that it is something embedded in the American context, and that it is in some sense a sociological phenomenon are all arguments that have been made in some form or another in the literature. As the first chapter in this Dissertation, our sole objective here is to familiarize ourselves and our readers with the literature on the origins of the Chicago School, so that

2 A modified version of this chapter was presented at the XXVII Encontro Nacional de Economia Política (Campos, 2022a).

3 A school of economic thought does not imply strict adherence to a permanent set of doctrines. See Section 3.3 for further discussion.

4 What exactly constitutes the Chicago School will be discussed further. It must not be confused with the Department of Economics at the University of Chicago: the cases of Armen Alchian (Benjamin, 2010; Mirowski, 2011), Aaron Director, George Stigler and W. Allen Wallis (Nik-Khah and Van Horn, 2012; Van Horn and Nik-Khah, 2020) exemplify this divergence.

5 The transition was gradual (Rutherford, 2010; see also Mitch, 2016; 2020 for greater details on Chicago), but by the early 1960s it was widely recognized as unique to Chicago, as Miller (1962) and Brofenbrenner (1962) innovatively identified.

some context is provided for the discussion that the following chapters make, and which we intend to have some originality.

We understand ourselves to be following the recent literature on neoliberalism conceived as a heterogeneous and flexible political and intellectual phenomenon (Plehwe, Walpen and Neunhoffer, 2006; Mirowski and Plehwe, 2015; Slobodian, Mirowski and Plehwe, 2020). In this literature, neoliberalism presents particular features in its many historical and geographical instances, while maintaining its central goal of building a competitive social order. Aligned with it, we here interpret the birth of the Chicago School as the birth of a neoliberal thought collective specifically suited to the American environment.

Our argument will be made in three steps. First, we will take a look at what American economics as a whole was going through in this moment of transition, which is necessary to contextualize the transition that happened in Chicago as part of this broader American movement. Section 2 takes care of the American transition, and the contextualization of Chicago in it is done in Section 3.1. Second, in Section 3.2 we examine the relationship of Chicago to neoliberalism, qualifying exactly how one influenced the other, and dismissing along the way some interpretations we think have the wrong emphasis. Third, in Section 3.3 we reconstitute how those two influences, the general American movement and neoliberalism, were institutionalized in Chicago, giving birth to a distinctively American neoliberal thought collective. Some concluding remarks follow.

2. American economics gets transformed

In the interwar period, American economics was not dominated by any single school of economic thought. In contrast with the postwar decades, when different branches of neoclassicism largely prevailed in academia, interwar American economics was pluralistic[6]. In a first approximation, we could see this pluralism as the coexistence of different traditions in economic thought, namely institutionalism and neoclassicism. In this sense, we could look, for example, at the presidency of the American Economic Association (AEA), one of the most important professional associations for economists in the United States. By looking at presidency names in the interwar period, we can see representative institutionalist economists such as John R. Commons (1917), Wesley C. Mitchell (1924) and John Maurice Clark (1935), as well as representative neoclassical economists such as Irving Fisher (1918), Herbert Davenport (1920), Allyn Young (1925) and Jacob Viner (1939).

Nonetheless, this interwar American pluralism was not rooted in a coexistence between institutionalists and neoclassical economists. It is, in fact, difficult to outline the borders between these

6 This pluralism did not encompass everyone, as the spirit of Lee's (2004a; 2004b) work importantly reminds us.

two schools of economic thought at that moment, and the urge to address the topic by finding these boundaries may represent the structure of American economics post-World War II — to which we are accustomed — more than the actual scenario before the war (Samuels, 2000). What seems to have prevailed then was a flexibility that is hard to conceive from our current perspective, with individual economists having the freedom to employ a range of different methods and concepts, and to study a variety of subjects without being deemed incoherent in their choices. Interwar American pluralism, beyond the coexistence of institutionalism and neoclassical thought in academia and inside departments of economics, was defined by a great heterogeneity within institutionalism and neoclassical thought, as well as within the formulations of individual economists (Morgan and Rutherford, 1998).

Beyond academia, the pluralism of economics entered into the policy sphere. Neoclassical and institutionalist economists already had influence on policy issues, as they had established that position in the Progressive Era[7] in search of relevance and credibility (Fourcade, 2009; Leonard, 2015), and maintained it during the 1920s and 1930s. The exact manner through which influence was exercised depended on the political affinities of the economist (Leonard, 2015) and on their conceptions of the economist's role in society (Biddle, 1998, 112-114), but there were no clearly established barriers which truly prohibited any economist from intervening in policy issues. Because of this absence of barriers to economists' intervention in policies, there was a wide range of possibilities for them to do it[8].

Among the different determinants of this transformation in American economics, a change in how a properly scientific economist was conceived seems to have been important. This change had as its main component a transformation of the conception of value neutrality. Value neutrality was already a preoccupation of economists, but after World War II it took a particular form in the US: rational choice-oriented mathematical formalism. This approach went beyond Economics and reached other social scientific disciplines (Erickson, 2010), and allowed economists to investigate phenomena hitherto considered noneconomic (Fleury, 2010). Notwithstanding this expansion, economics was the most receptive of the social sciences to rational choice-oriented mathematical formalism (Isaac, 2010).

Scholars have argued that this process, which has been qualified as a revolution in Economics (Blaug, 2003), originated from a few different reasons. This heterogeneity supports the claim that the transition to postwar neoclassicism was a complex phenomenon, nonreducible to a single explaining factor (Morgan and Rutherford, 1998). Notwithstanding this, the search for scientific

7 The Progressive Era is dated by Leonard (2015) from 1885 until 1918. Fourcade (2009) is included as a reference because, even though she does not mention the Progressive Era by name, she talks about the same phenomenon in the beginning of the professionalization of Economics in the US, which dates back to 1885 at the very least, when the American Economic Association (AEA) was founded.

8 See Balisciano (1998).

status in a changing scientific environment (Barber, 1996; Solovey, 2001) and the necessity to adjust the claims of Economics to a political environment marked by the Cold War (Balisciano, 1998; Bernstein, 1998; Barber, 1996; Fourcade, 2009; Goodwin, 1998) seem to be the central factors in explaining the general transition toward the formalization of Economics. The specific ways in which they acted upon the science can help to explain the benefits this process represented to neoclassicism to the detriment of the American institutionalist tradition.

The new environment made the search for scientific status depend on two central elements: (the appearance of) political neutrality and the direct social utility of scientific knowledge. The appearance of political neutrality as a benchmark for scientific status suited the neoclassical way of understanding economic phenomena because it poses reality as being ultimately determined by the decisions of rational agents acting upon a world of relative scarcity. Since it is all about rational, individual choices, elements of power or historical processes do not appear as anything more than environmental factors for economic behavior: market power by monopolists or workers' unions, market failures of all sorts and so on are all elements that each individual considers in his own maximizing calculations. What he will do in those circumstances, be they as they may, is always a matter of individual rationality. This, it seems, is useful to avoid accusations of partisanship. The Cold War environment, especially in the McCarthy[9] period (1948–1956), when academic freedom was effectively at a low point (Mata, 2010, 80), made that benefit for neoclassicism particularly relevant. The novel environment also demanded practical guidance for public policy in both domestic and international affairs. Neoclassicism, with its cost-benefit analyses obtained through rational choice mathematical models, fulfilled the task appropriately, both in military (Solovey, 2001) and more regular social and economic matters (Fleury, 2010).

9 The claim that McCarthyism had a real impact on economists' academic practices has been recently contested by Weintraub (2017). His arguments bear on many separate issues, which he considers to better evaluate the specific claim that McCarthyism caused economists to change their academic practices so that they would be more aligned with mathematical models. This would be due to the impression of neutrality mathematical models lent to Economics, but Weintraub finds that claim to be unsupported by existing evidence. We do not argue here about mathematical models specifically, but rather over neoclassical thought. In that realm, we believe it is reasonable to claim that McCarthyism was part of a political environment in which "allowed" (not persecuted) political positions were limited, and that economic theories which did not resort to relations of power to explain economic phenomena, such as neoclassical thought, were less likely to be considered aligned with forbidden political positions. Therefore, although Weintraub's arguments seem to be sound and to effectively contest a specific form of the proposition that McCarthyism did not cause a shift in Economics toward mathematical theories, a reformulation of this thesis might still be acceptable. This reformulation would claim that McCarthyism created an environment in which explicitly considering relations of power as explanatory variables in economic theory would make one more susceptible to political persecution, therefore creating a barrier to the reproduction of non-neoclassical approaches. This barrier was not a deterministic one, as heterodoxy remained alive, but it was fairly effective, as the reported harassment and ostracism cases of the period show (Lee, 2004a; 2004b).

Notwithstanding the variety of factors that contributed to this transformation of the scientific economist's conception, funding was an important one (Goodwin, 1998; Solovey, 2001). The funding of economic science after World War II was undertaken importantly by branches of the US military, but other government bodies, nonprofit philanthropic entities and private enterprises were also relevant. This reached some of the most important research centers in Economics in the postwar period, for example the Chicago School of Economics (Van Horn, 2018) and the Cowles Commission for Research in Economics (Mirowski, 1999), which received funding from private corporations and the military, respectively. How those funders thought economists should behave in their scientific activities was certainly an important factor determining what economists effectively did.

3. The birth of the Chicago School

Chicago economics went through a transformation that mirrored the broader American phenomenon. While American economics was changing, the theoretical and political pluralism until then prevalent in the University of Chicago's Department of Economics was gradually abandoned, and a school of thought with relative coherence was established. Notwithstanding the similarities between the two movements, there were two elements distinguishing the particular Chicago case from the more general American movement. Namely, these two elements were a particular conception of neoclassical economic theory and the political movement we today understand as neoliberalism. They were institutionalized into more stable structures that provided the nascent school with some unity, without making it a space hostile to debate and dissent. In fact, in this institutional structure important disagreements took place and were a constitutive part of how the school worked.

3.1. The pluralism prevailing pre-World War II

Pluralism was a central characteristic of Economics at the University of Chicago since the founding of its Department of Economics in 1892. Although J. Laurence Laughlin, its first department-head, was politically conservative, the department remained diverse. Neoclassical economists and institutionalists coexisted peacefully there at least up to the 1930s, when this balance began to shift favorably to the neoclassicals (Rutherford, 2010; Fiorito, 2012). Even long after the departure of its last great institutionalist, John Maurice Clark, in 1926 (Rutherford, 2003, 361), the institutionalist approach was not lost within the department. Frank Knight played an important role in keeping the institutionalist tradition alive. His influence was exercised through the graduate course on price theory and his own course on "Economics from an institutional standpoint" (Asso and Fiorito, 2013, 64). Notwithstanding his critical stance towards a number of institutionalist positions (Fiorito, 2000, 271), he had respect for the tradition and did not let it perish immediately in the department.

J. Laurence Laughlin, a political conservative, headed the department between 1892 and 1916, but did not impose his own political preferences upon it. Trained as a conservative economic historian in the then-prevailing Harvard tradition (Mitch, 2011, 241), he was a leading example of a conservative economist in the United States at the time (Coats, 1963, 489). He used his academic work and public prominence to defend businesses and to counter the progressive efforts for social and economic reform and regulation (Mitch, 2011, 241). But his political preferences were not imposed upon the department's decisions to hire certain individuals, nor did they produce a bias in its scientific production. Moreover, his work was not in accordance with the neoclassical approach that would be characteristic of the post-World War II Chicago School. As a participant in monetary theory and policy debates at the turn of the 19th to the 20th century, he defended a conception of price formation that resembled the institutionalist approach more than the quantity theory of money (Dimand, 2020). Thus, during Laughlin's long heading of the department, economic heterodoxy found in it a prosperous ground to flourish (Coats, 1963, 491).

As a matter of fact, Chicago may even merit the label of institutionalism's birthplace (Rutherford, 2010, 26)[10]. Until 1920, some of the most important institutionalist economists had passed through its Department of Economics: Thorstein Veblen, John Maurice Clark, Walton Hamilton and Wesley C. Mitchell were the main names (Rutherford, 2010, 25). This was not an exceptional benevolence of Laughlin toward institutionalism, but a structural feature of his pluralist policy as head of the department.

Some of the so-called precursors of the Chicago School started to arrive at Chicago circa 1920. In 1916 came Jacob Viner, who became a full professor in 1925 (Fiorito, 2012, 832); Henry Simons and Frank Knight arrived in 1927. In 1920 and 1926, respectively, Henry Schultz and Paul Douglas, two economists also said to have influenced the school (Reder, 1982, 3), arrived. Institutionalism was sometimes criticized by this group of economists, but they did not have anything like a coherent and relatively unified thought tradition that binded them together (Rutherford, 2010, 29). Furthermore, they had important disagreements among them and were criticial also of each other's works. Notwithstanding this, with them arrived to Chicago some of the elements that would be passed on to the members of the postwar Department of Economics, where the Chicago School would be born.

Some of the core ideas of the Chicago School were brought together with those economists that arrived pre-World War II. Henry Schultz and Paul Douglas, two pioneer quantitative economists (Reder, 1982, 3), were important in the promotion of mathematical and quantitative

10 The decoupling of the Chicago School from its institutionalist roots is discussed by Schliesser (2012).

empirical methods in the 1930s, later relevant, but not distinctive[11], elements of the economic approach of the Chicago School. Douglas helped to spread the empirical use of statistical tools (Cain, 2010, 270), but he was not an ardent adherent to neoclassical economics. In fact, he was sometimes quite critical of it. On the other hand, Schultz adhered more strictly to neoclassicism and was particularly concerned with finding empirical evidence to its fundamental tenets (Mirowski and Hands, 1998; Hands and Mirowski, 1998; Hands, 2010).

The influence of Jacob Viner was felt mainly through his graduate course on price theory. His course was obligatory for all students, and it was so demanding (Barber, 2010, 343) that many chose to first take its undergraduate equivalent, taught by Henry Simons, before taking Viner's course (Reder, 1982, 8). Despite this theoretical importance of Viner for the Chicago School, he distinguished himself politically from it. His active participation in the New Deal policies would make him differ with his then-students that would end up forming the School at Chicago. Moreover, he did not consider himself a member of the school, and was not even aware of its existence until after he went to Princeton, in 1946.

Unlike Viner, Henry Simons exercised his influence primarily through political positions, instead of academic work or teaching. He was initially located at the Department of Economics, but was sent to the Chicago Law School because of his lack of scientific achievement and teaching capacities (Kasper, 2010, 334). Despite this, his political point of view was much more welcome at the Department of Economics. His famous 1934 pamphlet *A Positive Program for Laissez Faire* contained propositions that were apparently well-accepted in the 1930s among Chicago economists. W. Allen Wallis, George Stigler, Milton Friedman and Aaron Director, a few of the prominent Chicago School members that were then only graduate students, apparently accept Simons' policy proposals (Van Horn and Nik-Khah, 2018, 100). They would come to later reject, at least in part, those proposals (Peck, 2011, xxxi-xxxii), potentially because of Simons' adherence to classical liberalism instead of neoliberalism[12] (Van Horn, 2011; Van Horn and Nik-Khah, 2018).

Although in the 1930s they were accepted by most of his students, the political positions held by Simons were not prevalent within the Department of Economics faculty in the mid-1940s. In 1945-46, notwithstanding his, Frank Knight's, Lloyd Mints', Viner's, and H. Gregg Lewis' presence in the department, he felt that he could only trust Mints and himself in terms of political positions. He reckoned more liberals were needed there, but did not conceive their hiring as possible, given the department's explicitly pluralistic hiring policy. Different schools of thought in Economics, then, were

11 In fact, it appears that Mathematics, because it spread so widely (Blaug, 2003), was relatively less popular in Chicago (McDonald, 2009, 172). An empirical tendency, however, seems to have been more important there (Hammond, 2010; Harberger and Edwards, 2022; Miller, 1962; Mirowski, 1999; Reder, 1982).

12 This association between Simons and classical liberalism has been contested by Caldwell (2011, 312-316).

sought to have proportional representation in the faculty (Van Horn, 2015, 97), which is further evidence of the plurality of Chicago Economics in the mid-1940s.

Among the precursors of the Chicago School, the influence of Frank Knight may have been the greatest. Also relevant, however, were his divergences relative to the positions of his then-students who would go on to form the school. His most important role was perhaps that of uniting those students in a tight network of personal relationships, which was called by a posterior analyst the 'Knight affinity group' (Reder, 1982). His liberal political positions were certainly to some extent shared by the Chicago School[13], and the central role he conceived for the price theory of neoclassical economics in understanding the behavior of economic agents and the economic system was also something on which the school members would agree with in some way. This is not unexpected, given Knight's relevant role in determining the content taught through the graduate price theory course until the mid-1950s (Emmett, 2006)[14].

Notwithstanding this, his critical stance toward quantitative and empirical techniques in Economics, as well as the broader socio-analytical framework within which he saw neoclassical price theory functioning appropriately made him differ substantively from his students. At least since the middle of the 1920s, he had been influenced by the German sociologist Max Weber, and this led him to conceive neoclassical economics as an 'ideal type' theory. In this light, actual economies could not, and were not supposed to, correspond directly to the theory. The latter was useful to understand the most fundamental basic relationships that governed the economy, but the historical and social content of it could not be appropriately known without resorting to comparative historical investigation (Emmett, 2006). In part due to this methodological preference, Knight ended up being mostly excluded from the postwar economics profession and from the Department of Economics. (He was primarily located at the Committee on Social Thought in the 1950s). This may also be the reason for the absence of any profound theoretical influence of him on the Chicago School economists.

The Department's faculty variety attests for its plurality before the 1940s. Economists of different theoretical, methodological and political orientations coexisted in the faculty, and some of them even combined traits from different traditions in economic thought. From the 1940s onwards, this plural environment would be restricted in favor of a particular orientation to Economics. What resulted

13 In a different manner from the postwar views of his former students, however. As Burgin (2009) remarks, Knight was a liberal in his philosophical beliefs more due to his disagreement with any available alternatives than to any unrestricted agreement with the tenets of a liberal society. He did favor it as the best available option, but found serious issues in its functioning and considered it a form of social organization doomed to fail. This vision of a liberal society contrasts with the more idealized picture of the market's functioning that came to prevail in the postwar Chicago School, of which Aaron Director's conception is representative (Van Horn and Emmett, 2015, 1453-1454).

14 Though he also used the same means to preserve the department's pluralism. See Asso and Fiorito (2013).

was a distinguished school of economic thought. The latter was not a rigid list of doctrines, to which one had to adhere to be a legitimate member of the school. Nonetheless, the school that emerged had an underlying unity that did not exist before the mid-century transition.

3.2. The neoliberal element in Chicago

Among the elements that marked the transformation of Chicago economics was neoliberalism. Beyond the shared presence of many individuals — particularly, in the beginning, those related to funding activities —, neoliberalism is linked to Chicago in a more fundamental way. We shall not overestimate the relevance of the particular political project of neoliberalism — that of building a competitive social order — as a determinant of the form the Chicago School took, but we cannot neglect it either. That form is also connected to the broader American context we have discussed above, both in terms of general political and social transformations, and in terms of the transformations through which economics went. Nonetheless, neoliberalism did play an important role. The economists of the Chicago School were profoundly immersed in the neoliberal political movement, and their commitment to it certainly influenced how they thought about the economic issues that occupied their time as academic economists.

The polysemic character of neoliberalism demands some brief comments. It is sufficient to note here, following the major works on neoliberalism from the perspective of the history of Economics, that it is a political movement, organized in the postwar period primarily around the Mont Pèlerin Society (MPS)[15] and aimed at reinvigorating liberalism for its perceived challenges in the mid-twentieth century (Mirowski and Plehwe, 2015). The MPS was, at the time — although it still exists today —, an organization that gathered many liberal personalities (politicians, journalists, businessmen, academics, etc.) to discuss the theoretical and practical features of this renewed liberalism in conditions of semiprivacy. As the main organizing body of a political movement, by definition dynamic, the discussions held within it were heated, and many disagreements among neoliberals emerged throughout the movement's historical development. There was not a single set of stable doctrines to which one had to adhere to qualify as a neoliberal; the very definition of neoliberalism was itself constantly subject to debate.

15 Before the war, an attempt to build what the MPS would become had already happened in the Walter Lippmann Colloque (1938). It was a reunion of mostly European liberals gathered to discuss the work of Walter Lippmann, a liberal American journalist, with a particular emphasis on his diagnosis of liberalism's critical situation in the 1930s. This early effort of renewing liberalism to build neoliberalism was interrupted by World War II, but some of the Colloque's participants, perhaps most importantly Friedrich Hayek, got the general project back on track with the 1947 Mont Pèlerin Society. There is a vast literature on the Colloque, especially on the field of neoliberalism studies. For a short panoramic view, see, for example, Biebricher (2018, 13-18).

Apart from the presence of Chicago economists in the MPS, recent literature has highlighted the intricacy of the Chicago School to neoliberalism in a shared origin. The fact that Milton Friedman, George Stigler, Aaron Director and Frank Knight attended the MPS's first encounter in 1947, and that other Chicago School economists were also active members in the following decades would be sufficient, in our view, to affirm that the neoliberal political movement should be taken into account when investigating Chicago Economics. Nonetheless, the important presence of the Austrian economist Friedrich Hayek in the founding of both groups is a link that is also worth examining to assess the strength of the connection.

The MPS was a Hayekian initiative unambiguously, but his role in forming the Chicago School is more controversial. Van Horn and Mirowski (2015) put forth the argument that Hayek, through the mobilization of funds and personnel, established the Free Market Study (FMS) at the Chicago Law School in 1946. The latter was a project initially aimed at producing an American version of Hayek's best-selling *The Road to Serfdom* (1944), led by Aaron Director, an enthusiast of Hayek's political ideas (Van Horn, 2013), and with the participation of Chicago economists (including Simons and, as a late but welcome addition, Friedman) and lawyers. According to Van Horn and Mirowski (2015), this would be the beginning of an important shift in Chicago Economics, marking the formation of the distinguished school of thought later recognized as the Chicago School.

The argument of Van Horn and Mirowski, then, is that the school's origins are part of Hayek's greater project to reinvent liberalism in the immediate postwar period (Van Horn and Mirowski, 2015, 158). The influence of neoliberalism would be enforced through a relatively close control of the intellectual production undertaken in the Free Market Study by its funder (whom Hayek connected to Chicago), the conservative Kansas City-based William Volker Charities Fund (Van Horn, 2018, 484). This makes the patronage of the study an important element in this narrative, since the intellectual activity that took place within it could have been relatively independent in a different financial context.

Hayek's role in forming the school, however, was much more relevant in its early phases than it was from then on, when the actual economic doctrines characteristic of the Chicago School would be built (Van Horn, 2015). He initiated the process, but the more it advanced, the less it resembled an adaptation of Hayek's ideas for the American public[16] (Van Horn and Mirowski, 2015, 166). In 1950, Hayek would go to the University of Chicago, not to the Department of Economics, but to the Committee on Social Thought, where he could more appropriately pursue his research interests unconstrained by the approach Chicago economists were developing, diverging from his own

16 These differences between Hayek and Chicago may be partly due to the shift of the neoliberal movement's center from Hayek to Friedman in the course of the 20th century. See Burgin (2012).

intellectual conceptions, closer at the time to social philosophy than Economics[17] (Caldwell, 2011; Mitch, 2015).

An important criticism directed at the identification of neoliberalism as a relevant cause of the Chicago School's birth is the absence of sufficient evidence to assert the influence of corporate funding on the school's scientific production. Caldwell (2011, 317-324) and Irwin (2018, 761, 763), for example, point out that the presence of Volker money would not be sufficient or necessary for Chicago economists to sustain the political beliefs they had. Particularly in the case of Friedman, whom Van Horn and Mirowski (2015, 168) call a proud "intellectual for hire", Irwin (2018, 763) argues that "Going through Friedman's papers at the Hoover Institute should convince anyone that Friedman was someone with deeply held convictions and was not a sellout to corporate interests".

We think those critics have a point, but they also miss one. To better understand this, we should distinguish between adhering to a particular political movement and maintaining that adherence. We should also grant that adherence to any political movement — be it neoliberalism, conservatism, socialism or any other — impacts the intellectual production of a professional economist, so that the adherence's effects are felt in the acceptance of an economist's professional work. If the economist adheres to political movement *A* in a social setting where political movement *A* is heavily criticized, his professional work will probably also be heavily criticized in that it mirrors, to some degree, that political adherence. In the light of this, we may say that money was not a primary factor in the Chicago economists' adherence to the neoliberal political movement, but it was a primary factor for the maintenance of that adherence. Had they not received sufficient funds due to their political inclinations' impact on their academic work[18] — which we have granted is absolutely certain to happen, to some degree —, they would not have achieved professional success. The fact that they did reach a high degree of professional success[19] is testimony to the existence of sufficient funds to maintain their political adherence, even if initially they opted for it out of sheer personal preference — which might have been the case.

Therefore, the influence of neoliberalism on Chicago Economics is at least twofold. In a first moment, the adherence of Chicago economists to the neoliberal political movement coupled their academic work to a particular political viewpoint, which influenced the former decisively. The decision to adhere to this particular political movement, we reckon, was not necessarily related to monetary incentives, but could have happened mostly out of sheer conviction. Throughout the following

17 It seems that Hayek was in fact mostly forgotten as an economist until he was awarded the Nobel Prize in Economics in 1974. From then on, his work started to be reinstated in Economics (Mirowski, 2020, 248).

18 On the presence of funding for free market economic ideas in the second half of the 20th century, see Backhouse (2005).

19 From 1969 to 2009, nearly half of all Nobel Prizes in Economics have been awarded to members of the Department of Economics at the University of Chicago (Van Horn, Mirowski and Stapleford, 2011, xvii).

decades, however, this particular political inclination was maintained in important part because it allowed them to access the financial resources necessary to make their work valuable and overall successful. A research program that does not attract any or at least sufficient funds usually cannot subsist for long, and Chicago's certainly managed to live a long and healthy life. Recognizing the nondeterministic influence of Volker Fund money in Chicago at its origins, then, does not seem to render the influence of neoliberalism any less relevant. The main source of neoliberal influence on Chicago economists at the school's birth was their deep commitment to it, not necessarily the money, and in its posterior development money was essential to make this commitment sustainable through the decades.

3.3. Institutionalizing the school

What was produced from the transition on was a neoliberal thought collective[20]. A thought collective is a group of thinkers engaging in social interaction to produce their thinking. This social interaction binds them in their intellectual endeavors, creating some unity during the time the collective exists, but it does not impose definitive limits. A thought collective is historical in nature, and its limits are constantly changing due to its members' own actions. Heterogeneity, therefore, is a constant, even if some form of unity or commitment is also always there[21]. The neoliberal character of the thought collective that emerged in Chicago is exactly the profound intellectual influence we considered just above to be one of the forms of influence exerted by the neoliberal political movement on the Chicago School from its birth[22].

According to this interpretation, it is precisely in heterogeneity that what constitutes the Chicago School is observable. It is not so much a set of close-ended theoretical, methodological or political propositions that characterize it, but a set of common political and theoretical problems that were open to a range of different answers, made coherent due to their institutionalized engagement with both Chicago-style neoclassical Economics and neoliberalism — which were, in turn, shaped by that process of collective institutionalized interaction. A central element in any thought collective, as this characterization of the Chicago School desirably makes clear, is a set of social and institutional structures that put the collective's members in interaction with one another.

The distinct approach to Economics that characterizes a thought collective is developed and maintained by its institutionalization (Garnett, 2012; Kolev, 2020). Curriculum, personnel,

20 The conceptualization of neoliberalism being comprised of different thought collectives, Chicago being one of them, was first put forth, to the best of our knowledge, by Mirowski and Plehwe (2015).

21 This stands in line with the literature on schools of economic thought. See Dow (2004; Negru, 2013; Kolev, 2020).

22 For more on thought collectives, see Wojciech (2019). Regarding neoliberal thought collectives, see Plehwe (2018).

graduate funding, and the overall infrastructure and material resources available to the organization within which the thought collective is being developed or has established itself are used for the preservation of its specific approach, enforcing its continuity over time. This continuity, however, is never guaranteed and is subject to potential inflections and ruptures. To forge responses in terms of Economics in a coherent, albeit not homogeneous, manner, an institutional structure was created in the Department of Economics at the University of Chicago to enable the nurturing of a "Chicago subculture" (Reder, 1982, 2). There was no answer given *a priori*; different ones were forged along the way through a process of concerted and more or less delimited collective thinking. To institutionalize such a subculture, faculty composition, curriculum, academic training and research procedures were transformed[23].

In terms of faculty changes in the department, 1946 was a seminal year (Mitch, 2016), but some prior events were also relevant. Beyond Simons' sudden death[24], in 1946 Jacob Viner left for Princeton, Milton Friedman was brought in to replace him, teaching the price theory graduate course, and Theodore Schultz became chair of the Department of Economics. Before 1946, Oskar Lange, a prominent mathematical economist and member of the Cowles Commission for Econometric Research, then located at Chicago, left in 1945 to serve his national Polish government; Henry Schultz, one of the pioneer econometricians, had died in 1938; Paul Douglas also left Chicago to pursue a political career after serving in World War II; and Theodore Schultz had arrived from Iowa State University in 1943.

Viner, although one of the intellectual precursors of the Chicago School, did not consider himself a member of it (Reder, 1982, 7), and had important disagreements with its Economics (Van Horn, 2011). As stated in a famous letter to Don Patinkin[25], Viner did not realize that there was a school of thought at Chicago when he was there. He only became aware of its existence when he returned to a conference after he had left the Department. To Patinkin, he wrote that, "at no time was I consciously a member of it [the Chicago School], and it is my vague impression that if there was such a school it did not regard me as a member, or at least as a loyal and qualified member" (Patinkin, 1981, 266, quoted in Reder, 1982, 7, brackets added). His departure, therefore, especially in light of being substituted by Friedman, which was not obvious at the time (Mitch, 2016), meant an advance in the path to build the Chicago School.

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23 Another relevant element in the process of institutionalizing the Chicago School was the creation, maintenance and reproduction of a network of academics defending its characteristic theoretical and political beliefs (Henriksen, Seabrooke and Young, 2022).

24 On Simons's death, see Van Horn (2014).

25 See Reder (1982, 7, note 19).

That Friedman was not the department's obvious choice to replace Viner is telling of its institutionalized pluralistic character still in the mid-1940s. At the time, the main debate was between the Knight affinity group (including Knight, Simons, H. Gregg Lewis and Lloyd Mints) and the Cowles Commission's members (Jacob Marshak, Tjalling Koopmans, and not as a member but as a sympathizer, Paul Douglas[26]). Their political and methodological differences were relevant, and they manifested themselves in this dispute to fill Jacob Viner's role as teacher of the price theory graduate course when he left for Princeton. Names as varied as John Hicks, Paul Samuelson, Friedrich Hayek, Lionel Robbins and George Stigler, apart from Friedman, were considered, and Friedman initially did not rank in the higher half of the faculty's aggregate preferences (Mitch, 2016, 1719). His hiring was the product of historical contingency, which stood in the way of some preferred options, such as Hicks and Stigler, and of Friedman's conciliatory character due to his experience in statistical analysis, something positively valued by the Cowles economists. There was not, then, a defined preference among the Chicago faculty in terms of economic theory or political positions[27].

That substitution was even more relevant due to the importance of the graduate price theory course as a means of acculturation for Chicago students (Reder, 1982, 9). Even without Viner's particularly harsh treatment of students (according to many accounts), which contributed to the overall difficulty of the course, between the 1930s and the 1950s, some relevant changes occurred in the structure of curricular responsibilities in the Department of Economics, helping to keep its relevance in that sense (Emmett, 1998). Graduate requirements were tightened, demanding students to follow a rigidly structured line of courses, examinations and research work. The focus of the courses also changed from problem-oriented to method-oriented[28], with neoclassical price theory coming to the forefront in both courses and examinations (Emmett, 1998, 144-145; see also Fiorito, 2012).

Not only did neoclassical price theory arise in importance within the course, but it did so in a particular version. The latter was primarily built between 1945 and 1951 by Friedman (McDonald, 2009, 165), who would go on to teach it from 1946 to 1964, and then again from 1972 until his retirement from the classroom in 1976 (Hammond, 2010, 7). Apart from drawing on annotations from a course he had previously lectured at Columbia and from his correspondence with George Stigler (Hammond, 2010, 12-13), he constructed the course, from its introductory readings, as a means to teach students more than neoclassical economic theory. He taught them a whole vision of the functioning of economics and the role of economists heavily informed by his own reflections,

26 According to Mitch (2016, 1718), Douglas had great professional sympathies for Cowles.

27 Mitch (2020) argues this persisted into the early 1950s.

28 Possibly related to broader movements in Chicago social sciences. See Emmett (2010b).

crystallized in *The methodology of positive economics* (1953) (McDonald, 2009, 168). Those reflections, to be extremely succinct, propose that the economist should be concerned with predicting economic phenomena in an empirically verifiable way, so that accurate predictions lend credit to the theories upon which they were formulated, whatever the logical plausibility - or realisticness - of the latter.

Another feature of Friedman's price theory course was its focus on applications to concrete economic problems[29]. He impressed on the course his conception of price theory as "a tool to solve problems rather than a set of problems to be solved" (Hammond, 2010, 10), an inheritance from his reading of Alfred Marshall's methodology. That is what might have led Miller (1962) to identify in Chicago economics an equalization of the actual and the ideal market: pure theory was used as a direct instrument to evaluate the economic reality's functioning. Bronfenbrenner (1962) also saw something similar. In his view, the emphasis of Chicago economists on the testing of hypothesis, notwithstanding its merits, is the counterpart of deliberate ignorance of many relevant aspects of reality, namely "the narrative or insightful history of how the facts developed to be what they are" (Brofenbrenner, 1962, 75).

This feature of the Chicago School variant of price theory can also be traced back to the events in which Friedman and other Chicago economists — namely, Stigler and W. Allan Wallis — were involved during World War II. According to Mirowski (1999, p.701), the application of Marshallian partial equilibrium analysis to concrete problems was something Columbia's Statistical Research Group (SRG) had imported from the British tradition in military operations research (OR). Through the participation of Chicago economists (Friedman, Stigler and Wallis) in the SRG, it was passed on to the Chicago School. This practical inclination was parallel to a nearly complete disconsideration of a realistic approach to human behavior, which is also present in Friedman's *as if* methodology, since the immediate application of partial equilibrium analysis taken up by both British OR and the Chicago School did not require an empirically sound explanation of the underlying fundamentals of the law of demand[30]. Enforced through the price theory graduate course built and taught mostly by Friedman, therefore, was this particular approach to neoclassical economic theory.

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29 This preoccupation with applications seems to have been a general feature of Chicago social sciences. See Bulmer (1980).

30 The supply aspect may also be an important — and, to the best of our knowledge, underexplored — component of the distinctive approach to economic theory and methodology developed in Chicago. We thank professor Paulo Fracalanza for pointing this out to us. Nonetheless, it does not seem to have been as contested within neoclassical economics in the immediate postwar period, where Mirowski and Hands (1998) and Hands and Mirowski (1998) pinpoint the origin of its three main branches — Chicago, Cowles and the revealed-preferences approach —, and that is why we don't feel the need to explore it here. Even if it may have been relevant for Chicago economics, it has not been relevant in the sense we are exploring here; that is, a feature that distinguished it from the other branches of neoclassical economics at that moment.

At this point, the influence of neoliberalism on the Chicago School can be seen in a way that is not related to corporate funding of the department's activities or faculty, as noted in the last subsection. Apart from being an inheritance of SRG's OR approach, the suppression of concerns about how actual behavior occurred in the economy was also functional for the neoliberal political movement. Friedman's *as if* methodology and the associated understanding of price theory allowed him and other Chicago economists to escape from complicated mathematical models and cognitive considerations, and go on to assert that the market worked perfectly fine through the supra-individual scale mechanisms of the law of demand — interpreted by Friedman in his 1953 essay as an evolutionary process —, making the market the best allocative mechanism available (Mirowski, 2011, 260). This has led Maas (2014, p.97) to claim that Friedman's methodology ultimately rests on political-ideological beliefs, something we do not necessarily endorse[31], but which contributes to an understanding of the Chicago School that accounts for neoliberalism's influence outside of financial ties to corporations.

The other important faculty change in 1946 was Theodore Schultz's arrival and ascendancy to organizational leadership in the department. His institutional role as the department's head was essential in building the school[32]. As a policy-driven intellectual, Schultz brought important assets to Chicago. Apart from the practical orientation of research activities, Schultz, who thought a free market required some form of planning to be installed and maintained, brought with him from Iowa a set of social networks connecting him, and now the department, to powerful actors (Burnett, 2010, 69). He navigated among them by painting his claims, which, like all economic theory, were embedded in particular political beliefs, as strictly objective social science (Burnett, 2010, 71).

He also pioneered and enforced the establishment of the workshop system at the University of Chicago during his heading of the Department of Economics, from 1946 to 1961. This type of initiative, which was not unseen at the time and was even inspired by the Cowles Commission's own workshops, took a distinctive format in the department due to the intense engagement of senior faculty (Reder, 1982, 2). The workshop system served as a means to normalize a scientific paradigm in Chicago economics, contributing to the formation of disciplinary research specialists, in line with what was happening in social science education at the time (Emmett, 1998; 2010b). This specialization was not, however, a "gradual turn inward" (Emmett, 1998, 138) in the sense that economic reality was disconsidered in research activities; quite the contrary, the workshop system was designed as "a laboratory for applied economic research" (Emmett, 2011, 94), and the concern of dealing with matters relevant to policy was there.

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31 For an opposing perspective on this point, see, for example, Hirsch and De Marchi (1990, 99-100).

32 Arnold Harberger, an important member of the school, recently pointed to Schultz's role in this process (Harberger and Edwards, 2022, 7).

In fact, the practical orientation of the workshop system suited fairly well the practical concerns many Chicago economists had in their positions as Mont Pèlerin members. As political activists for neoliberalism, faculty members such as Milton Friedman and George Stigler, who ran important workshops on topics of policy relevance (Monetary Economics and Industrial Organization, respectively), were certainly concerned with policy implications when they conducted the debates. Although perhaps not fully consciously, and probably with the best of intentions, as senior faculty members running the debates on current research matters in highly contested policy arenas, they most likely imprinted their own views on students and colleagues. By doing so, they were fulfilling the role Emmett (2011, 111) attributes to the workshops, and in which senior faculty were central: building "a common format of analytical criticism and research success" — in other words, building a certain set of questions and ways of answering them "correctly"[33].

Having set up that institutional infrastructure, the Department of Economics at the University of Chicago was in condition to become one of the central *loci* of the Chicago School of Economics' formation. The economists there managed to collectively produce scientific answers articulating the neoliberal political project's practical demands and their particular interpretation of neoclassical economics. They reached neither final nor completely homogeneous responses, as we have already stated; disagreements did occur, including on central topics of the school's work, but they were made coherent inside the shared social space of knowledge production that constituted this specifically American neoliberal thought collective.

4. Concluding remarks

In this chapter, we have argued that the Chicago School of Economics that emerged in the mid-1940s was a distinctively American neoliberal thought collective. To do so, we took three steps. First, we argued that the Chicago transition was a particular institutionalization of the broader contemporaneous transition in American economics. The broader transition, influenced in important part by a shifting conception of what it meant to be a respectable economist and by the Cold War political environment, made neoclassicism more apt to dominate American academic economics. The emerging orthodoxy was not homogeneous, and the Chicago branch of neoclassical theorizing had its specific characteristics determined by the personal trajectory of its members — for example, on Columbia's SRG and as neoliberals in the MPS —, as well as by specific features of the Chicago Department of Economics, such as the existence of a tradition in neoclassicism through Viner and Knight, among others.

33 Although Emmett does not show us exactly what was this shared format of analytical criticism and research success, we can turn to early analysts of the Chicago School such as Miller (1962) or Reder (1982) for some interesting characterizations. The latter seems particularly influential.

Second, we argued that this particular institutionalization was importantly marked, among other things, by the influence of neoliberalism. We specifically discussed the form of that influence, in which the characterization of neoliberalism as a political movement rather than a stable set of preconceived doctrines to be either accepted or rejected is central. Many Chicago economists participated in neoliberalism's central organization in the postwar period, the Mont Pèlerin Society, which means they were committed to a shared political objective as part of the movement, something that certainly influenced how they thought about Economics.

Third, to enforce those movements in a manner that was relatively persistent in time, although allowing for some flexibility, institutional structures were put into place to produce this specifically American neoliberal thought collective. The price theory graduate course was one of them, systematically transmitting to students not only knowledge on how economic phenomena worked, but also an approach marked by an empirical orientation of basic Marshallian neoclassical price theory and by the disregard of actual individual economic behavior. The empirical orientation was also enforced through the workshop system, which put students to interpret economic phenomena within relatively determined lines of reasoning via the example of more senior scholars.

The result of the transition, therefore, was an academic environment within the department that made it possible for a specifically American neoliberal thought collective to emerge. This process did not create a homogeneous intellectual environment, but a common ground on which Chicago economists could disagree on various topics while remaining united by the general acceptance of their version of neoclassicism and neoliberalism. Within this environment, the Chicago School economists were able to make the important contributions they made to Economics and to social thought more broadly, while also contributing to the dissemination of the political understandings and goals of the neoliberal political movement through their economics.

Chapter 2 - Milton Friedman, George Stigler, and a proper policy role for the economist[34]

1. Introduction

Among the important topics on which Chicago economists disagreed was the conception of what the appropriate role for the economist should be in policy issues. This appropriate role, which we call for short the economists' proper policy role, refers to the possibility and the desirability of interventions by economists in policy issues and to the specific form those interventions, if possible and desirable, should take. The disagreement that appeared within the School was led by two of its most prominent members, Milton Friedman and George Stigler.

Their divergence has been identified first, to the best of our knowledge, by Melvin Reder (1982, 25). In his canonical examination of the Chicago School of Economics, he notes that the divergence had been a well-known fact for at least ten years prior to the publication of his paper. Nik-Khah (2017; 2020) reaffirms the existence of this divergence in a 1972 conference held at the University of Virginia to honor Milton Friedman's sixtieth birthday and his *Capitalism and Freedom*'s tenth anniversary of publication. In the published version of Stigler's intervention in that conference, he says:

> As I mentally review Milton's work, I recall no important occasion on which he has told businessmen how to behave. [...] Yet Milton has shown no comparable reticence in advising Congress and public on monetary policy, tariffs, schooling, minimum wages, the tax benefits of establishing a menage without benefit of clergy, and several other subjects, and the rest of us have played this game only with less vigor and skill. [...] Do people in political life do not know their own interests, or do they not seek to further them? (Stigler, 1975, 312)

Further ahead in the published version of his talk at the conference, Stigler makes clearer his own position:

> The advance of science is the channel of the intellectual's major autonomous influence upon society. A rational society must accept tested scientific findings because they reveal a portion of the inescapable external world. Scientific knowledge must be accepted by men of all parties (Stigler, 1975, 316)

This moment identified by Nik-Khah as the one in which Stigler's distinct position in this topic emerges more clearly is also the one in which Friedman's position emerges more clearly.

34A modified version of this chapter was presented at the 50º Encontro Nacional de Economia as Campos (2022b).

Despite having been intervening in public debate before then, for example participating in radio programs (Nelson, 2020, v.1, chap.3) and in debates on the American Enterprise Institute (AEI) (Peck, 2011, xl), it was in the 1964 presidential campaign that Friedman effectively launched himself in the broader political arena as senior economic advisor for Republican candidate Barry Goldwater (Peck, 2011, xl-xli). Therefore, the mid-1960s marks the moment in which Friedman and Stigler apparently began to diverge more pronouncedly in their behavior as academic economists with regard to policy issues: while the former went on to intervene strongly in public debate, the latter restricted himself to academic circles.

This divergence between them would only become more clear in the following decades. There are at least two elements that allow us to state that: the Chilean episode and the *Free to Choose* television series. They are considered, of course, against Stigler's lack of direct participation in the public sphere, which is evidenced exactly by the lack of evidence on his participation. The Chilean episode[35] consisted in Friedman visiting the then-recently established dictatorial government of Augusto Pinochet in Chile in 1975. Pinochet's government is known as one of the first real experiences of neoliberalism, and many in its economic team were Chilean economists who had studied at the University of Chicago - the so-called Chicago Boys. The relevance of Friedman's connection to the Chilean regime beyond a loose intellectual influence has surely been questioned, but the fact that he went there to visit and talk about the new regime's economic policies reverberated strongly in the media and in society, projecting him in the public arena even more strongly.

The second element that stands for the increasing distance between Friedman and Stigler in the following decades was the former's *Free to Choose* television series (see Burgin, 2013). Aired in 1980 on the Public Broadcasting Service (PBS), the ten one-hour episodes of the series were widely successful. Their aim of transmitting to a broad audience the workings - and mainly the advantages - of a market system was achieved due to a combination of factors, including Friedman's academic standing as a Nobel Prize winning economist, his rhetorical abilities, which were well suited for television, and the large network of businesses and business-funded foundations that supported the dissemination of the ideas the series sought to transmit. This was a capital moment in Friedman's public exposition, as he reached an audience far wider than the politically and economically-interested people that had given him importance, even if negative, due to his involvement in the Chilean episode and in national economic policy debates.

However, the roots of their disagreement in this realm go far back. Reder (1982, 25) himself had pointed to the fact that Friedman and Stigler had begun to diverge intellectually in the mid-1940s. Those roots, notwithstanding their identification, have not hitherto been systematically

35See Hammond (2011) and Schliesser (2010).

analyzed to trace the origins of Friedman and Stigler's divergence on the proper policy role for the economist. What we intend to do is exactly that: reconstitute the origins of their divergence. We go a little further than the mid-1940s, identifying those roots since the beginning of their careers in the mid-1930s. To do so, we analyze the two economists' academic writings (understood as writings aimed for academic audiences) in the period 1935-1965, using them as sources on both their own actions as academic economists and their normative representations of the economist's proper policy role. There were certainly personal, psychological, pecuniary, and various other contingent factors in determining the divergence they displayed - notably after the mid-1960s - in their policy roles. Nonetheless, our investigation shows that how they conceived the proper policy role for economists was at least coherent, and potentially causally related, to their actual policy roles as economists.

To reconstitute the origins of their explicit divergence, we employ, in addition to an analysis of the content of their academic writings, a classificatory analysis. We divide their texts using two pairs of categories: empirical and theoretical; and policy- and 'pure-knowledge'-orientation. The first distinction pertains to the role of empirical evidence, methods and reasoning in economics - if the text is primarily concerned with doing or informing empirical work, it is classified as empirically-oriented, and it is classified as theoretically-oriented otherwise. It is relevant for the economists' proper policy role because it helps us understand how they conceived economists should intervene in policy issues, if this intervention is deemed both possible and desirable. The second distinction pertains to the role of policy-informing and -advising in economics - if the text is concerned with informing economic policy or with defending any specific type of economic policy, it is classified as policy-oriented, and it is classified as 'pure-knowledge'-oriented otherwise. This is more clearly relevant in helping us understand their conceptions of the economists' proper policy role, as it pertains directly to the issue at stake.

The paper has three sections, apart from this introduction and some concluding remarks. Section 2 deals with the years between 1935 and 1945, the two economists' formative years, when they were still experimenting professionally, but already demonstrating the fundamental difference that would later turn out in their divergence over the economists' proper policy role. Section 3 deals with the period 1946-1955, when Friedman and Stigler dealt more directly with the proper role for the economist in general, laying out the foundations upon which their conceptions of the economists' proper policy role could be built. Finally, Section 4 deals with the period between 1956 and 1965, when the latter conception is effectively developed.

2. Formative years, 1935-1945

We begin with the formative period of the two economists, from 1935 to 1945. During this

period, in the beginning of which they were finishing their graduate education, they both published a number of papers and worked on a few government agencies and as researchers for the National Bureau of Economic Research (NBER). In this period, the normative content on the economist's proper role in society is less clearly interpretable from their academic writings, therefore we resort more strongly to their biographical histories and to our classificatory analysis of their academic writings.

In terms of biographical histories, throughout their participation in government activities in various institutions and as researchers at the NBER, although what they did was to some degree similar, Stigler and Friedman had different perceptions of their experiences. In their memoirs, the two economists devote fairly different quantities of space to their involvement in the National Resources Committee (NRC), in the NBER, in Columbia's Statistical Research Group (SRG) and in other government and policy-related activities in the 1930s and early 1940s. To be fair, their style of reconstitution is also equally different: Stigler's memoirs are much shorter in number of pages and more reflexive than descriptive in content than Friedman's. Nonetheless, how they understand those experiences is telling and is partly reflected in the choices that led them to Chicago in the early 1930s, where they first met. Whereas Friedman was driven by the centrality and practical importance of economic matters in the context of the Great Depression (Friedman and Friedman, 1998, 33-34), Stigler remarks the importance of this context for the economics profession but highlights himself as being somewhat dislocated from it (Stigler, 1988, 51).

In his participation in government activities, Stigler does not seem to have been an enthusiast. He worked at the National Resources Committee for part of a year in 1935, but in the 1930s, his memoirs state that his main efforts were dedicated to his academic career (Stigler, 1988, 52). He worked at Iowa State College from 1936 to 1938, defended his dissertation in 1938, and went to work at the University of Minnesota. In 1942, he joined what would become the Office of Price Administration (OPA) but quickly left due to a perceived irrelevance of his efforts against price controls and in favor of combating inflation (Stigler, 1988, 59-60). He participated in Columbia's SRG, together with Friedman and their Chicago colleague W. Allen Wallis, but for a relatively brief time: he spent 10 months with the SRG, whereas Friedman spent 31 months and Wallis spent 45 months (Wallis was the SRG's leading organizer) (Wallis, 1980, 324). In Wallis' (1980) reconstitution, Stigler does not appear significantly, and in Stigler's (1988, 61-62) own recollection, his contribution to the group was not particularly important.

Stigler's contact with the NBER seems to have been of greater importance. He first joined the NBER in 1942 and only completely disconnected from it twenty-five years later, having contributed most in his Columbia years (1947-1958) (Stigler, 1988, 68), probably due to geographical proximity

with the bureau in New York. Stigler, considering the NBER experience as a participation in a distinguished school of thought (in equal standing to the Chicago school), reports his main lesson at the NBER as being the centrality of empirical validation of any theory if it is to be regarded as a guide to policy (Stigler, 1988, 71).

Friedman saw his policy experiences much more favorably and importantly than Stigler. He also worked at the NRC, where he felt “a great sense of excitement and achievement in the air” (Friedman and Friedman, 1998, 60) and the possibility of working for the benefit of society. When he went to the NBER in 1937, to work under Simon Kuznets on what would become his dissertation on income from independent professional practices, he converged with Stigler in stating that the bureau had great influence on his career. Whereas Stigler rapidly participated in the OPA, Friedman worked at the US Treasury Department’s Division of Tax Research in Washington from 1941 to 1943 and remarks in his memoirs how he felt “the sense of shaping the destiny of a nation, the excitement of the political process” (Friedman and Friedman, 1998, 110), quite differently from his colleague and friend’s disillusionment. In the SRG, where both participated, Friedman stayed for much longer and apparently, from his, Stigler’s and Wallis’ recollections, with greater enthusiasm and dedication to the group’s cause, that of using statistical tools to create predictions that enabled the government’s military arms to act effectively.

Beyond these biographical recollections, their conceptions on the proper policy role for the economist can also be inferred from their academic writings in the second half of the 1930s and early 1940s. Those writings are classified in Table 1.

Apart from various comments, reviews and short notes, Friedman published eight academic texts from 1935 to 1945: three with coauthors and five as the only author (Cole, 2019). In six of the period’s texts, he is concerned with empirical matters but not with policy applications in particular. He discusses empirical methods of estimating elasticities of demand (1935) and demand curves (1938), and with W. Allan Wallis (1942) they discuss how the prevailing theoretical concept of indifference functions is not particularly adequate for empirical estimations. He also discusses statistical methods that are used in empirical studies, such as the analysis of variance (1937) and hypothesis testing (1940). In Kneeland et al. (1936), in which Friedman does not seem to have had large participation, the details of an empirical study to be done in conjunction by the NRC, the Bureau of Labor Statistics and the Bureau of Home Economics are presented. Those six texts seem to be more concerned with ‘pure knowledge’ purposes than with policy applications.

In two of the period’s texts, he is concerned with policy matters, but does not approach the subject eminently empirically. In his text with Kuznets (1939), which was elaborated within the NBER and would later become his PhD dissertation, there is certainly a strong empirical basis for the

arguments they develop. As we interpret it, however, the main aim of the text is to develop a theoretical explanation for the data gathered on income from independent professional practice, making its orientation theoretical. The policy-oriented nature of this paper is not immediately displayed, but can be inferred from the context. The text did not explicitly propose any sort of policy to be followed, but it nonetheless generated controversy within the NBER due to its diagnosis of differential incomes in the medical profession as the result of monopolistic professional practices (Nelson, 2020, v.1, 73-76). This implicitly pointed to the breaking of that monopoly as the way out of differential incomes. In his 1943 text, he is more explicitly concerned with policy matters, as the paper discusses fiscal policy alternatives, and is also more explicitly in the theoretical side of the empirical classification, as his argument is not strongly supported by or concerned with systematic analysis of empirical data.

Stigler published nine papers from 1935 to 1945 (Longawa, 1993). Five of those texts are theoretical and are not concerned with policy matters. They discuss perfect competition theory (1937a), the economics of Carl Menger (1937b), the inadequacy of empirical demand curves to their sound theoretical counterparts (1939b), production and distribution theory (1939a) and the theory of duopoly (1940), all from a theoretical standpoint. In none of them he is primarily preoccupied with policy matters, even though the relevance of competition and monopoly for economic policy is not dismissable.

Other three of Stigler's texts from this period are also theoretical, but policy-oriented. He discusses social welfare analysis in a fairly critical manner (1938; 1943), pointing to its theoretical limitations as an orientation tool for policy decisions, and he discusses monopoly (1942), criticizing reports by the Temporary National Economic Committee due to the unsoundness of their theoretical underpinnings. That his discussion of monopoly is not solely concerned with advancing 'pure knowledge', but also with informing policy decisions, can be inferred most clearly from its motivation - debating with a government body interested in policy applications of that knowledge. He published only one text we consider empirical in its orientation (1945), in which he discusses estimating procedures for the 'cost of subsistence', a minimum cost diet. In it, he seems to be concerned with policy applications to alleviate poverty.

Evaluating together the biographical evidence and that available from the two economists' publication histories, we can identify differences regarding what the economist should do, and what they, as economists, sought to do in society in their intellectual formative period. From biographical evidence, Friedman, in this period, seems to conceive the role of the economist as an eminently practical one, directly influencing society through public policies and government offices. His publication history, however, leads us to believe that as an academic he preferred to do empirical

research without any clear policy application in mind, and at times deviate to policy themes in a manner that is more close to theoretical than to empirical analysis, albeit empirical evidence was also used in those efforts. In these formative years, therefore, his policy concerns did not penetrate that heavily into his academic publications. Stigler's biographical evidence, on the other hand, leads us to think he did not practice much direct intervention, nor did he do it with great enthusiasm. Nonetheless, his published work is slightly more concerned with policy matters than Friedman's. The main difference is in the empirical classification: whereas Friedman is an empirical worker, Stigler works mostly theoretically to make his points, be they concerned with 'pure knowledge' (the majority) or with policy matters.

Table 1 - Classification of Friedman's and Stigler's academic writings (1935-1945)

FRIEDMAN (1935-1945)		
	EMPIRICAL	**THEORETICAL**
POLICY-ORIENTED		1939 (w/ Kuznets); 1943
'PURE KNOWLEDGE'-ORIENTED	1935; 1936 (w/ Kneeland et al.); 1937; 1938; 1940; 1942 (w/ Wallis)	
STIGLER (1935-1945)		
	EMPIRICAL	**THEORETICAL**
POLICY-ORIENTED	1945	1938; 1942; 1943
'PURE KNOWLEDGE'-ORIENTED		1937a; 1937b; 1939b; 1939a; 1940

The difference in the two economists' conceptions of the proper role for the economist in this period, therefore, seems to be one of directness, or of proximity. Whereas Friedman sought to directly influence policy through government offices but not so much with his academic writings, Stigler sought to influence policy exactly through his academic writings. Friedman positioned himself fairly closely to the policy-making process while Stigler tried to restrain himself to academic environments, but both seem to have been equally preoccupied with how policies would turn out, albeit they may not have had a clear or definitive ideas of how that should be.

3. Standing academic ground, 1946-1955

Before 1946, the evidence indicates Friedman and Stigler were not particularly close (Hammond and Hammond, 2006, 2). Especially from that year onward, they would engage in close collaboration. They shared an office at the University of Minnesota for one year, and despite having published a single paper together in 1946 (Friedman and Stigler, 1946), their collaboration occurred intensely in the second half of the 1940s and early 1950s. The latter was particularly concentrated in the fields of methodology and price theory.

Their preoccupation with methodology and price theory was particularly important at the time due to the criticism neoclassical economics was under since the 1930s. That criticism manifested itself in what can be called in a broad sense the marginalist controversy (see Backhouse, 2009; Lee, 1981; and Mongin, 1992; 1998), an empirically-motivated questioning of the validity of the conception of economic decisions as made based on marginal calculations. Despite more or less direct interventions in the debate, which both Stigler and Friedman can be considered as having made, the general atmosphere for neoclassical economists was that of a defensive stance, and the two economists responded accordingly, each in his own way. As part of their efforts in that sense, they contributed to the conception of the economist's proper role in general, which would later inform their conceptions on the economist's proper policy role.

On the other hand, both economists were also preoccupied in the postwar period with the defense of liberal values against the then-identified rise of collectivist ideas[36]. This was the motivation for the creation of the Mont Pèlerin Society (MPS), of which both were founding and active members. The MPS gathered liberals of the world to discuss in semi-private conditions the current problems they faced as a self-perceived marginalized political group, and to desirably develop feasible solutions to them. Because Friedman and Stigler were members of the MPS and active participants in the political movement that formed around it, neoliberalism, we see this as a relevant conditioner of their conceptions on the proper role for the economist in general and in policy issues.

Due to their participation in the neoliberalism political movement and their role in defending neoclassicism, both economists began to have clearer ideas of what they wanted to stand for as economists. Differently from the preceding period, in which there is no clear indication of the path, either theoretical or political, that they were defending, from the mid-1940s those paths would be clearer for both. Of course, they were working in the neoclassical tradition since their graduate education in Chicago. But after the mid-1940s they became engaged in producing and defending a more specific type of neoclassicism - that which would be known as typically Chicagoan -, and also in

36Friedman remarks the importance of the Mont Pèlerin Society (MPS) for his political involvement in his memoirs (Friedman and Friedman, 1998, 158-159). He was already somewhat involved in political discussions before then (Nelson, 2020, v.1, chap.3), but the MPS was certainly an important moment in his trajectory.

producing and defending a specific type of political philosophy, neoliberalism. And what is relevant here, both were defending variants of the same theoretical and political programs: their objectives as professional economists were not substantively different. What distinguished them, and what we are interested in investigating here, is the role they ascribed to the professional economist in influencing policy issues, that is, the *way*, the *means* through which economists could help attain their (mostly shared) goals.

Table 2 - Classification of Friedman's and Stigler's academic writings (1946-1955)

FRIEDMAN (1946-1955)		
	EMPIRICAL	**THEORETICAL**
POLICY-ORIENTED		1947a; 1948; 1951; 1952a; 1952b; 1953c; 1953d; 1954[37]
'PURE KNOWLEDGE'-ORIENTED	1947b; 1947c;	1946; 1948 (w/ Savage); 1948 (w/ Hotelling et al.); 1949; 1950; 1952 (w/ Savage); 1953a; 1953e; 1955
STIGLER (1946-1955)		
	EMPIRICAL	**THEORETICAL**
POLICY-ORIENTED	1949c	1946; 1950a; 1955a
'PURE KNOWLEDGE'-ORIENTED	1949b	1947a; 1947b; 1947c; 1949a; 1949d; 1949e; 1949f; 1950b; 1950c; 1951; 1952; 1953; 1954a; 1954b; 1955b

One dimension in which we can observe the impact of this shared context in their academic writings, and begin to see their different responses to it, is our classification schema, presented in Table 1 for the period 1946-1955. In it, we observe that Friedman's and Stigler's academic writings were in large part theoretical (representing, respectively, 89% and 90% of their academic writings in this period). They also wrote policy-oriented texts (respectively for Friedman and

37Reprinted as Friedman (1968).

Stigler, 42% and 20%), but not as much as we would expect given their above-mentioned political compromises. Therefore, in this period it seems that they were primarily preoccupied with defending their ground in academia, not so much in the broader political arena, although that also took some part of their effort.

Despite the insightfulness of analyzing this classificatory schema, further evidence on their conceptions on the economist's proper role in general, and particularly in policy issues, can be obtained through an analysis of the content of some of their academic writings. Their theoretical writings were importantly concerned, in this period, with delimiting economics as science. Friedman tried to do it mainly through methodological and price-theoretic texts, in which a particular conception of the economists' appropriate day-to-day behavior was defended. Stigler, on the other hand, tried to do it mainly through history of economic thought work, in which a particular conception of the relevant traits of a mature science of economics was defended. Employing those two different strategies, Friedman and Stigler started to build their different conceptions on the proper role for the economist in general. They were coincidental in some respects, but they contained some relevant differences that would lead to their divergence on their conceptions of economists' proper policy role later on.

3.1. Friedman's methodological strategy

In his methodological and price-theoretic writings from this period, Friedman defends a particular conception of how the economist should ideally behave. His main thesis is that the economist should be able to generate predictions contradictable by empirical evidence. He defends that ideia in his 1946 paper on Lange's book coupled with a critique of what he would later label Walrasian economics: mathematical formalisms that leave aside matters of empirical relevance and even of direct reference to reality's categories. The same tone of criticism, directed at normalizing the conception of the economist as a predictor and defending a particular conception of non-Walsarian neoclassical economics, appears in his 1947 paper on Lerner's book. He also defends that notion of the economist's practical activities in his 1950 paper on Mitchell, praising the latter exactly for his alignment with that methodological precept of prediction, and in his 1952 paper with Savage, where the two gather evidence in favor of their expected-utility hypothesis of behavior under risk by testing its predictions.

The predictive quality of good economic theory is associated by Friedman with the Marshallian tradition in economics and opposed to the Walrasian tradition. In his 1949 paper on the Marshallian demand curve, he makes explicit the label Walrasian economics, associated with a branch of economics that values abstractness, generality, mathematical elegance and descriptive accuracy (the latter presumably, by the context, a result from the former three characteristics) over

empirical and practical relevance, represented by his own branch of Marshallian economics. In his 1955 paper specifically on Walrasian economics, he argues that the formal mathematical categories developed by Walras were a fundamental step in the advancement of economics as a science but were, in Friedman's time, already exhausted in their contributions. The current need of economics in his time was Marshallian-oriented empirical research to give precise, concrete content to those categories. Therefore, prediction should be the economists' orienting methodological precept to advance economics as a science by taking a step toward Marshall.

As a corollary of his defense of the predictive capacities of economics as its main asset as a science, Friedman defends in some of the period's papers the unnecessariness and even the undesirability[38] of realistic[39] hypotheses. Discussing individuals' behavior in situations in which uncertainty makes the outcome of choices unknown *a priori*, Friedman and Savage in their 1948 paper propose understanding the process of choice in those conditions as one in which expected values of outcomes are taken into account. However, they digress to clarify that they do not believe people actually calculate and compare those expected values: they behave *as if* they did, meaning that considering they act in this way generates predictions of real behavior that are sufficiently accurate. This would receive its most famous formulation in the 1953 'The methodology of positive economics', coupled with an evolutionary analogy of the pattern of leaf growth in trees (in which they grow *as if* trying to maximize sunlight reception, even if they do not know the principles of physics or biology) and reproducing the example of the expert billiard player that had already appeared in Friedman and Savage (1948) (in which the billiard player, who does not know mathematics or physics, plays *as if* he did). The idea that predictions should be favored over realistic assumptions, however, remains essentially the same in both texts.

Those two arguments Friedman puts forward delineate a role for economists: they must solve concrete problems, and to do that successfully they must use their science of economics to generate accurate predictions. At first sight, however, the concentration of his academic writings in theoretical texts during this period seems to be at odds with that image. The coherence of his tentative interventions in economic policy subjects from a theoretical standpoint with his normative defense of empirical economics as the basis for sound action over reality can be understood resorting to an October 4, 1948 letter to Stigler. There, Friedman delineates a four-step schema of the scientific process. The excerpt from the letter is worth full quotation:

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38 In a letter written to Stigler in November 19, 1947 (quoted in Hammond and Hammond, 2006, 65), Friedman says: "I should like to offer the general proposition that every important scientific hypothesis almost inevitably must use assumptions that are descriptively erroneous. [...] In a way, the better the hypothesis the greater the extent to which it simplifies, the more sharply will its assumptions depart from reality". The undesirability of realistic hypotheses would appear in his 1953 methodological essay as well.

39 What unrealistic means in Friedman's methodology is discussed by Hirsch and De Marchi (1990).

> One might, I suppose, separate out four kinds of things that economists and other scientists do: first, the collection of data to provide something to generalize from; second, the derivation of hypotheses to generalize the empirical uniformities discovered in the data; third, the testing of these hypotheses; and fourth, the utilization of them (Milton Friedman to George Stigler, October 4, 1948, quoted in Hammond and Hammond, 2005, 91-92).

Based on that rationalization of the scientific process, we can conceive that Friedman, in the texts in which he was concerned with policy topics in this period, used what he seems to consider already tested hypotheses to inform action instead of testing new hypotheses. He does so when discussing exchange-rate policy (Friedman, 1951; 1953c) and fiscal and monetary policy (Friedman, 1948; 1952a; 1952b; 1953d). In his texts with Savage (1948, 1952) and in his 1953a, he does execute the third phase of his four-step scientific process scheme by proposing hypotheses and testing them against empirical evidence.

The content of those texts shows us how Friedman sought to create a methodological image of the economists' everyday scientific practice that allowed them to intervene in the policy domain without sacrificing their scientific credibility. This image had as its central element the generation of predictions contradictable by empirical evidence. Nonetheless, as we have seen, Friedman himself in this period was more concerned with creating that image than with applying it in his own academic writings. As has been asserted elsewhere in a variety of ways (Burgin, 2012, 161-164; Maas, 2014, 96-97; Mirowski, 2011; Teira, 2007, 523; Vromen, 2009, 260; see also Teira and Bonilla, 2009), Friedman's conceptualization of the economists' desirable methodological procedures served in important part to clear the way for other tasks that interested him more, had he planned that in advance or not. We believe it reasonable to conjecture, in accordance with some of the literature on this point, that he did not want to concern himself with the realisticness of assumptions as long as predictions were accurate because his interest was in directly intervening in reality[40], and for that purpose accurate predictions were enough.

3.2. Stigler's historical narrative strategy

Stigler's work on the history of economic thought in this period is an important tool for normalizing a scientific paradigm in economics, containing a conception on the economists' proper

40 It is important to note that here the 'new' argument is that Friedman made it *possible* for economists to intervene in policies, but he did not defend, in this context, the *necessariness* or the *desirableness* of that intervention. I owe the clarification of this distinction to José Ricardo Fucidji. That Friedman *wanted* to intervene, beyond conceiving this intervention as possible, should be sufficiently well-evidenced by his actual interventions in the first decade of his career, as described above, and in the remainder of his career after 1955, as described below.

role in general, and specifically in policy issues. His writings of that sort are sometimes aimed directly at theoretical conceptions and sometimes aimed at the image of the good scientist. In the first case, he uses historical arguments to defend certain theories against existing or potential alternatives. In the second case, he praises systematic theorizing and empirical work as the means to reach a mature, cumulative status in the science of economics. His writings in that field were not exclusively descriptive of the history of economics, although much of it is quite remarkable historical work. Rather, they contained a picture of history with a preconceived direction orienting the analysis[41], so that the historical reasoning and documentation served the purpose of justifying a desirable path of economics toward its mature form[42]. In that path, two elements appear most clearly: the systematization of economic knowledge in a relatively closed and stable framework; and the centrality of empirical investigation, particularly regarding the theory's predictions[43].

In terms of contributions to economic theory through work in the history of doctrine, Stigler sought to defend some of the main tenets of neoclassical theory in his interpretation. Some of them were the negatively-sloping demand curve, marginal productivity theory, and the nonnecessity of microfundamentals to the law of demand. His notes on the Giffen paradox (Stigler, 1947b) serve to consolidate the view that the paradox, although recognized by an authority such as Marshall, was not as relevant as it is sometimes deemed to be, so that the negative-sloping demand curve holds its ground as a central theoretical element in neoclassical economic theory. His discussion of Wood's work (Stigler, 1947c), a relatively minor figure in the history of economics, even though he occupies the unique role of first receiver of an American PhD in the field, seems to be justified primarily by Wood's pioneering development of the concept of marginal productivity and its application to the discussion of income distribution. His history of utility theory papers (Stigler, 1950b, 1950c) defend a progressive line from approaches that tried to measure absolute utility to those that did not care for the psychological or utility basis of demand curves, as long as the latter were empirically identifiable - which is also what the Giffen paradox paper defends is the case, even if explicitly recognizing the difficulty and perhaps the impossibility of systematic proof. This approximates Friedman's argument for the irrelevance of a theory's assumptions' realisticness, since what truly matters is the empirical coherence of predictions - in this case, of demand curves, despite the absence of plausible explanations of the microeconomic behavior that generates them[44].

Those theoretical advances are put by Stigler in a timeline of scientific progress in

41Freedman (2007) agrees with this as a general feature of Stigler's work.

42Rosen (1993) also identified this normative dimension in Sitlger work on the history of thought. Rosenberg (1993) and Diamond (2005) emphasize its positive dimension.

43Those two points are also identified by Rosenberg (1993, 836, 846).

44On the absence of microfundamentals to demand curves in Chicago economics, see Hands and Mirowski (1998), Mirowski and Hands (1998) and Mirowski (1999). See also Hammond (2006).

economics. The latter begins with Ricardo and reaches its high point with the advent of more sophisticated statistical techniques of empirical investigation in the beginning of the twentieth-century. The fundamental contribution of Ricardo, even though in many theoretical points regarding the economy's functioning he was less right than some of his contemporaries and sometimes actually wrong, was the systematicity he gave to his analyses (Stigler, 1952). Differently from Smith or Malthus, Ricardo sought to give an ordered and systematic character to his economic thinking, anticipating a feature of modern-day (in Stigler's time) economics that is highly valued by Stigler himself and that he would a few years later (Stigler, 1955b) defend as an important part of scientific progress.

The advance of economics as a science, in Stigler's view, has as a second major component greater centrality of empirical evidence in scientific practice. This is also reflected in how he reads the history of nineteenth-century economics. In his interpretation of classical economic analysis (Stigler, 1949e), Stigler defends the view that classical economists employed marginal concepts and analyses to investigate concrete problems but not in their theoretical works. For Stigler, this is due to the greater appropriateness of marginalist concepts to analyze economic reality. The same cause, the coherence of marginalism with reality, is used to explain why this type of economics came to prevail throughout the nineteenth-century. Particularly at the end of the century, Stigler identifies an increased preoccupation with empirical studies in economics, as his investigations of empirical studies of consumption (Stigler, 1954a) and his history of utility theory argue (Stigler, 1950b; 1950c). As this step was taken by economists, economics as a science began to advance at a faster pace, supported as it was by advances in statistical techniques.

These two paths to a mature science of economics were used by Stigler in his critiques of contemporary developments in the discipline. In his (Stigler, 1949b) review of the *Survey of Contemporary Economics*, one of the central features of modern economics Stigler criticizes is the abandonment of historical evidence in favor of statistical investigations. His criticism is not based on a rejection of statistical studies, as his position is rather the contrary: he praises statistical empiricism. What he criticizes is that those statistical investigations are done in an unsystematic fashion, resorting to casual empiricism instead of the appropriate systematic evaluation of empirical evidence. In his review of Galbraith's book (Stigler, 1954b), Stigler again criticizes the unsystematicity of empirical evidence being used by economists, this time coupled with a critique of the unsystematicity of Galbraith's theoretical framework. In both cases, he sees the casual use of empirical evidence and the incompleteness of theoretical systems as negative features that keep economics from becoming a truly mature scientific discipline[45].

45On the perception of maturation of the neoclassical paradigm in Chicago, see Schliesser (2012).

In addition to its role in maturing economics, knowledge systematicity as a precept of good science served as a defense mechanism of neoclassicism from its critics. The idea that economics should advance to an increasingly encompassing, unified and coherent theoretical framework amounted to saying that neoclassicism should be the center of all economic theory, and alternatives should be incorporated into it without sacrificing its main tenets[46]. Developments that escaped the core of neoclassicism should be incorporated into it in its own language. Thus Stigler's criticism of imperfect competition theory, as well as Friedman's extension of neoclassicism to uncertain environments through his and Savage's expected-utility hypothesis. In both cases, features of the world that traditional neoclassical economics could not explain were incorporated without sacrificing the main elements of the theory, extending it beyond its traditional reach to preserve it from its critics[47].

As the above shows, in this period Friedman and Stigler conceptions on the economists' proper role in general, and specifically in policy issues, had mostly shared goals, but diverged in strategy. Both saw the economist as capable of generating knowledge that referred to the real world and that was relevant for policy issues due to its truthfulness, attained by the empirical testing of predictions and by the systematicity of their theoretical and empirical enterprises. Friedman, however, argued in favor of that view methodologically, prescribing how everyday scientific practice should be done. Stigler, on the other hand, argued historically, pointing to movements in the history of economics as evidence that the science was advancing in certain directions that were desirable in some of their components – in addition to the empirical testing of predictions, the advance of economic knowledge's systematic character. This difference in strategy is not minor, and it mirrors the difference identified in their formative years: it shows how Friedman sought to and thought it appropriate to intervene directly where he wanted changes done, whereas Stigler thought that to achieve similar results the best means would be indirect, mediated by the scientific community.

4. Dealing with the political element, 1956-1965

Friedman and Stigler had been defending a view of economics as a cumulative science throughout the first postwar decade, and they were not alone. American social sciences in general adopted a rhetoric of increasing scientific rigor and objectivity in their activities during that period (Solovey, 2001, 183). In the second half of the 1950s, however, and particularly from the 1960s on, this rhetoric began to lose traction as criticism regarding the social sciences' political nonneutrality arose, particularly due to their use by the US government's military. This was related in academic circles to the rise of civil rights movements. In contrast with the McCarthyism period (1948-1956), in

46Freedman (1995) and Demsetz (1993) also identify Stigler's proneness to extending the neoclassical paradigm instead of adopting alterantives.

47See Keppler (1998) for a different, but complementary take on this process.

which social scientists were intellectually constrained by political motives[48], what immediately followed was an explicit recognition of the connections between the universities and its scholars and society at large, coupled with an urge for greater academic freedom (Mata, 2010, 81-82).

In that context, Friedman and Stigler began to wrap their heads around the political problem and its relationship to science, shifting from a concern with the economists' proper role in general to a more specific concern with the economists' proper policy role. Their own trajectories made that wrapping around particular for each of them, and we do not suggest that they passively responded to that 'external' environmental datum. As we will see shortly, their particular academic positions in the period affected how they conceived and practiced the proper policy role of the economist in their academic writings. On the one hand, Friedman showed greater explicit alignment with political positions, particularly in terms of government interventions in social life and in economic policy. At the same time, he continued to publish highly specialized academic papers, in which those political alignments did not come to the forefront. When they did, however, it was as explicit advice, something that resembles his more direct way of trying to intervene in reality that was observed in the last two sections. Stigler, on the other hand, also engaged with the political element in his academic writings, but he did so once more from a distanced, mediated standpoint. Whereas Friedman defended certain political views, Stigler studied the engagement of scientists, specifically economists, with politics and political positions and events. In the course of doing so we can identify his preferred positions, but they are not explicitly defended as in some of Friedman's texts from this period.

In classificatory terms (as presented in Table 3), what we perceive is the increased relevance of empirical work by both economists. Friedman increased the percentage of empirically-oriented work from 11% in the 1946-1955 period to 50% in the 1956-1960 period, whereas Stigler increased the same percentage in the same periods from 10 to 22%. The preoccupation with policy issues was the main feature of 38% of Friedman's academic writings between 1956 and 1960, a slight decrease from its 42% value in the preceding decade. In Stigler's work, the same percentage fell from 20 to 11%. In Friedman's case, the maintenance of policy concerns with increased empirical work is rationalized in the following subsection by resorting to the content of his academic writings from this period. In Stigler's case, the diminishing preoccupation with policy topics is not paradoxical with the above-mentioned political preoccupation. Rather, as the content analysis of his work below will further argue, it is evidence that he sought to examine and intervene in the political process from a more distanced, mediated standpoint. An analysis of the content of both economists' academic writings, then, proves useful in interpreting those classificatory changes, and it is pursued in the following two subsections.

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48 See Weintraub (2017) and Lee (2004a/ 2004b) on McCarthyism in American economics.

4.1. Friedman and the dual strategy to advance monetarism

In the latter half of the 1950s, Friedman's academic writings came to bear more frequently on theoretical and policy-related macroeconomic issues. The context of that movement might have as an important element the fundamental transition in Friedman's macroeconomic thought identified by Nelson (2020, v.1). He argues that Friedman was not a strict adherent to monetarism in his macroeconomic thinking until the 1948-1951 period, during which he transitioned more strongly toward that theoretical tradition. This transition, which Nelson characterizes in terms of theoretical and policy positions taken by Friedman, can be seen as relevant for the form of the latter's conception of the proper policy role for the economist. Whereas Friedman was involved in policy-making and -advising before his monetarist years, he did not have to defend an alternative, marginal theoretical position, as his identification with monetarism was not yet complete. With his transition to monetarism, which was not a dominant theoretical tradition or an influential policy position in the 1950s[49], he would have to argue more strongly, in theoretical, empirical and political terms, to shift the environment of macroeconomic debate in a successful manner.

In that light, Friedman's methodological work in the 1946-1955 period can be understood as a path-opener for his work in the 1956-1965 period, when he would use his academic writings as tools to advance his then-marginal economic policy and theoretical positions. Having set his idealized image of the intellectual as an individual concerned with concrete problems and helping to solve them through the empirical testing of theoretical hypotheses' predictions, Friedman had a clear way forward concerning himself with concrete macroeconomic problems and arguing for his own positions as those more coherent with empirical evidence. In his texts from this period, we find two features of interest. The first is Friedman's dual strategy to shift the macroeconomic debate in favor of his positions. In the late-1950s, he would manage to do this by both stating his points in an extremely clear - and sometimes nearly pamphletary - manner and backing them up with strong, systematically collected and analyzed empirical evidence. In the early-1960s, he would abandon the use of nonacademic arguments in his academic writings and effectively turn outside of academia to advocate his preferred positions and policies, meanwhile keeping up the solid empirical work he had already been doing. The second is Friedman's increasing reliance on the public as the legitimate means to economic change, something that indicates his leaning toward the use of public debate to advance economic policy positions. In the late-1950s, this appeared in a theoretical recognition of the public's tastes as the fundamental of economic reality's current state, whereas in the early-1960s this appeared through Friedman's turn outside of academia to public debate as the locus of his intervention attempts.

49See Nelson (2020, v.1, 1761-177), but see also Lee (2004b) for some broader context.

Friedman, in a few of his texts from this period, particularly from the late-1950s, is extremely clear - and sometimes nearly pamphletary - in stating his preferred positions. In Friedman (1956), a book chapter in a volume edited by Friedman himself on the quantity theory of money tradition preserved at Chicago, he delineates clearly and defends that theoretical approach in proper academic form. In Friedman (1957a) and (1958b), this proper form is at times forgotten in favor of more pamphleteer-like arguments - that, to be sure, do not occupy the whole of those two texts, but only some portions of them. In 1957a, he argues against consumer credit controls. After some fine arguments derived from economic theory and from casually presented[50] historical evidence, Friedman asserts that consumer credit controls are dangerous governmental instruments if we want to live in a free enterprise society, and a very strong case should be made for them if they are to be accepted. He goes on to actual political propaganda:

> One of the trends of our time has been a trend toward an increasing willingness on the part of a large part of the community to accept intervention by the government into individual affairs. Consumer credit control is a small symptom of this general tendency. [...] We are all of us to some extent willing to accept degrees of government intervention which would have seemed rather astonishing and unacceptable at an earlier time in our history and which I trust will again become distasteful at some future date (Friedman, 1957, 16 on the Collected Works online version)

He had not hitherto taken such explicit political positioning in writings directed at academic audiences. A similarly approach is identifiable in his 1958b paper, where at one point he nominally says to have surveyed the history of the world - the complete one - and concluded from it that an inflation of 5 to 20 percent a year is not a serious threat to a free market system, whereas a nonfree market system would not be able to handle it as effectively. In both of those texts, we highlight the arguments that more closely resemble some kind of political propaganda on his part - they are certainly not the whole of either. Nonetheless, they do depart from his usual defense of macroeconomic positions as found in his pre-1956 texts. Somewhat different from his pre-1956 macroeconomic writings are also his more strictly academically-formatted texts from the 1956-1960 period.

In his 1957b text and in his 1957 paper with Gary Becker, Friedman makes a theoretical and statistical scrutiny of some claims in consumer economics. In the first case, he responds to a paper by Fisher on savings, consumption and income, in which the latter analyzes the behavior of those variables with statistical data from the United Kingdom. Friedman criticizes many points in Fisher's analysis and does so in an extremely qualified manner, discussing theoretical (mostly

50 It is casual in the context of this text, not in the context of Friedman's work as a whole.

formalized) and statistical issues seriously and in deep detail. His text with Becker compares the adequacy of different functional forms of the consumption function to explain empirical data for the United States. It does so by exploring statistical characteristics of different consumption functions, their theoretical underpinnings and their adequacy as predictors based on their performance on available empirical evidence. His 1959 paper argues in favor of his theory of consumption as an explanatory hypothesis for historical monetary movements in the US. It does so by using a great deal of statistical evidence to support the adequacy of his theory and recognizing its limitations when the data are not favorable. Both are topics of policy relevance, as predicting macroeconomic variables accurately has implications for macroeconomic policy management, but they do not resort to nearly propagandistic arguments such as having surveyed the history of the world or pointing out distasteful levels of government intervention in economic life to get their points across.

Table 3 - Classification of Friedman's and Stigler's academic writings (1956-1965)

FRIEDMAN (1956-1965)		
	EMPIRICAL	**THEORETICAL**
POLICY-ORIENTED	1958a	1957a; 1958b; 1961b; 1962b; 1964a; 1964c
'PURE KNOWLEDGE'-ORIENTED	1957b; 1957 (w/ Becker); 1959; 1961a; 1961d; 1962a; 1963 (w/ Schwartz); 1964 (w/ Meiselman); 1965 (w/ Meiselman)	1956; 1960; 1961c; 1963a; 1963b; 1964b
STIGLER (1956-1965)		
	EMPIRICAL	**THEORETICAL**
POLICY-ORIENTED	1962 (w/ Friedland); 1964b	1958b; 1961b; 1963a; 1963b; 1965c; 1965d
'PURE KNOWLEDGE'-ORIENTED	1956b; 1958a; 1961a; 1965a; 1965b	1956a; 1957; 1958c; 1959a; 1959b; 1960; 1961c; 1962a; 1962b; 1964a

These two groups of texts seem to point to a dual strategy aimed at shifting the

macroeconomic debate in favor of Friedman's positions. On the one hand, he is more explicit and adamant on his theoretical and its associated policy positions. On the other hand, his empirical work on the National Bureau of Economic Research (NBER) on consumption and monetary economics is generating papers that are profoundly academic in form, although ranging over issues of policy relevance. This could be understood in light of his marginal position in those matters at the time, yielding a dual approach to defend those positions: write effusively in favor of his preferred theory and policy recommendations and resort to solid empirical scientific work to back those positions. This dual strategy comes closer together in his 1958a chapter, in which he argues for the quantity theory of money as the appropriate explanation for price movements in the United States based once again on the empirical evidence collected in the context of the NBER project and the Chicago workshop on monetary economics. He then transits to policy recommendations that closely resemble those he defended in a more pamphleteer manner in other texts, but in a much more academically adequate form.

The second feature of interest for our investigation of Friedman's conception of the appropriate policy role for the economist is his attributing increasing relevance to the public as the legitimate source for bringing about economic change. In the excerpt reproduced above of 1957a, he attributes the increased level of government intervention in economic life to a disposition of a large part of the community to accept it and urges that the community will change its preferences away from government intervention. In his 1958b paper, Friedman claims that, although the easiest technical solution to inflation would be to maintain the quantity of money stable, we then have to face the political problem of a genuine desire of the population to do so. In his 1958a chapter, he claims that one of the advantages of a stable rate of change in the money stock over alternative policies is its "ease of public understanding" (1958a, 54), something that seems relevant for the success of any policy: a lack of public acceptance might create serious trouble for it. However, it is in his 1960 chapter in a volume in honor of his former Columbia professor Harold Hotelling that this point is made most clearly and generally. Friedman argues there, in general terms, that if something was preferable to the existing situation, it would have been chosen by the individuals and would therefore *be* the existing situation. Whatever it is that currently exists in economic reality is not merely a matter of technical conditions - individuals' tastes and preferences are determinant to what reality looks like. He does not go beyond that to defend economic change, but the natural conclusion must be that to change reality, one must act upon individuals' preferences and tastes.

In the first half of the 1960s, those two movements - his defense of monetarism and his appeal to the public as the source of legitimate social change - would be maintained, but in slightly different forms. This difference is due to the stricter separation of academic from nonacademic work

that can be observed through his academic writings from the period 1961-1965. In this latter period, he did not defend his political and policy positions employing his dual strategy described above for the preceding period. Rather, he restricted himself mostly to empirical or theoretical texts discussing issues relating to macroeconomics, especially to monetary economics and the relationship between the stock of money and the business cycle. He did defend his preferred policy position in those texts, but knowledge of macroeconomic phenomena, not policy, was the main component of them. How governments and societies should act was a byproduct of his work, not its central theme. His appeal to the public as the source of legitimate social change, on the other hand, would be kept, but leaving his academic work to enter his nonacademic work. This is manifest in the publication of his first explicitly political book, *Capitalism and Freedom*. He would also have, from 1962-63 onward, greater influence in policy circles and in public debate, in sharp contrast with his marginalized position in the preceding period (Nelson, 2020, v.2, 1-3). Even further, in 1966 he began his *Newsweek* weekly column, through which he managed to reach a greatly broader audience with his thinking and writing.

Most of Friedman's academic writings from 1961 to 1965 are dedicated to defending his theoretical and policy positions in macroeconomics. There are direct empirical works, such as his papers with Anna J. Schwartz (1963), with David Meiselman (1964; 1965), and his 1961d, defending the importance of the stock of money in business cycles. There are more theoretical works, albeit informed by his empirical work, making much the same point, such as his 1961c and 1964b, as well as papers that deal in a more or less historical fashion with the movements in monetary theory and policy in the United States, and that end up producing a narrative that is favorable to his own preferred theoretical and policy positions (Friedman, 1962b; 1963a; 1964a). Some of his papers are concerned with international affairs, mainly balance of payments and monetary standards issues (Friedman, 1961b; 1964c); one deals with his permanent income hypothesis (Friedman, 1963b), and two others can be considered primarily contributions to empirical techniques (Friedman, 1961a; 1962a). Most of those texts are not concerned with policy matters, and those that do so manage to avoid the near pamphleteer rhetoric that sometimes appeared in some of his work from the preceding period. From that moment on, it seems that Friedman separated more sharply his public debate persona from his academic works, conciliating the two in a way that rendered him an increase in both academic and political relevance[51].

51Friedman's relevance both as an academic and as an advocate for certain policy and political ideas may allow us to qualify him not only as a first-hand dealer in ideas, but also as a second-hand dealer in ideas, using Hayek's (1949) distinction. The second-hand dealers in ideas are those that disseminate ideas created by others in the public sphere, such as journalists, think tanks, and we might say today, internet influencers. Friedman played that role. But he also produced many of the ideas he disseminated, which makes him also a first-hand dealer in ideas, that is, a producer of ideas. Professor Manuel Luz has suggested Friedman's role as a disseminator of ideas through popular writings, media appearances, think tanks, and funding mobilization

4.2. Stigler and the conservation of academic and political life

The period from 1956 to 1965 saw one of Stigler's most relevant career moves: in 1958, he joined the University of Chicago's Graduate School of Business (GSB). There, he occupied the Charles R. Walgreen Chair for the Study of American Institutions, a chair created by the pharmaceutical businessman Charles R. Walgreen and maintained with funds from his Foundation to 'promote familiarity with the American Way of Life', as Walgreen's first donation to the University in 1937 stated (Mitch, 2020, 252). Walgreen was concerned with incentivizing further study and promotion of traditional American values when he created the chair, and his Foundation observed that task when determining who was to occupy it. Stigler's choice was not the first option of the GSB's Dean W. Allen Wallis, but it was the one he managed to convince relevant people in the Foundation that would satisfy the latter's requirements (Mitch, 2020, 255-257; Nik-Khah, 2011, 124-125). According to the expectation, Stigler had a good relationship with the Foundation in his period on the Chair. This was due not only to political affinities, to which Nik-Khah (2011, 126) points, but also to Stigler's sufficient flexibility to broaden his range of disciplinary interests to better promote the recuperation of traditional American values the Foundation sought (Mitch, 2020, 260-261).

His coming to Chicago is contemporaneous with two[52] movements in Stigler's work: first, his attention was turned to the relationship between academic activities and nonacademic events; second, the political element gained greater importance in his academic analysis. Despite his continued investigations over competition and monopoly as a theoretical and empirical issue and a few papers on the history of economic thought (Stigler, 1958c; 1962a; 1965a), topics with which Stigler was dealing since the 1940s, the late 1950s saw a novel movement in his intellectual trajectory and the addition of a new element in a subject he was already working on. The novel movement was the explicit treatment of political matters, which appeared in a paper that dealt with the goals of economic policy (Stigler, 1958b) and in a paper that dealt with the political inclinations of economists (Stigler, 1959b). The new element was added to his work on the history of economics, or, more generally, on his investigations on economics as an autonomous object, be they historical or not. In Stigler (1960), he adds to the systematization of theory and the centrality of empirical research the relative isolation from society's most urgent demands as a trait of a mature science of economics. In

amounts to an ideologial entrepreneurship.

52A third movement that is identifiable is his work on the economics of information (Stigler, 1961c; 1962b; 1964a). Although this is an important contribution he made to economic theory, we did not think it was closely correlated to his position on economists' proper policy role, so we opted to leave it out. His main contribution in this realm seems to be treating information as a commodity that has costs and benefits associated with it and that should be taken into account to explain economic phenomena, particularly deviations from the standard neoclassical model, such as the existence of only one price in a market for a homogeneous commodity. He then applies this insight on information as a commodity to the labor market and to the study of oligopoly.

some sense a part of both movements, there is his work on the Fabian socialists (Stigler, 1959a).

In the papers in which Stigler deals directly with political matters in the period 1956-1965 he defends a particular conception of political conservatism. Stigler (1958b) defends the idea that cultural components should be incorporated to a country's economic policy goals. In the United States, the goals of maximum output, substantial growth and minimum inequality of income cannot mean the same thing they mean in Soviet Russia, where the same general goals are pursued. Rather, they should incorporate what is specific to American culture: "the development of the individual" (Stigler, 1958b, 172). He sees individualism at a historically low point due to erroneous conceptions over the causality of social phenomena and to actually occurring transformations in society created by urbanization and industrialization. Notwithstanding those conceptions and social transformations, individualism should not be kept out of economic policy goals: to face the most pressing economic and social problems of his day, it is the individual, not big businesses or the government, who should be trusted (Stigler, 1961b). Individualism should be incorporated through the investigation of its contemporaneous forms so that it can be preserved and once again flourish in American society. If it was not directly related to Stigler's movement to the Walgreen Chair, the argument in this paper is certainly functional to the Chair's proposal of reviving the American Way of Life.

In his 1959b paper, Stigler more explicitly specifies what he deems to be political conservatism and argues for the importance of economics in advancing it. By doing so, he sheds light on how to achieve a more individualist economic policy: through economics. Stigler's definition of conservatism is worth direct quotation:

> I shall mean by a conservative in economic matters a person who wishes most economic activity to be conducted by private enterprise, and who believes that abuses of private power will usually be checked, and incitements to efficiency and progress usually provided, by the forces of competition (Stigler, 1959b, 524)

Defining it in that manner, he argues that economics makes one more prone to conservatism due to the discipline's thorough study of the competitive market's functioning, which makes alternative proposals of economic organization appear to be naïve or excessively simplistic. This is because those alternative proposals usually bear some degree of abstractness in their elaboration, which causes them to overlook important features of modern economies' functioning. Stigler further asserts that this particular political inclination of economics influences the direction and the substance of professionally produced economic knowledge. Topics more relevant for a conservative position receive greater attention and empirical relationships are valued differently according to their degree of coherence with the conservative worldview - something that does not hold if we consider the intellectual class as a whole (Stigler, 1965d). If one wishes to influence economic policy in the

direction of individualism or, which in Stigler's sense amounts to much the same, of conservatism, economics should be granted great power over it.

While this line of reasoning over political matters is being developed, Stigler is defending as a characteristic of maturity in science the relative isolation from current worldly events. In his 1960 paper, the increased specialization in economics is attributed to the advance of empirical investigation as a separator from economists and the real world. The only reliable source of realness left in economics are studies of empirical researchers, and in them lies the power to determine which transformations of the world are relevant, persistent and sufficiently disseminated in their impacts on economic life to be incorporated into economic theory. As they are specialized researchers, they are guided in their judgment by the discipline's own internally-determined criteria. This relative - not absolute, he emphasizes at times - isolation is considered a sign of maturity for economics, which we can reasonably consider as a trait that would lead economic knowledge to be generally closer to truth. Being closer to truth, economics would be in a better position to influence economic policies so that they would be better, or, in Stigler's view, more conservative, incorporating the traditional American individualist values.

In this context, it is positive for Stigler that economics is becoming more concerned with the scientific study of public regulation (Stigler, 1965c). Despite the frequency with which economists give advice to policy makers throughout the discipline's history, until the mid-1960s Stigler reckons this advice was ill-informed. Not because economic theory was faulty or its advice was necessarily wrong or undesirable, but because no systematic empirical investigation was made beforehand to assess the policy's potential benefits and costs. Stigler sees, however, and we can interpret it as a final sign of maturity for the science of economics in his view, that economists are becoming increasingly interested in studying the effects of the public regulation of economic activities. By doing so, they create an effective knowledge basis for informing economic policy. Stigler himself is a contributor to this movement through his 1962 paper with Claire Friedland and his 1963b, 1964b and 1965b (not all empirical, but all concerned with evaluating the potential impacts of public regulation). In this way, economics can contribute to organizing society more in accordance with Stigler's individualistic preferences while remaining relatively isolated from current events in the sense that all interactions are mediated by the scientific methods and community.

The criticism of the proximity of economics to current events as a sign of immaturity, and of collectivism as an undesirable form of social organization appear together in his 1959a paper. There, Stigler develops a thorough critique of the economic theory underlying the proposals for social reform of the Fabian socialists. He proceeds to criticize their economic theory as unsound and insufficient in that it cannot adequately account for any of the alleged problems of capitalism.

Nonetheless, those ideas had widespread influence on the economic and social policies of their time due to the fine rhetoric and debating abilities of the Fabians. Therefore, generally bad social reforms, those aligned with socialism, gained ground and contributed to the already underway movement toward collectivism based on unsound economic theory. This serves, it seems, as a historical illustration of the twin principles, that good economic theory can only be produced in relative isolation from current events and that economics is an important, albeit not necessarily the most important, element influencing the direction of political debate and public policy.

To be sure, Stigler was not completely isolated from current events. As he recalls in his memoirs (Stigler, 1988, chap.8), by 1960 he had appeared before congressional committees on competition issues on two occasions, in 1950 and in 1960, and would appear one more time later on in his career[53]. Reflecting on those appearances and the general relationship between the scholarly work and the political participation of economists, Stigler concludes that one cannot remain isolated from political interests once involved in the political process. The attempt to do so would probably lead to the economist's involuntary removal from the political process because the latter demands some (at least publicly displayed) fidelity to a political agenda. Although those thoughts are nearly three decades apart from his 1960 paper, the latter can be seen as a first moment of his thinking in that direction.

Those developments in Stigler's thought on the proper policy role for economists are reflected in his own work on industrial organization in the 1956-1965 period. The picture that emerges is that of an economist that works on strictly academic channels, producing papers for specialized professional journals and talking in terms of theoretical refinement and precision and of systematic empirical evidence. Stigler's work on industrial organization, in the period 1956-1965, seems to be just that: he approaches the subject from a more historical perspective and gathers some preliminary evidence (Stigler, 1956a), draws criticisms on the way empirical evidence has been gathered and analyzed by other scientists in the field (Stigler, 1956b; 1961a), refines the concept of competition and extends its theoretical treatment (Stigler, 1957; 1964a), and finally gathers and analyzes empirical evidence in a systematic way, both on the workings of competition and monopoly and on its regulation (Stigler, 1958a; Stigler and Friedland, 1962; and less systematically in Stigler, 1965b). He is dealing with an issue that has important implications for economic policy and that is central for its individualist or conservative character, since competition is one of the tenets of a free enterprise system, but he does so within the walls of the scientific community, trying to remain relatively isolated from current events.

In this period, when the political element came to the forefront, Friedman and Stigler coped

53Leube (1986) interprets this as 'several' appearances, but we, and apparently also Stigler, think this is unwarranted.

with it differently. Friedman was more direct in his defense of certain policies and political values and appealed to the people as the source of legitimate and as a necessary condition for social change. At the same time, he backed up some of his positions with solid academically-collected and -analyzed empirical evidence. The relationship between science and policy, therefore, was conceived by him as one in which the individual scientist could use his knowledge to directly inform policy-making. Stigler, on the other hand, did not seek to intervene directly in society or in policy-making. By studying the relationship between the political and economics as an academic subject, he concluded that economics was an important instrument in advancing his preferred political positions due to its innate characteristics, and therefore he was dispensed from direct intervention as long as he contributed to the advancement of his science, which he did. The relationship between science and policy, therefore, was conceived by him as one in which the individual scientist could only legitimately and effectively inform policy-making mediated by the scientific community and its collective consensus on policy-relevant matters.

5. Concluding remarks

Milton Friedman and George Stigler were two of the most important economists of the Chicago School of Economics, but they disagreed over the economists' proper policy role. This has been identified in the literature (Nik-Khah, 2017; 2020; Peck, 2011; Reder, 1982), but its historical origins had not hitherto been systematically explored. This is what we did in this chapter, investigating how, in their academic writings, this disagreement emerged and transformed itself through the years until it became notorious in the mid-1960s. From then on, it would only become easier and easier to identify the differences between Friedman and Stigler in this realm, as Friedman's public debater trajectory escalated in the 1970s and beyond, whereas Stigler remained mostly constrained to academic activities.

Their divergence appeared first in their formative years, when they were still experimenting professionally. At that moment, their differing relative preferences for participating in government agencies showed how they conceived the effectiveness of direct intervention in policy-making: Friedman thought it was a good means of action, but Stigler didn't seem to agree. In the first postwar decade, they started to more clearly formulate conceptions of the economists' proper role in general, motivated by the contested theoretical environment in which they were inserted. Then, because they were also beginning to form more consolidated theoretical and political positions that were quite similar, their differences were mainly strategic: whereas Friedman argued methodologically, Stigler argued through historical narrative building. This strategic difference contained a fundamental divergence between them: Frieman thought it was possible and desirable to influence real world

events directly, whereas Stigler thought economists could only achieve such a goal indirectly, mediated by the scientific community. In the latter half of the 1950s, then, when the political element gained relevance and was incorporated by them in their academic writings, this strategic difference gave rise to a more substantial difference in their conceptions of the economists' proper policy role. Friedman, on the one hand, began to explicitly defend certain positions (and at the same time back his positions up with solid academic work) and to appeal to the public as the source of legitimate policy transformation. He thought he could manage to change economic policies with his own hands, directly intervening in public debate, and that this could lead to substantial results. Stigler, on the other hand, went on to study the relationship between economists, economics and political events. He concluded that economics, due to its conservative character, would in the course of its appropriate development towards scientific maturity lead society at large in the right policy direction. Therefore, he did not have to be concerned with trying to influence policies, as the scientific community, in which he was actively participating, would be sufficient to achieve his desired policy outcomes.

Friedman and Stigler were part of the same collective of thinkers and, as part of the constitutive character of any such collective, they disagreed over important topics. By doing so, they were not being inconsistent in their positions as members of the Chicago School. Rather, they engaged in a concerted intellectual effort. In the specific case we have explored in this chapter, their disagreement over the proper policy role for the economist, this concerted intellectual effort ended up creating a sort of 'division of labor': while Friedman acted on the front of public debate to advance the School's theoretical and political positions, Stigler was more concerned with the academic dissemination of the latter. As we have noted, Stigler also participated in public debate, and Friedman also participated strongly in academic debate. Nonetheless, the forms they acted can still be regarded as complementary rather than competing, even if this complementarity meant that they disagreed over the proper policy role for the economist.

Chapter 3 - Milton Friedman and George Stigler on efficiency and intervention in the marketplace of ideas

1. Introduction

We have discussed how Friedman and Stigler, in the context of a collective of thinkers centered around the University of Chicago's Department of Economics, disagreed on an important topic: the proper policy role for the economist. We have also seen how, throughout the period 1935-1965, this disagreement developed in various forms while their fundamental divergence remained intact. Whereas Friedman saw direct intervention as an effective means of changing economic policies, Stigler saw them as subject to effective and legitimate change only indirectly, mediated by the community of academic economists. This was at the heart of their different behaviors as academic economists: while Friedman acted on the front of public debate to advance the School's theoretical and political positions, Stigler was more concerned with the academic dissemination of the latter.

If we have already shown what constitutes their divergence and how it evolved historically, there is still ground to cover in terms of explaining it. Surely there is personality, psychological factors, and a good deal of contingency in explaining why they differed in their strategies to influence policies as professional economists. But we argue in this chapter that their (mostly implicit) conceptions of the marketplace of ideas can be an important factor in that explanation. The latter concept has two different, although historically interrelated, meanings. The first refers to how the market functions as an information processor, which we shall call the marketplace of ideas in general, or simply the marketplace of ideas. It is preoccupied with what kind of information and epistemic capacities individuals have, with how they employ them in the market, and with the latter's epistemic role. It is important to emphasize that as it emerged with Hayek (a story which will be told ahead in the chapter), it was not merely a marginal dimension of the market. It was the central feature of all markets: they were conceived primarily as information processors, not as arenas of commodity exchange. Those epistemic features, therefore, were determinant for the market's functioning, and this turns out to be important when it is translated into the neoclassical idiom. The second meaning of the marketplace of ideas refers specifically to how the marketplace for scientific ideas functions. It is defined similarly to the marketplace of ideas in general, but applied to science. Their historical interrelation is due to the fact that the marketplace of ideas in general emerged previously, and its existence made it possible to apply the notion to the scientific realm: it was first necessary to conceptualize markets in epistemic terms so that the epistemic realms of life, including science, could be conceived as markets.

This definition of the marketplace of ideas has not been previously clearly stated, nor is it usually explicitly mentioned. Its use in the context of the history of economics has been made in the work of Philip Mirowski and Edward Nik-Khah[54] in their studies of neoliberalism and its penetration into economics, as well as into science in general. We believe the definition we attempted in the last paragraph is adequate in the light of the historical discussion they do on the subject, and that it will also be possible to extend it in that form to the historical discussion we intend to do in this chapter. But because of the lack of a clear definition and of explicit employment of the term, we deem it necessary to say that the discussion developed in this chapter is much more tentative and exploratory than those of the preceding chapters. It is an attempt at using this historically derived concept, that has been useful to understand other situations in the history of economics, to our particular research problem: Friedman and Stigler's divergence. We opted to attempt this given the dissatisfaction with how their divergence was explained in Chapter 2, although we believe its historical origins and general character to have been sufficiently well evidenced there.

In the way we have defined them above, the marketplace of ideas and the marketplace for scientific ideas can serve as the economists' epistemology, conveying what economists think about the nature and functioning of knowledge, and of scientific knowledge in particular. This is exactly why we think it can go a long way in explaining why Friedman and Stigler differed. As we attempted to show in Chapter 2, they did not seek strikingly different objectives: their difference was in how they sought to transmit their mostly shared objectives to people involved in policy-making, whether directly or through the scientific community. How one seeks to transmit knowledge depends on what one thinks are its nature and functioning, and, when we consider two professional economists, those same characteristics identified specifically in scientific knowledge can also gain great importance. Hence the relevance of the marketplace of ideas, both in general and in science, to explain Friedman and Stigler's divergence.

To explain their divergence on the proper policy role for the economist in terms of their marketplace of ideas conceptions, this chapter is structured in three sections, apart from this introduction and some concluding remarks. In Section 2, we investigate how the marketplace of ideas came to be, and how it came to Chicago. In Section 3, we investigate how Friedman responded to the penetration in his immediate intellectual environment of the marketplace of ideas by building his own conception of it. In Section 4, we show how Stigler, because of his different intellectual trajectory, built his own concept of the marketplace of ideas, in which he devised a special position for the marketplace of scientific ideas. His own formulation was better suited, in his view, to the defense of

54Mainly Mirowski (2009), Mirowski and Nik-Khah (2017), Nik-Khah (2017; 2020), and Nik-Khah and Mirowski (2019).

their largely shared policy goals[55].

2. Crossing the sea: how the marketplace of ideas came to Chicago

The marketplace of ideas first appeared in the area of law, but the concept that influenced economics is not this original one. Despite early appearances in the works of thinkers such as John Milton and John Stuart Mill, a proper concept of a marketplace of ideas only appeared in the twentieth-century (Peters, 2004). Its first official appearance is in the work of United States Supreme Court Justice Oliver Wendell Holmes Jr. Holmes' concept has been widely employed in First Amendment judicial practice and legal scholarship to defend the absence of regulation on speech as a generally positive thing[56]. But this version of the concept does not seem to have influenced economics.

The version of the marketplace of ideas that influenced economics originated in Hayek's challenge to market socialists in the Socialist Calculation Debate[57]. The Debate originated in Austria in the 1920s, when Ludwig von Mises ([1920] 1963) challenged the feasibility of a centrally planned socialist economy on the grounds that calculations that must be done for an economy to function properly could not be done without a market price system. This is because, for von Mises, only a market price system, with money prices turning the subjective evaluations of individuals into an objective standard upon which they could base their economic decisions, could enable rational decision-making. In the absence of those synthetic indexes of what is going on in the market, the latter's characteristic uncertainty regarding the future would not be nearly as well apprehended by the acting individuals[58]. The argument here is not yet the one later developed by Hayek, in which the emphasis is on the knowledge-transmitting properties of prices and the market more generally. Rather, von Mises' emphasis was on the role of money in enabling individual calculation: without money prices, there was no way of transforming subjective evaluations into objective, ready-for-decision-making information.

In von Mises' view, socialist economies were not amenable to economic calculation because money prices for the means of production by definition could not exist. Those money prices,

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55 Again, see Chapter 2.

56 The First Amendment of the United States Constitution deals with matters of freedom of speech. For some of the discussion on the jurisprudence of free speech and the marketplace of ideas, see Goldman and Cox (1996), Napoli (1999), Papandrea (2019), Sherman (2019), Smolla (2019), and Sorial (2010). Specifically on Holmes' view of the marketplace of ideas, see Blasi (2004).

57 This narrative in which Hayek was the originator of the marketplace of ideas concept as it prevailed in economics is built by Mirowski (2009), Mirowski and Nik-Khah (2017) and Nik-Khah and Mirowski (2019). On the Socialist Calculation Debate in general, see Vaughn (1994) and Cottrell (1998).

58 On the influence of von Mises' monetary thinking on his calculation debate argument, see Horwitz (1998). Horwitz points out that von Mises' emphasis on the function of money in economic calculation was largely taken for granted by the later Austrians, including Hayek, and largely obliviated by the neoclassical market socialists due to the latter's analytical apparatus.

which allowed rational decision-making to exist by enabling the comparison between values of alternative paths of action, emerged only when free trade was possible. For free trade to exist, however, private property was a necessary presupposition. In a socialist economy, by definition, the means of production would be a property of the whole community, and therefore not up for trade in the market. This would cause them to have no economic value crystallized in money prices. Without the latter, comparisons between alternative productive processes and uses of those means of production would not be possible, and economic calculation, which consists precisely in those comparisons, would itself be impossible. A socialist economy, then, could only be organized in bases other than economic rationality. Decisions could be made, but they could not be rational economic decisions.

This paper by von Mises posed a fundamental problem for socialist economists, concerning how to organize a socialist economy. Among the different responses that appeared from the socialist side, there were attempts to create a framework for the functioning of a socialist economy that would not fall prey to the problem raised by von Mises (Hayek, [1935] 1963a). This is what we could call the market socialism response. Those economists were concerned with trying to prove that an economy without private property of the means of production could achieve a rational organization and allocation of its resources, just like a market economy presumably did. Market socialism was particularly influential in the English-speaking debate, and it met Hayek as its primary intellectual opponent. In the course of their intellectual exchange to determine the feasibility of an efficient socialist economy, the marketplace of ideas would appear within economics. At this stage, as previously hinted, the marketplace of ideas in general would emerge, to be later applied to science by other thinkers.

The turning point for the matter to be reconceptualized in epistemic terms is Hayek's ([1935] 1963b) own synthesis of the English-speaking debate. In it, he says that the attempts of market socialists to devise an efficient procedure of economic organization for a socialist economy are overly concerned with the formal properties of the system. Market socialists employed general equilibrium models to show how the efficient allocation of resources they claimed a socialist economy would make was possible. But, in Hayek's view, no practical concerns were present in their proposals. Mainly, he points to concerns of epistemic nature that a socialist economy, according to him, would not be able to circumvent. Not only the amount of information needed to make all of the calculations necessary to solve the formal systems proposed by market socialists was way beyond human computational capacities, but there was information simply inaccessible for anyone but those in whom it was embedded. This was what Hayek considered to be a kind of practical knowledge, only transmittable through experience, and therefore unable to be centralized in any kind of governmental body responsible for economic planning.

This epistemic turn in the socialist calculation debate was first accepted within the market socialist side by Lange (1936). In the first part of his 'On the economic theory of socialism', Lange depicts the economic problem as one in which three types of data must be available for it to be solvable: (1) individual preferences; (2) relationships between preferences, or the ratios of substitutability between different goods or economic alternatives; and (3) resources available. In his view, von Mises saw socialism as impossible because (2) was nonexistent: the absence of value, since there was no trade in the means of production industries, made comparisons of preferences between alternatives impossible. Contrary to von Mises, however, Lange says that whenever we have knowledge on preferences and on the resources available, the determination of (2) can be obtained by the technical possibilities of production. The information and the informational capacities of planners in a socialist economy, therefore, are equivalent to those available for market agents in a capitalist economy. As in the latter, mistakes do occur, but through a process of trial-and-error the right terms of equivalence between different goods and economic alternatives are reached. If we can mimic this trial-and-error process in a socialist economy, which is exactly the task Lange (1936; 1937) sets himself, then there is no reason for it to be informationally inferior to a market economy.

Hayek's response to Lange led to his formulation of the marketplace of ideas concept. This happened in two stages. First, Hayek (1937) delved into individual epistemology to try and understand what economic agents had to know for the market result to obtain. His conclusion was that most of the information that was necessary for an efficient outcome in the market was knowledge that could not be easily transmitted from one individual to another. In fact, he ended up claiming that each individual was himself mostly ignorant of the economy and its functioning, but had only that knowledge necessary for his own practical purposes. After that first step, Hayek (1945) conciliated this individual ignorance with the efficient results empirically obtained in the market through the reconceptualization of the market as an information processor: if individuals were mostly ignorant, the partial bits of knowledge each of them possessed were transmitted optimally through the market's price system. Because this knowledge was mostly noncodified, a central planning board or any other governmental body had no prospects of achieving the same result. Hayek's marketplace of ideas, therefore, conciliated individual ignorance with market informational efficiency in a way that rendered deliberate attempts of economic planning bound to fail.

Once the marketplace of ideas was born in the heat of this debate, it got to the ears of Chicago economists through the Cowles Commission for Research in Economics[59]. The Cowles Commission had been founded in the early 1930s in the state of Colorado, out of the concerns of

59On the Cowles Commission, see Christ (1952), Dimand (2022), and Mirowski (2002). The brief history we tell of Cowles here is based largely on Christ (1952), a detailed reconstruction of the Commission's origins and early development.

businessman Alfred Cowles with the state of economic knowledge at the time. His concern was initially with the inability of economists to predict the 1929 financial crisis and the subsequent Great Depression, and he sought to take matters into his own hands to remediate that precarious economic knowledge. An enthusiast of mathematical and statistical economics, he financed the founding of a research institution within which he would gather empirically-inclined economists with a quantitative background to advance his cause for economic knowledge.

The marketplace of ideas got to Cowles through its personnel. We could say that the epistemic characteristics of market agents and of economists were something that marked Cowles from its inception, motivated exactly by a perceived lack of adequate knowledge from both these types of agents. Nonetheless, some of its members had been querying with Hayek on the socialist calculation debate, which we have seen was where the marketplace of ideas concept was born within economics. Nominally, Abba Lerner, who intervened in the debate on the socialist side[60], arrived in the Commission in its last couple of years still in Colorado; Oskar Lange arrived in the early 1940s; and Jacob Marshak, who had participated very early on in the debate on the socialist side, still in the 1920s, also participated importantly in the Commission's activities, ascending to its research directorship in the early 1940s. Beyond those names, as we will see ahead, other economists at Cowles were also preoccupied with similar matters relating to information and its relationship to the possibilities of economic policy and planning.

The transmission from Cowles to Chicago seems to have happened through the former's physical relocation, which led to the heated intellectual climate existent between the members of the then-nascent Chicago School and the Cowlesmen[61]. In 1939, after the Commission lost its research director and could not manage to fill the position due to its location, far away from the main research centers in the United States, it decided to move to Chicago. The decision had some grounds in terms of research interests, since an econometric project dear to the Cowlesmen had been established in Chicago by its faculty member Henry Schultz in the 1930s, but also in terms of personal motives, as Alfred Cowles saw it fit to be at Chicago due to the death of his father. Once there, the Commission would be installed in the same building where the Department of Economics was located inside the University of Chicago, and some of its members would hold joint appointments in the Commission and in the Department. For the first half of the decade, we can presume the coexistence of the Cowlesmen with other economists at Chicago was relatively peaceful, but in the second half of the decade things changed.

It was in the intellectual disputes of the Cowlesmen with a young Milton Friedman that the

60 Mainly through Lerner (1934; 1936; 1937; 1938).

61 This intellectual query is well-known, but see, for example, Nik-Khah's (2022) comments.

marketplace of ideas passed on to Chicago. Arriving at the Department of Economics in 1946 (see Mitch, 2016), Friedman soon developed an important position as the main debater of the Cowlesmen on all grounds, theoretical, methodological and political (Maas, 2014). Their intellectual query remained until Cowles left for Yale in 1955. During those nearly ten years, the exchanges they had were the source of Friedman's preoccupation with the informational capacities of markets and its influence on the economy, especially on the possibilities of planning it. He was, therefore, preoccupied with the marketplace of ideas in general, not specifically with the marketplace for scientific ideas. As Hayek's, his own concept's central characteristics were the primacy of aggregate market results over individual epistemic capacities and, what is distinctive of it, the dismissal of the possibility and of the relevance of knowing individual decision making processes. This would be a way out of the intellectual contest Friedman was involved with the Cowles economists, but it was perhaps more an avoidance than a resolution. While Friedman managed to defend the free market from economic planners, he had to sacrifice the sacredness of individual rational maximization[62] to do it. He did not deny it; in his view, individuals were maximizers. But he denied we could know this directly. It was only through market results that we could infer the rationality of individuals.

Friedman's marketplace of ideas opened up an important gap within the Chicago School that had to be addressed. This is because, differently from Hayek, Chicago economists were neoclassicals, and individual maximizing rationality was a central element of good economic theory for them. Hayek could more easily dismiss the rationality of individuals and emphasize their epistemic limitations. Chicago economists, on the other hand, incurred a greater intellectual sacrifice by doing so. Even if Friedman did not come to deny individual maximizing rationality, his assertion that we could not know it directly, only through market results, caused some internal conflict both within his School and within his own work.

Despite Friedman's own attempts at revising his epistemic response to Cowles further on in his career, his colleague George Stigler also attempted to solve the problem. Because Stigler was physically removed from Chicago until 1958, he was inserted in a different intellectual context, albeit also one of contestation. During the first postwar decade, Stigler was involved in the marginalist

62Individual rational maximization is taken here in its most generic meaning, that is, that individuals are able to adjust means to ends in a way that makes the utility or the profit they make out of their actions the maximum that could possibly be made given the constraints imposed upon them. It is a means-ends relationship. Rationality, optimality, maximization, and efficiency, in this sense, can be taken interchangeably. Of course, there are important discussions on all of those topics, and attempts at differentiating them, but they need not concern us. For our discussion, which attempts merely at understanding how Friedman and Stigler considered the possibility of rationality/optimality/maximization/efficiency to prevail in the marketplace of ideas and in the marketplace for scientific ideas, the way we have defined them is sufficient to distinguish their positions from one another. For more on rationality and those related concepts (which often get conflated or confused), see Blume and Easley (2008), Heap (1998), and Sent (2008).

controversy[63]. In it, the main issue was not the possibility of planning or the efficiency of market results, but rather the rationality of individual market agents. Due to this diverse context, Stigler formulated his marketplace of ideas concept differently from both Hayek and Friedman. In particular, the main difference is that Stigler transitioned from a characterization of the marketplace of ideas in general to investigate further the marketplace for scientific ideas in particular. By doing so, he found a way to preserve individual rationality in all contexts. Rather than reject the rationality of market agents, Stigler criticized the rationality of economists so that the former would be maintained. His addressing of Friedman's gap was not to dismiss, but to extend individual rational maximization beyond its traditional boundaries.

3. Some things you just can't plan: Friedman's marketplace of ideas

Friedman's marketplace of ideas mirrored Hayek's in that the epistemic advantages of the market were attainable on the aggregate process rather than by each individual participant. This is perhaps due to the shared intellectual context in which they formulated their concepts, within the socialist calculation debate. Friedman was not rigorously within the debate, but he was certainly debating participants of it and people generally favorable to the market socialist position. The market socialist position, as we have seen and will continue to see in this section through their followers' later writings, was fundamentally sustained by the possibility of predicting not only market outcomes, but individual decision making procedures. Lange, as well as other market socialists at Cowles[64], were preoccupied with formulating a general model of a market economy that built on individual decision-making processes that were known, and that generated an aggregate outcome that could also be known and that reflected individual behavior. To curtail the epistemic possibility of economic planning, then, both Hayek and Friedman sought to undermine the possibility of knowing and controlling individual decision making processes, meanwhile maintaining the epistemic qualities of the market to defend its efficiency.

When Friedman came to Chicago, his interests intersected with those of the Cowles economists. He would begin his postwar career by responding to work from Cowles economist Oskar Lange on macroeconomics, as well as to somewhat similar work by Abba Lerner. This macroeconomic thinking would be further explored throughout the 1950s in his involvement with the National Bureau of Economic Research and with Chicago's workshop on money and banking, but some of the main conclusions he drew in response to Lange and Lerner would remain intact. Because

63On the marginalist controversy, see Mongin (1992) and Cottrell (1998).

64Beyond the economists mentioned throughout this section, we should also mention Lawrence Klein (1946a; 1946b), a political follower and student of Lange (Assous and Carret, 2020) who gave quite a bit of attention to this problem of creating coherence between micro and macroeconomic behavior.

he came to teach the price theory graduate course in the Department of Economics, he also developed an interest in microeconomics, particularly in choice theory, another important area of Cowles research. In both of those fronts, he would collect elements to arrive at his marketplace of ideas conception, crystallized in his 1953 ‘The methodology of positive economics’. As we have hinted at, however, his conception would create some uneasiness both within Chicago and in his own work, so that attempts to resolve it in some way or another appeared throughout the following years.

Friedman’s first problem with Cowles’ epistemics was their faith in logic as the prime tool for understanding reality. This is clear in his 1946 review of Lange’s *Price flexibility and employment* (1944). Lange’s book is a theoretical investigation of the conditions under which price flexibility leads to a full employment equilibrium. Dismissing the usefulness of partial equilibrium analysis in the beginning, he goes on to build a general equilibrium approach to incorporate all of the determinants of the equilibrium adjustment process. His final conclusion is that the conditions under which price flexibility manages to reach economic equilibrium are extremely restrictive, and we should not expect it to be so but in an extremely special situation. Special as it may be, such a situation did happen between the 1840s and the 1930s. At the time he writes his book, however, those special conditions have faded, and price flexibility can no longer deliver its promise. What is needed, then, is an economic policy that intervenes on prices, quantities, market structures and monetary matters to evade the many potential pitfalls that can stop equilibrium attainment.

Friedman’s review of Lange’s book is not explicitly concerned with the latter’s propositions for economic policy, but rather with methodology. His review’s main argument is methodological: Friedman criticizes Lange because his arguments are strictly logical. As he sees it, Lange’s analysis does not consider facts from the real world as anything more than restrictions on the range of possibilities considered. There is no empirical evidence supporting Lange’s conclusions on the conditions under which price flexibility leads to economic equilibrium; the difficulty of achieving the latter is based solely on the existence of a number of factors that must function in a certain way for it to happen. The probability attached to those factors actually functioning appropriately is never mentioned, let alone empirically substantiated. To Lange’s approach, Friedman contrats his own preferred methodology. In the latter, a large number of facts yields some generalizations - a theory - that is put to test on empirical reality, leading, when contradicted by it, to a revision of the theory. Lange’s theory, as Friedman interprets it, is merely a very complex and sophisticated formal system of logical interrelations among categories that do not correspond directly to elements from empirical reality, and that therefore cannot yield hypotheses to be tested against it.

This methodological criticism has an underlying epistemological divergence. Whereas Lange thinks economists can achieve knowledge of the economy through logical reasoning alone,

Friedman thinks they can only achieve true knowledge based on empirical analysis[65]. The former is complete from the outset: something that is logically correct one time, is logically correct all the time. Furthermore, logical truths are attainably by reasoning alone, and any sound reasoning can reach the correct conclusions: there are no limits to the amount of truth a single economist can achieve. If knowledge is conceived in this way, then Lange's methodology is appropriate: there is no need to empirically verify economic propositions beyond logical coherence, and all truths can be known a priori. Friedman, however, seems to be opting for the empirical view. According to it, knowledge is always contingent and subject to further empirical testing. There is always the need to empirically verify economic propositions, and one can never be completely sure about the assertions economists are sustaining. As time goes by and empirical evidence accumulates, they can end up being wrong.

The epistemological option Friedman makes against Lange appears more closely related to the issue of economic planning and policy in his 1947 review of Lerner's *The Economics of Control* (1946). This is because Lerner's book is more explicitly directed towards a defense of economic planning. For Friedman, not only has Lerner committed the same methodological mistakes as Lange, he has gone further by applying this abstract reasoning to concrete problems without taking into account the social, political, and what is most important for us, epistemological realities inherent in them. The epistemological reality ignored by Lerner and by Lange is most clearly expressed in Friedman's criticism of Lerner's functional finance approach to macroeconomic management. He says Lerner's proposals are unsound not because of logical inconsistency, but because the imponent reality of insufficient knowledge to inform precise macroeconomic management is not considered. Governments cannot know the right timing to act on macroeconomic variables because this knowledge is simply not available for them[66].

The reconciliation of Friedman's epistemology of contingent knowledge with some assertion of fact in economics is made mainly through his microeconomics. This is where he considers the knowledge each individual agent in a market has, and separates it from the knowledge attainable by the market as an aggregate process. He, as any individual agent, faces the epistemological restrictions mentioned when commenting on the works by Lange and Lerner. But the market faces no similar restriction. As a collective process, it aggregates the thinking and acting of all the individual agents that participate in it and manages to produce results that are on average correct. This means that each individual can be wrong, but if we average out the direction and magnitude of their errors, the mean is right. Individually they may be inefficient, but when each individual action is considered in

65This dimension of Friedman's criticism of Cowles, based on his defense of a more empirical approach to economic theory, may be seen as a dispute over the meaning of empirical economics, as suggested by Espinel (2022). Whereas the Cowles economists did do empirical research, it was of a nature different from Friedman's own approach, and his criticism therefore portrayed the Cowles approach as excessively theoretical.

66This point is reinforced with a greater empirical basis in Friedman (1961).

the light of all of the other individual actions together, the result is efficient. As a collective of market agents, individuals manage to obtain results they would be individually incapable of obtaining: this is the marketplace of ideas functioning at its finest.

But Friedman's microeconomics is not all efficiency and maximizing - at least not at the level of the individual. In fact, in the midst of Cowles' criticism of individual agents' rationality and its impacts on collective results, the way he encounters to defend his marketplace of ideas concept is by dismissing the relevance of individual rationale and behavior nearly completely. Instead of defending the adherence of market agents to the optimizing rationality ideal, Friedman claims it is aggregate results that matter. Even if knowledge of individual rationality and decision-making procedures were possible, it would not be relevant or desirable. Whatever individuals are thinking and doing, what really matters is the aggregate results generated in the market. Individuals may or may not know how the economy functions, but the market can coordinate their actions to function effectively, as if they knew it all.

This position by Friedman was developed in response to the Cowles economists as they turned to the problem of creating coherence between individual decision-making and social results. Arrow (1950) framed the problem in welfare economics terms, and demonstrated, through strictly logical reasoning, that choices made by rational individuals did not lead to socially rational results. The individuals he considered chose not only based on their economic tastes - that is, self-interested orderings of different social states -, but also based on their values - more general valuations located beyond each individual's self-interest. That values should influence decision-making so as to render social results nonoptimal is something more easily acceptable even by economists who consider the market to be a perfectly functioning allocative system, and Arrow (1950, 333, footnote 10) even attributed this distinction between tastes and values to Milton Friedman in a footnote. But Arrow claims that considering only individuals' tastes would not alter the results: social optimality was not a necessary product of individual rationality.

That social results of rational decision-making need not be optimal was also argued by Haavelmo (1950). Discussing the notion of involuntary economic decisions, which he abstractly defined as those decisions that pertain to the whole set of rules that make up the economic system within which individual decisions are made, he claimed that the existing set of available alternatives for individuals to choose from do not necessarily lead to a social optimum. This is because the economic system itself can be transformed: considering the possibilities of a different system, the optimal social choice available under the current system may be suboptimal. To transform the system, however, individuals are separately impotent. What is needed is collective action, through means that cannot be voluntarily chosen - hence involuntary economic decisions. Because of this, the current market

economic system and the social optimum it lets us reach should not be taken to be the best economic result available to humankind: transforming the economic system through collective action is a viable and potentially desirable alternative.

Differently from social efficiency, individual efficiency was possible and we could know how to reach it. According to Marshak (1950), it was possible to devise a model of individual decision-making in which maximization of utility would be feasible. This is exactly what he does in his paper, adding an extra presupposition to the standard mathematical definition of rationality to arrive at the result that there is a class of linear utility functions that can be maximized by a rational individual. Those utility functions were not only maximizable, but knowable: he claims they are manageable in terms of mathematical manipulation, meaning individual maximization was something epistemically possible - he arrived at a formula for it. Therefore, whenever individuals deviated from these well-established and known presuppositions of rationality, it was reasonable to expect results to deviate from their optimum.

All of those points amount to saying that individual maximization was epistemically possible, and that it would not naturally lead to social efficiency. The natural conclusion in terms of policy was summarized by Koopmans (1951), research director at Cowles since 1948. He synthesized the Commission's arguments relating to the policy implications of individual decision making as conceived by its economists. He argued, referencing the Socialist Calculation Debate explicitly, that economic efficiency in terms of production allocation was epistemically possible without a market. The information needed to achieve efficiency was fundamentally technical in character, relating to input-output relationships and production processes that engineers could know. There was no essentially impossible epistemic task: planning could be done. When this is said in light of the limitations to the social efficiency of markets raised by his colleagues, the logical conclusion must be that central planning is desirable.

Friedman shaped his marketplace of ideas concept in response to those formulations by the Cowlesmen. His response was one of fundamental disagreement. Against Marshak, he claimed individual maximization was not something that one could get to know directly. It did happen in his perspective, but it was a phenomenon brought about by market forces which, through competition, made maximization a necessity for survival. Because it was only the market that could make this happen, leaving us in the dark as to how it happened on the individual level, planning was not epistemically possible. Against Koopmans, Friedman thought the knowledge necessary to reach the efficiency achieved by the market was not at our disposal, not even for the finest economist.

He did not, however, entirely reject the Cowles economists' points. Although his general position is that markets usually function adequately as information processors and therefore yield

efficient results, in some ways, and in some specific cases, Friedman accepted the epistemic limitations of markets. For example when values, instead of just tastes[67], are at stake, as Arrow noted, but also in cases where the source of epistemic failure was not specified. It was possible that the market would not function as it generally does in his view, transmitting information in an optimally efficient way. Whenever this nonoptimal situation happened, Friedman took the possibility of profound social transformation seriously. To fix the market, it might be sometimes necessary for collective action to take place so that the economic system in its structural or institutional aspects be changed. In those cases, we can conceive of him as an actor trying to change exactly the economic system to which Haavelmo pointed as a source of profound economic transformation.

The fundamental point of disagreement between Friedman and those Cowles economists related to the possibility of knowing how to maximize individual utility. It was not a disagreement on the maximization of utility: Friedman thought it was maximized just like the Cowlesmen thought it was. The issue at stake was whether or not we could know how to maximize it. From Marshak's point of view, as we have seen, we could: he even modeled it and claimed he could advise people on how to be rational. For Friedman (1953, 19-23; see also Friedman and Savage, 1948, 297-298), it did not matter what the individual thought he was doing, and in fact it didn't even matter what he actually did. What mattered was that the market results of individual behavior were efficient[68]: if individuals were not maximizing[69], the forces of competition would exclude them from the market. The truthness of actual individual behavior was to be inferred from the truthness of the predictions in terms of aggregate market behavior that could be derived from it. The actual behavior could not be known for itself; not even the individuals themselves, who were the agents of their own behavior, knew it.

If not even individuals themselves knew how to maximize utility, then they could be mistaken. In fact, in some of his later work Friedman had no problem in claiming that individual agents made mistaken calculations regarding their future economic prospects. As he remarks in a 1963 paper commenting on concepts he used in his work on consumption economics, the intertemporal nature of economic decisions would make this frequently the case. The relevant information for individual decision making is not related only to present or past events, but also to the future. Nonetheless, individuals' expectations regarding the future are not always correct: unanticipated events may cause

67As differentiated above, in the discussion of Arrow (1950).

68A similar point is made by Sent (2008) when discussing Friedman's conception of rationality. Her point is that Friedman largely neglected the issue of whether or not individuals were rational, and focused on predictable aggregate markets instead. This is also mentioned in the discussion Dimand (2022) makes of Friedman's relationship to the Cowles Commission.

69Alchian (1950), whom Friedman mentions in this discussion, can be considered as somewhat diverging with Friedman on the issue of maximization proper. Although he also proposed market competition would select only the most apt individuals, he conceded that the latter may not be maximizers. On this particular point, see Key (1995). On Alchian's and Friedman's arguments in this respect, see Benjamin (2010) and Mirowski (2011).

them to change what they consider to be the most efficient decision. This makes even decisions that are well informed considering currently available information liable to be bad decisions in the light of information that will only be available in the future, in the form of unanticipated events.

Friedman's point when defending the epistemic capacities of markets, therefore, was not that individuals behave rationally, but that the market did the thinking. If individually they made mistakes, when all decisions by all individuals are taken together and subjected to market forces the aggregate result is an efficient one. On average, differently from when taken individually, market participants made correct predictions of future events. Because of this, notwithstanding individual mistakes and epistemic limitations, the market result was that on average correct decisions were made and efficiency was reached. The necessary information to artificially recreate market efficiency was not attainable by the human mind: only market forces could canalize this information to produce an efficient aggregate outcome. This made central planning, and even Keynesian macroeconomic management, epistemically impossible. It was not a technical matter, as Koopmans had thought: the market did the thinking, and we could not reproduce it through other means.

Despite his fundamentally critical position on Cowles' economics, Friedman's allowance for individual mistakes ended up creeping up on his later work. His writings on education, for example, recognize some failures in this particular market that curtail its efficiency. In the chapter of his *Capitalism and Freedom* dedicated to education, Friedman (1962c) concedes the existence of a market failure that has as its source the fact that education relates to things beyond market tastes, to employ the distinction Arrow (1950, 333) attributes to Friedman: it relates to values. Especially in elementary and secondary education, the teaching of social values provides social stability, and therefore benefits the whole of society. Because of this failure in the market's capacity to transmit information adequately regarding the society's need for social stability through education, the latter merits public subsidy to be provided at a socially optimal quantity. Notwithstanding this recognition, Friedman argues that the government should only subsidize it, not administer education itself. But as the government does administer education, Friedman's proposal in this text is exactly a suggestion of transforming the educational system in a way that would (mostly) remove government from administering education and restrict its role to subsidizing it when market failures were identifiable. Therefore, Friedman's analysis of education presents a marketplace of ideas in which informational failures can occur, and can be responded to through systemic reform taken up by collective action.

A more general acceptance of epistemic market failures appears in Friedman's discussion of speculation. In a 1960 paper, in which he attempts to demonstrate that speculation which increases price variation may be economically beneficial, we can see a concession for imperfections in the marketplace of ideas. He argues that speculation, be it stabilizing (decreases price variation) or

destabilizing (increases price variation), may be economically beneficial whenever it arises out of individuals' decisions. This is not the case, however, when *avoidable* ignorance is present in individuals' decision making: if they could have known *ex ante* that the decision effectively made would prove wrong, but did not know, then speculation will not be economically beneficial. The causes of this epistemic failure, differently from his work on education, are not specified, but it is clear that he conceives them as possible. In that type of case, Friedman concedes that it is justified to have the government intervene in the marketplace of ideas to help spread the omitted information at the root of avoidable ignorance.

Because Friedman conceded the existence of exceptions in the marketplace of ideas' efficient functioning, intervention had some space to occur successfully. To put it in historical time, it seems that his concessions to suboptimal efficiency in the market's epistemic abilities appear more frequently in his writings from the early 1960s. This is perhaps due to the necessity to sustain a position more strongly aligned with the virtues of markets in his debates with the Cowles economists. In this later looser context, Friedman seems to have pursued some of the arguments he had drawn in the earlier period to their natural conclusions: if individuals were not necessarily always right, then some things might go wrong in the market as a whole. Notwithstanding its generally efficient functioning, Friedman came to concede that some particular areas of social and economic reality were subject to potential failures and inefficiencies. In those cases, he saw the need to try and remediate its functioning.

This functioning could be remediated through collective action. As Friedman notes in a few of his academic writings (1957; 1958a; 1958b; 1960), the existing state of things in the market as well as in economic policy depends fundamentally on the public's tastes. From an increase in the level of government intervention in economic life through the stability of the money stock's change rate to the general results obtained in a market, economic phenomena cannot go either beyond or above what people make it. Because of this, to transform the existing economic situation in a direction less subject to suboptimal market epistemic efficiency, the right information would have to be transmitted to large groups of people. This could in principle be done by anyone seeking to remediate those failures, but the results would not always be an improvement of the current situation: people were not perfectly rational, and they could fall for false prophets. Thus, in part, his great desire to participate in public debate on economic and social policy: to better inform the public as to in which direction to pursue systemic economic change. The need was for someone who had the objective scientific knowledge to make things better, and Friedman felt he was well suited for the job.

4. Trusting science to think for us: Stigler's marketplace of ideas

Although in some points Stigler concurred with Friedman's famous methodological statements, the abandonment of concerns with individual rationality does not seem to have been one of those points. He in many places adhered to the proposition that economics should be concerned with producing empirically falsifiable predictions (for example, Stigler, 1950b; 1950c; 1954). To recognize this as meaning that individuals' motives and rationality should not be a concern for the economist, however, was not something he seems to have sought to do. In fact, Stigler seems to have been unaffected by the criticisms raised to the idea that the neoclassical economic agent was perfectly rational early on, and to have stood for that rationality[70]. Differently from both Hayek and Friedman, Stigler did not dismiss individuals' epistemic capacities. Instead, he affirmed them further. All of the calculation and thinking that his two intellectual colleagues had attributed to the market process in their quests to undermine the possibilities of planning proposed by the market socialists, Stigler introjected into the individual. The information processing the market did was not so much, for him, a thing that happened outside the individual mind, but within it.

This is attributable in important part to the context in which Stigler was doing his economics. Differently from Friedman, his intellectual opponents were not the Cowles market socialists, but his critics in the marginalist controversy. The controversy was a movement of criticism of the marginalist theory of economic decision making, which portrayed economic agents as maximizing profits and utility by equating marginal costs and marginal revenues. This movement began in the United Kingdom with a group of Oxford economists (see Lee, 1981), but the spirit of criticism moved to the United States through the work of Richard Lester (1946) in the mid-1940s. Once in the United States, it evolved into a generalized criticism of marginalism as a theoretical endeavor, particularly aimed at its fundamentals in terms of individual behavior. In that context, marginalists, including Friedman and Stigler, sought to refute antimaginalist criticism.

Friedman's deemphasizing of the individual decision making process in favor of aggregate market results was also partly a response to the marginalist controversy (Backhouse, 2009), but Stigler took another route. In that context of contestation, Stigler did not feel the least cornered. His response to Lester (Stigler, 1947a) was of complete disbelief in the evidence he had brought to bear on the issue. Lester (1946), based on data gathered from questionnaires with businessmen, raised doubts on the marginalist explanation of firms' hiring behavior. Whereas the latter claimed that firms altered their number of employees according to wage shifts so that marginal costs (wages) and revenues (labor productivity) were equal, Lester's data pointed to movements in the firm's demand as the main sources of change in the number of employees. Furthermore, he took this discrepancy to mean that marginalism was not an accurate description of the behavior of firms in general, and that

70In fact, Hammond (2020, 611) has argued that individuals acting efficiently in the pursuit of their self-interest is the central tenet in Stigler's work.

more empirical studies on individual behavior should take place.

Stigler (1947) did not feel the need to respond to Lester on the same grounds. He did not bring evidence to support the contrary claim that marginalism was accurate, and disqualified Lester's own evidence. In fact, in a surprising statement for an economist who would go on to defend the empirical testing of economic theory, he asserts that Lester's evidence should not be considered relevant even if it had been gathered from thousands of firms (Stigler, 1947, 156). This, we reckon, is due to Stigler's absolute adherence to the principle that economic agents are rational decision makers. In his view, to disconsider the truthfulness of individual rationality and claim that only at the market level maximization was attained, as Friedman ended up doing to circumvent Cowles' market socialism, was both unnecessary and detrimental to economic knowledge.

The same posture in the face of criticism informed Stigler's (1947b) response to another antimarginalist criticism: the kinky demand curve. The kinky demand curve had been proposed in two different papers at approximately the same time: Sweezy (1939) and Hall and Hitch (1939). In both cases, what was at stake was firms' price responses to changes in demand: both texts argued that upward demand shifts did not generally lead to price increases, but downward demand shifts generally led to price decreases. The issue, however, was the reason behind this: for both Sweezy and Hall and Hitch, this happened because firms thought in a manner that was strange to marginalist profit maximization. In Sweezy's version, the firm acted according to an imaginary demand curve derived from its expectations of other agents' actions. As Sweezy (1938) makes clear in an earlier text, considering expectations on the future behavior of other market agents was outside the traditional scope of neoclassical economics: there was much more to individual decision making processes than equating marginal costs and revenues. In Hall and Hitch's version, this pricing behavior was a byproduct of the full-cost pricing principle, according to which firms priced by applying a profit mark up over their costs. This is also at odds with marginalism: the full-cost principle proposes firm managers had no consideration for marginal costs or revenues. In both cases, therefore, it was the behavior of firms that was at stake, and Sweezy and Hall and Hitch thought the latter deviated from standard marginalist assumptions.

Stigler's response in this context was akin to his response to Lester's wage-employment criticisms. For one, he dismissed the relevance of Hall and Hitch's formulation due to its unsystematicity and focused on Sweezy's. This was convenient as the former conception of the kinky demand curve carried with it greater epistemological problems for firms: they used a full-cost pricing principle because they did not have the necessary information to calculate marginal costs and revenues. Sweezy's formulation, on the other hand, was transformed in a set of predictions about pricing behavior that did not consider his fundamental challenge to the decision-making process

through which firms formed their prices[71]. As he had made clear in his 1938 paper, including expectations of other agents' behavior was a somewhat radical departure from standard neoclassical theory, with implications even for the definition of the scope and method of economics. Stigler, however, ignored this and translated him in strictly marginalist terms. By ignoring Hall and Hitch and misrepresenting Sweezy, therefore, he escaped the controversy in its central theme, which pertained to individual decision-making, without ever denying that firms and other economic agents maximized at the margins.

This absolute adherence to individual maximization was something on which Stigler elaborated further, and in a more general form, in a 1951 commentary. In it, despite initially praising detailed empirical work as an urgent necessity for the maturity of economics as a science, Stigler denies this relevance when practical matters are at stake. Whenever a practical conclusion must be reached, he says, the received (that is, neoclassical) theory deserves more emphasis than the existing empirical evidence. This is because the received theory is a codification of past experience, synthesized in a logical framework of analysis that is more useful as a guide to economic practice than any amount of empirical evidence. We can take this to mean that Stigler believed that most complications that were brought to bear on economics by empirical evidence amounted to not much beyond already established knowledge. Trying to add all sorts of incentives and motives for individual economic behavior is usually much less adequate than simply considering what experience has proven to be right: that people in the market are looking to rationally maximize what they get.

Throughout the following years, the marginalist controversy came to a halt. This happened because neoclassical economists managed to absorb much of the criticisms raised by antimarginalists into their own analytical framework (Lee, 1984). During approximately the same period, Cowles had also left Chicago, so that when Stigler came back in 1958 the environment was not as contested as before. A clearer path to discuss and reformulate the tenets of neoclassicism was what he found at his old intellectual home. As we have suggested, Friedman, because he had formulated his marketplace of ideas concept in response to the Cowlesmen in a way that rendered individual epistemic failure possible, in that less contested context deviated to accept the possibility of market imperfections. Stigler, on the other hand, because he had been keeping on to individual rationality despite all criticism, seized the opportunity to reestablish the centrality of individual maximization on new bases.

Stigler did flirt with Friedman's irrelevance of assumptions hypothesis at that time. In a paper on 'The economies of scale' (1958), Stigler argues that the best available technique to empirically investigate the existence and the determinants of economies of scale is what he calls the survivor principle. Economies of scale refer to the gains in productivity that arise from an optimum firm

71On this episode, see Freedman (1995).

size. The survivor principle that Stigler employs to study economies of scale consists in the notion that firms that have the greatest economies of scale in an industry survive, increasing their share of market output, whereas the remaining firms do not, decreasing their share of market output. Therefore, tracing the survival of firms of different sizes would be a way of getting to know the optimum firm size in that industry: by predicting the final result, we can infer what was the correct presupposition in terms of the operation of economies of scale.

Even when coming close to Friedman's methodology, however, Stigler did the opposite of denying individual rationality. His survivor principle, states him in the beginning of the same paper, is something derived from the accumulated knowledge all sensible men must have. Contrary to the dispensing with individual rationality that Friedman was induced to make to defend himself against Cowles' market socialism, Stigler affirmed the rationality of the economic agent, and affirmed it as even superior to the knowledge held by economists. He, as an economist, was drawing on the knowledge of sensible practical men to produce his scientific knowledge.

This faith Stigler had been demonstrating on individual rationality was formalized by him in his work on 'The economics of information' (1961). In it, he departs from the observation that ignorance is pervasive in the economic environment, so that no individual has all the relevant information to make his decisions. In such an environment where ignorance prevails, individuals can profitably engage in a search procedure for information. This search is costly, mainly in terms of time but also potentially in monetary terms, and it yields benefits for those who engage in it: smaller prices for buyers and larger revenues for sellers. Because individuals are well-aware of the costs and benefits of information search, the degree of ignorance that remains in any market is not something epistemically impossible to achieve. Rather, searching for still more information is unprofitable: to do so is a priori epistemically possible, but it would not pay for individuals to do it. Even if signs of ignorance are found in the market's operation, they are not evidence of irrationality, but of rationality in the face of costly information search procedures - something economists until then ignored completely, but of which practical economic men were well aware.

In such a context, to minimize the costs of attaining information individuals develop devices to centralize search[72]. This is the case of advertisement, of specialized traders and of information pooling practices, consisting of the sharing of information among individuals. Those alternatives diminish the cost each individual must incur to obtain a certain amount of information, decreasing overall ignorance in the market, even if never quite eliminating it. This happens because those specific agents (advertisement agencies, traders and so on) have particularly low costs to search for information. Due to those differential costs, there appears a sort of division of labor in the

72This development of institutions out of informational search was something to which Stigler apparently hinted at early on in his career, albeit surely in embryonic form. See Hammond (2020, 580).

search for information (Stigler, 1962, 104), with a few agents doing most of the search that benefits the market as a whole. Through this reasoning, Stigler is able to incorporate the existence of those apparently foreign elements in a market in which individuals process information optimally as a natural result of optimality itself.

Some degree of ignorance, therefore, according to this perspective, was also a maximized result: it simply did not pay to look for further information. Rather, if we took into account the costs incurred in the search for information, then any results could be rationalized as the best ones available given the costs to be incurred and the benefits to be obtained by the search procedure. If subsidies were in place, as was the case in Friedman's (1962c) analysis of education, they were what was optimal for individuals given the degree of ignorance left by the maximization of information search. And if government administration of education was in place, this was also optimal, given the same presuppositions. There is no such thing as avoidable ignorance: whatever effectively exists in the market is what is right, including its distribution of information[73].

What has hitherto been said could serve to explain why Stigler did not seek to directly intervene in reality as Friedman did. But it does not account for the other element that differentiates them: Stigler still sought to influence world events, and in particular economic policies, but he did it mediated by the scientific community of professional economists. Faced with this perennial optimization of information processing in the market, Stigler still found a place for science to inform policies in a meaningful way. Long before his work on information, he had realized that the scientific enterprise had something different from other kinds of knowledge. In other words, for Stigler the marketplace for scientific ideas had some unique characteristics relative to the marketplace of ideas in general. This difference allowed science to get closer to the truth. In the light of his information economics, this meant that its costs and benefits in the search for information were particularly favorable. Science can be conceived as a centralized information seeking agent which emerged out of the division of labor in information search.

Stigler first pointed to the particular status of scientific ideas in 1950. In a book chapter on the trends in employment and compensation in higher education in the United States, Stigler (1950a) argues that science is an environment specially suited to the emergence of true ideas. His discussion aims at differentiating the activities of teaching and of research that occupy college professors. In the

73Stiglitz (2000, 1443-1444) considers this positive view of the market's epistemic capacities held by Stigler to be something that distinguished his pioneer economics of information from the field's later development, notwithstanding the fact that two reviews of the field put Stigler's insight on the search for information as an important influence on information economics (Kamenica, 2017; Lippman and McCall, 2001). According to Stiglitz, however, the economics of information would largely conclude that the marketplace of ideas did not function appropriately when left to its own means. See also Hemel's (2021) considerations on the new economics of information.

course of doing so, Stigler remarks how teaching activities show little to no competition due to the difficulty in assessing quality, whereas research activities are highly competitive. This is true both in personal terms, through the journals, and in truth-seeking terms, with every scientist looking to arrive at the relevant knowledge ahead of his peers. Because of this greater competition that exists in the proper scientific enterprise, that of research, science manages to reach truth-attainment levels that no other type of knowledge-related pursuit can achieve.

This particular status of science makes it a privileged source to inform policy. We can see this in Stigler (1959) particularly for the case of economics, but we think this point may be generalized. In this 1959 paper on the effects of studying economics on one's political preferences, he argues economics makes one particularly prone to conservatism. This is due to the discipline's thorough study of the competitive market's functioning, which makes alternative proposals of economic organization appear to be naïve or excessively simplistic. Those alternative proposals usually bear some degree of abstractness in their elaboration, which causes them to overlook important features of modern economies' functioning. When compared to what has already been studied at length, the market economy, they lose credibility, and therefore the political support of those economists who have done such lengthy study. It is, thus, the skepticism toward what is unknown, which is a necessary feature not only of economics, but of the scientific enterprise as a whole, that renders science a good policy-informing tool in Stigler's view. If one doubts what is not known with great certainty, the chance of taking the wrong turn appears to be significantly diminished. This may be generalized for the sciences in general. Due to their profound skepticism, which demands detailed knowledge on anything that it may consider to be truthful in any meaningful sense, science is trustworthy information for policy-makers. Science will not seriously consider things that are overly abstract or that lack supporting empirical evidence, and therefore alternatives that have no real basis to be trusted are simply ignored.

Surely, not all science is equally well-suited as an information source for policy-making. For this particular information search agent to function effectively, its internal structure should mirror a free market, in which competition effectively reigns and commands the behavior of individual scientists. In Stigler's view, a first central component to this is that science must be insulated from current events and policies (Stigler, 1960). This insulation does not mean complete isolation: science must ultimately refer to the real world. But its agenda should not be dictated by anything external. It is a fundamental turn when a science abandons controversies over policies and applications as its main driver for the interests of professional scholars dedicated to its advancement as a corpus of empirically sound and theoretically systematized body of knowledge. A truly mature science, in which competition in the marketplace of ideas can effectively take place, has to be insulated from current events and

policies to effectively and efficiently search for information.

Whenever external influences are present, science's beneficial properties are undermined. The government is an external influence particularly important for Stigler. That he has no special desire to have government intervene in reality is clear from Stigler's work as a whole, and by his consideration that its influence in the realm of science is deep and growing (Stigler and Blank, 1957, 13) we can reasonably conclude that this was a danger of which he was aware. But as Nik-Khah (2017; 2020) has argued, through the 1960s Stigler also came to look with caution even the faculty and directing bodies of universities, public and private. He saw in their capitulation to popular demands for greater democratic control of higher education a curtailing of academic freedom and of the competition he deemed essential to it. According to Nik-Khah, this was what motivated him to search for alternative institutional forms of organizing scientific research, leading to his preference for privately-funded independent research institutions.

When in its mature, competitive form, the scientific enterprise looked a lot like the economic marketplace in Stigler's view. In his 'The intellectual and the marketplace' (1965), he explicitly compares science with the market. Departing from the fact that intellectuals generally dislike the marketplace, Stigler faces this statement with a bit of surprise. Given that the marketplace's agents are central in promoting freedom of inquiry among the intellectual class and in providing the latter with the material resources they demand to execute their activities, there should be some positive disposition from intellectuals to the marketplace. But even more important than that: the organizing principles of both enterprises are quite similar (1965, 70). As the marketplace, the academic world is a voluntary system, in which freedom of expression is the key for truth production. Despite failures in the form of authority (equivalent to monopoly), the marketplace of ideas that reigns in science is mostly and essentially competitive, and this is its main strength[74]. Furthermore, what the intellectual generally dislikes about the marketplace, namely the prevalence of self-interested agents, is not something completely strange to science: self-interest also plays an important role in academic environments and decisions, which does not undermine the effectiveness of the system as a whole - just like in the market[75].

In Stigler's conception, then, the privileged role of science in informing policy is a result of individual rationality. Through costly search procedures individuals maximize the benefits they get from information acquisition, so that every result that arises in a market is informationally-efficient, even if some degree of ignorance is maintained. Furthermore, because there exist differential costs in

74The competitiveness of science even gives rise to the need of advertising one's ideas, as he suggests in Stigler (1955).

75When comparing science with a market explicitly, Stigler seems to be, along with people like Polanyi (1962), anticipating a tendency that would arise in science studies in the 1990s. On this movement, see Hands (1997), Hull (1997), and Mäki (1999; 2005).

the information search procedure, market agents with the specific function of searching for information appear. Due to this origin out of cost differentials, the latter are a natural result of individuals' self-interested pursuit for information. Among those special market agents is the scientific community, whenever it is competitively organized, without government or generally democratic influences. In that situation, science serves as a tool to facilitate the societal search for information and therefore ends up, as a special market agent born out of individuals' self-interest, legitimately and efficiently influencing policy-making.

5. Concluding remarks

When thinking about and acting as professional economists, Friedman and Stigler sustained different positions on what should be their proper policy role. Whereas Friedman saw direct intervention as an effective means of changing economic policies, Stigler saw them as subject to effective and legitimate change only indirectly, mediated by the community of scientific economists. Although there are certainly lots of personal, psychological and contingent factors in explaining why they differed in that realm, we have argued throughout this text that how they conceived the marketplace of ideas may also be an important element in making sense of it. The marketplace of ideas, as we have said, is an economist's way of thinking about the nature and functioning of knowledge in society in general, and it may also be applied particularly to the scientific realm, so its relationship to how one would seek to inform policy-making is fairly direct.

A preoccupation with how the marketplace of ideas worked got to Friedman through the Cowles Commission economists. In the late 1940s and early 1950s, he sought to undermine their proposals for economic planning in many realms, including the epistemic. Regarding the latter, Friedman argued that although individuals maximize utility or profits in their decisions, we could not know how they did it, nor could they. What happened was that individuals did not necessarily calculate maxima, but they were on average compelled to maximize due to the operation of market forces. Individually, they could be mistaken, even on a regular basis. Because he made this concession to the potential nonrationality of individuals in the market, Friedman was led to conceive with greater ease of market failures once he did not have to hold his ground against the Cowles market socialists. Due to those market failures, there was room for someone well-informed such as himself to intervene in public debate to ameliorate the situation.

Stigler, on the other hand, did not dispense with individual rationality. He did not face the Cowles economists directly, but in other intellectual queries around the same time he showed remarkable inflexibility in the face of criticism when it came to bear on individual economic agents' rationality. This inflexibility led Stigler to, instead of dismissing or undermining individual rationality,

extend it further. Thus his economics of information, which, extending individual maximizing rationality to information search procedures, made all market results optimal as long as information search was adequately accounted for. From individually rational informational search, he could derive the necessity of a centralized information search agent, which we have understood as legitimizing the role of science in policy-information. This means, in terms of the marketplace of ideas, that the marketplace for scientific ideas had a distinguished position within the more general marketplace of ideas, so it was a particularly well-suited locus to provide social and economic change. Thus his preferred route to influence policy-making.

Concluding remarks

In the course of this Dissertation, we have explored the divergence of Milton Friedman and George Stigler on the proper policy role for the economist. To do so, we had first to familiarize ourselves with the environment within which they were producing their economic theory: the Chicago School of Economics. Born in the postwar period under the light of the transformations in American economics and in the global political environment, notably by its coupling to the then-emerging neoliberal political movement, the Chicago School constituted a locus of intellectual production more or less delimited, but sufficiently open to dissent so that two of its leadings members could disagree on important issues. There, Friedman and Stigler could sustain the disagreement they did.

We then proceeded to identify their divergence as it unfolded in history. Looking at what they thought about it and how they acted as professional academic economists from their early careers into the 1960s, when their divergence culminated in strikingly different behaviors in relation to the formulation of policies, we propose to have identified its core element. It consists in that Friedman thought it efficient and legitimate to influence policies directly, telling the people and the policy-makers what they should be doing, whereas Stigler also wanted to influence policies, but thought that to do so efficiently and legitimately he would have to act mediated by the community of scientific economists. They were seeking similar ends, but the ways through which they attempted to do so were different. Although this may not be the only determinant of their actual behaviors as economists trying to influence policy, as personal, psychological, and many other contingent factors were most likely also at play, how they intellectually conceived the proper policy role for the economist is at least consistent with, and potentially a cause of, their actual behaviors.

After having traced the historical development of their divergence and identified its central feature, we attempted to find the roots of it through the concept of the marketplace of ideas. The latter is still quite underdeveloped as a concept, and beyond its usefulness in explaining Friedman and Stigler's divergence, we hope our effort also managed to somehow operationalize it a little further, facilitating other applications. This underdeveloped state, however, certainly made the task of using it a bit more difficult than the previous, strictly historical task of identifying their divergence and its core element. Therefore, Chapter 3,

where this discussion is made, is a bit more exploratory than the previous two chapters. However tentative, we have reached the conclusion that the Friedman and Stigler's divergence may be explained through the marketplace of ideas in that (i) Friedman conceived the marketplace of ideas in general, as we have called it, as liable to failures due to his neglect of the assumption of individual rationality, as opposed to his emphasis on the predictions generated by economic theory, and that (ii) Stigler, beyond accepting individual rationality fully, which already differed his marketplace of ideas from Friedman's, also conceived of the marketplace for scientific ideas as a part of the marketplace of ideas with its own distinctive characteristics. This led him to believe that direct intervention in economic policy was not legitimate nor efficient, as Friedman thought it was.

Furthermore, we believe the exploratory character of Chapter 3 allows it to contain important insights that may be further developed outside of this Dissertation, with potential to become contributions to the history of economic thought. There are, in principle, two paths that we think might be productive: (i) first, the marketplace of ideas can constitute a general analytical framework to relate economists' epistemology with their influence as professional economists in policies. Having this general analytical framework in hand in a more fully developed state, we may in principle conduct comparative studies between any economists; and (ii) using the marketplace of ideas concept allows us to discuss the different uses of the notions of rationality/optimization/efficiency/maximization, in their epistemic dimension, within the Chicago School, which is apparently presumed to have a single stance towards those concepts. That a single stance prevails we have shown that is not the case, even when little to no differentiation between those four concepts is done. Could we develop the discussion in that direction, introducing different definitions to those concepts, presumably the potential for heterogeneity would be increased, and more positions would be identifiable within the Chicago School.

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