

# 50° Encontro Nacional de Economia (Fortaleza/CE - 06 a 09/12/2022)

## Milton Friedman, George Stigler, and a proper policy role for the economist

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### Abstract

The Chicago School of Economics contributed importantly to the self-conception of economists' proper role in policy issues. Nonetheless, in the mid-1960s, the two most prominent Chicago School economists, Milton Friedman and George Stigler, notably disagreed over that role. Their disagreement appeared more explicitly in that Friedman intervened heavily in public debate whereas Stigler mostly restrained himself to academia. In this paper, we argue that their disagreement existed before the mid-1960s, being identifiable since the mid-1940s. We argue through an analysis of the two economists' academic writings from 1946-1960. Our conclusion is that their fundamental difference, which culminates in their explicitly recognized divergence in the mid-1960s, is that Friedman saw direct intervention as an effective means of changing economic policies, whereas Stigler saw them as subject to effective and legitimate change only indirectly, mediated by the community of scientific economists.

**Key-words:** Chicago School; Milton Friedman; George Stigler; Economics and policy.

### Resumo

A Escola de Chicago de Economia fez importantes contribuições para a autoconcepção dos economistas sobre seu papel apropriado em questões de política. Apesar disso, em meados dos 1960s os dois mais proeminentes economistas de Chicago, Milton Friedman e George Stigler, discordavam a respeito deste tema. Sua divergência se tornou explícita pelo fato de Friedman intervir pesadamente em debates públicos enquanto Stigler se restringia à academia. Neste artigo, argumentamos que sua discordância existia antes de meados de 1960, sendo identificável já em meados da década de 1940. Argumentamos através da análise de seus textos acadêmicos entre 1946 e 1960. Nossa conclusão é que sua diferença fundamental, que culmina na divergência explícita que se identifica em meados dos 1960s, é que Friedman entendia que intervenção direta era uma maneira efetiva de transformar as políticas econômicas adotadas, enquanto Stigler as via como estando sujeitas a transformações legítimas e efetivas apenas indiretamente, através da comunidade de economistas acadêmicos.

**Palavras-chave:** Escola de Chicago; Milton Friedman; George Stigler; Economia acadêmica e política econômica.

**Área ANPEC:** Área 1 - História do Pensamento Econômico e Metodologia.

**JEL Codes:** B20 - History of Economic Thought since 1925 - General; B31 - History of Economic Thought: Individuals - Individuals.

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# Milton Friedman, George Stigler, and a proper policy role for the economist

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## 1. Introduction

The Chicago School is recognized as one of the most important schools of economic thought in the postwar United States. Among its various contributions to the discipline, there is a particular 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. In their conceptions of the economists' proper policy role, Milton Friedman and George Stigler, the two most prominent Chicago economists, diverged.

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. There, Stigler criticized Friedman's proneness to public appearance and policy advising to governments. Through an analysis of some of Stigler's writings and participation in Mont Pèlerin Society's (MPS) meetings, Nik-Khah claims that Stigler developed a critical view of interventions in public debate and policy advising - which constituted the core of his divergence with Friedman - in the 1960s.

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. 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.

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 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 since the mid 1940s. To do so, we analyze the two economists' academic writings (understood as writings aimed for academic audiences) in the period 1946-1960, using them as sources on both their own actions as academic economists and their normative representations of the economist's proper policy role. The divergence they sustained throughout this period culminates in the more explicit divergence Reder, Peck and Nik-Khah have noted in the 1960s and beyond.

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 concerned with doing or informing empirical work, it is classified as empirically-oriented, and it is classified as theoretically-oriented otherwise. 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

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economic policy, it is classified as policy-oriented, and it is classified as ‘pure-knowledge’-oriented otherwise.

The paper has three sections, apart from this introduction and some concluding remarks. Section 2 deals with the years between 1946 and 1955, when Friedman and Stigler dealt with the proper role for the economist in academia, laying out the foundations upon which their conceptions of the economists’ proper policy role could be built, and Section 3 deals with the period between 1956 and 1960, when the latter conception is effectively developed. In both sections, in addition to an analysis of their academic writings’ content, we classify them as either empirically- or theoretically-oriented, and as either policy- or ‘pure knowledge’-oriented. The first distinction pertains to the role of empirical evidence, methods and reasoning in economics - if the text is 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.

## **2. Laying out the foundations, 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, 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<sup>3</sup>. 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.

3 Friedman, for example, states in his memoirs that his involvement with the MPS “strengthened my incipient interest in political philosophy and public policy” (Friedman and Friedman, 1998, 158) and “marked the beginning of my active involvement in the political process” (Friedman and Friedman, 1998, 159). Characterizing the MPS as the beginning of his political involvement might not be exactly precise, as Nelson (2020, v.1, chap.3) argues that Friedman’s work in US Treasury’s Division of Tax Research in the early 1940s, during which he spoke before Congress in policy-relevant matters of fiscal policy, was his actual first political involvement. Nonetheless, the MPS was certainly an important event in that realm, even if not the first one.

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 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 over 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 in the period. 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.

**Table 1 - Friedman’s and Stigler’s academic writings classified according to policy and empirical orientations (1946-1955)**

<b>FRIEDMAN (1946-1955)</b>		
	<b>EMPIRICAL</b>	<b>THEORETICAL</b>
<b>POLICY-ORIENTED</b>		1947a; 1948; 1951; 1952a; 1952b; 1953c; 1953d; 1954 <sup>4</sup>
<b>‘PURE KNOWLEDGE’-ORIENTED</b>	1947b; 1947c;	1946; 1948 (w/ Savage); 1948 (w/ Hotelling et al.); 1949; 1950; 1952 (w/ Savage); 1953a; 1953e; 1955
<b>STIGLER (1946-1955)</b>		
	<b>EMPIRICAL</b>	<b>THEORETICAL</b>
<b>POLICY-ORIENTED</b>	1949c	1946; 1950a; 1955a
<b>‘PURE KNOWLEDGE’-ORIENTED</b>	1949b	1947a; 1947b; 1947c; 1949a; 1949d; 1949e; 1949f; 1950b; 1950c; 1951; 1952; 1953; 1954a; 1954b; 1955b

<sup>4</sup> Reprinted as Friedman (1968).

### 3.1. Friedman's methodological strategy<sup>5</sup>

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 idea 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-Walrasian 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, at Friedman's time, already exhausted in their contributions. The current need of economics at 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 unnecessary and even the undesirability<sup>6</sup> of realistic<sup>7</sup> 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 do 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

5 The debate over Friedman's methodology is extremely vast, and we cannot intend to examine it here in the necessary detail to make definitive assertions on its interpretation. For further reading, we recommend Mäki (2009), Backhouse (1995) and Hirsch and De Marchi (1990), as well as the references they mention.

6 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.

7 The meaning of Friedman's use of the term 'unrealistic' is discussed by Hirsch and De Marchi (1990). They argue that Friedman's 'unrealisticness' does not mean falsehood, but rather an assumption that is at first sight implausible, in the sense that knowledge attained by introspection would have led to a different conclusion. The scientific value of such an assumption, for Friedman, should not be its coherence with introspective truths, but support by empirical evidence of the predictions it can generate. They argue for that interpretation, locating Friedman methodologically as a critic of Mill's introspective methodology.

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:

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 (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, and for that purpose accurate predictions were enough<sup>8</sup>.

### *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 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 over 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

<sup>8</sup> Hirsch and De Marchi (1990, 99-100) can be understood as disagreeing with this view. Albeit conceding that there are certain elements in Friedman's 1953 methodological essay which make it plausible to argue he was merely trying to remove barriers to advance his own purposes, reading his essay in this light makes it "easy to lose sight of the fact that he has something very interesting and original to say in 'Positive economics' about economic methodology" (100). Even if Hirsch and De Marchi present a convincing interpretation of Friedman's methodology, we still think it is plausible to understand the latter as at least partially concerned with opening up the way for the pursuit of his other purposes.

quite remarkable historical work. Rather, they contained a picture of history with a preconceived direction orienting the analysis<sup>9</sup>, so that the historical reasoning and documentation served the purpose of justifying a desirable path of economics toward its mature form<sup>10</sup>. 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<sup>11</sup>.

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, such as 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<sup>12</sup>.

Those theoretical advances are put by Stigler in a timeline of scientific progress in economics that 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<sup>13</sup>. 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,

9 Freedman (2007), evaluating Stigler's work on the history of economics, argues, in line with his arguments on Stigler's work in other fields, that he approached his object with preconceptions that oriented his analyses - something that goes against what Stigler himself, according to Freedman, found to be the main benefit of studying the history of economic thought. Stigler's work on the history of economics in this 1946-55 period seems to us to be certainly favorable evidence to Freedman's thesis in this 2007 paper, for which he argued based mostly on Stigler's work on Adam Smith.

10 The normative character of Stigler's investigations in the history of economic thought to the science of economics itself has been noted by Rosen (1993), who argues Stigler's interest in the history of economic thought was motivated, among other reasons, by his desire to determine the best ways to organize science. This desire can be understood as having also a positive (in Friedman's sense) dimension apart from its normative one: the discovery of the laws governing science, which Rosenberg (1993) claims to be the spirit of Stigler's history of economics. This preoccupation with the laws of functioning that make economics progress is also identified as a distinguishing characteristic of Stigler's work in the history of thought by Diamond Jr. (2005).

11 Those two elements have been identified also by Rosenberg (1993, 836, 846) as hallmarks of Stigler's conception of science. Another element that Rosenberg highlights is detachment of scientific inquiry from day-to-day concerns, which appears more clearly in Stigler's academic writings post-1955.

12 For more on Chicago economics and (the absence of) microfundamentals to demand curves, see Hands and Mirowski (1998), Mirowski and Hands (1998) and Mirowski (1999). For a criticism of the Mirowski-Hands thesis on the Chicago School, but that does not seem to affect the microfundamentals to demand curves issue, see Hammond (2006).

13 Stigler (1953) points to three important developments in economics in the nineteenth-century: greater specialization; less political influence; and closer attention to reality. The first can be interpreted as the systematicity Ricardo pioneered, and the latter with Stigler's valued empirical concerns. The second aspect receives more appropriate examination by Stigler in texts post-1955.

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 Gailbraith'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 Gailbraith'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<sup>14</sup>.

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<sup>15</sup>. 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<sup>16</sup>.

14 This normalization or maturation of the neoclassical paradigm as operated and observed by Chicago economists, in particular Stigler, is noted by Schliesser (2012). He argues that between the late 1940s and early 1950s a methodology that owed much to the NBER institutionalist tradition was characteristic of Chicago, and institutionalists were honorable intellectual adversaries. From the mid-1950s onward, however, a new narrative of that period began to form, chiefly by Stigler's efforts, and it told the story that economics was not yet a mature science at that time. It had come to maturity only in the late 1950s, when institutionalism was no longer considered respectable science and neoclassicism came to dominate academia, and therefore Chicago had its institutional roots erased from the official narrative of its origins.

15 Freedman (1995) argues this was part of Stigler's academic debating strategy: he incorporated his intellectual opponents' propositions in his own theoretical language so that he could metaphorically unarm them. Demsetz (1993) also argues that the extension of the neoclassical framework instead of the adoption of alternative frameworks was a pervasive feature of Stigler's work on industrial organization, his main field of study.

16 Keppler (1998) has a different, but most likely complementary explanation for this preference to extend neoclassical theory instead of adopting alternative frameworks. He argues, investigating the denial of the relevance of imperfect

As the above shows, in this period Friedman and Stigler conceptions on the economists' proper role in general, and specifically in policy issues, had some common points, 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: 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 acting on the scientific community consensus.

### **3. Dealing with the political element, 1956-1960**

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, 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 which social scientists were intellectually constrained by political motives<sup>17</sup>, 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, 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 section. 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 2), 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

competition theories by both Stigler and Friedman, that they were motivated by "the ambition to develop a practice and reputation for exactness in economics akin to the natural sciences" (Keppler, 1998, 273), and to do that there could not exist alternative paradigms of research: neoclassicism had to prevail undisputed.

<sup>17</sup> To a recent discussion on the impact of McCarthyism in American economics see Weintraub (2017). See also Lee (2004a; 2004b).

decade. In Stigler’s work, the same index 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.

**Table 2 - Friedman’s and Stigler’s academic writings classified according to policy and empirical orientations (1956-1960)**

<b>FRIEDMAN (1956-1960)</b>		
	<b>EMPIRICAL</b>	<b>THEORETICAL</b>
<b>POLICY-ORIENTED</b>	1958a	1957a; 1958b
<b>‘PURE KNOWLEDGE’-ORIENTED</b>	1957b; 1957 (w/ Becker); 1959	1956; 1960
<b>STIGLER (1956-1960)</b>		
	<b>EMPIRICAL</b>	<b>THEORETICAL</b>
<b>POLICY-ORIENTED</b>		1958b
<b>‘PURE KNOWLEDGE’-ORIENTED</b>	1956b; 1958a	1956a; 1957; 1958c; 1959a; 1959b; 1960

*3.1. 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 current or an influential policy

position in the 1950s<sup>18</sup>, 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 1956-1960 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. Friedman published five papers (one in coauthorship) and three relevant book chapters in the period, all discussing matters related to macroeconomics. In them, we find two features of interest. The first is Friedman's dual strategy to shift the macroeconomic debate in favor of his positions, 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. 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.

Friedman, in a few of his texts from this period, 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 those whole 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<sup>19</sup> 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.

18 As Nelson (2020, v.1, 1761-177) remarks, Friedman went from winning the prestigious John Bates Clark Medal in 1951, awarded to an economist under age forty for an outstanding contribution to economic research, to being considered a *persona non grata*. He would later climb again to the heights of the profession, but in the 1950s he would remain mostly marginalized in Nelson's account. Although he has a convincing point, this marginalization should be qualified by Lee's (2004b) considerations on American heterodox economics around the same period. Friedman's margins were far closer to the mainstream than the heterodoxy's margins.

19 It is casual in the context of this text. This evidence is most likely part of his monetary history project at the NBER and of the work done under the Chicago workshop for money and banking, as is probably the case of all of the evidence on monetary history he uses in this 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 UK statistical data. Friedman criticizes many points in Fisher's analysis and does so in an extremely qualified manner, discussing theoretical (mostly 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<sup>20</sup>, 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.

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 his 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" (254), 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.

<sup>20</sup> In Friedman's (1959) concluding remarks, he says: "The results summarized in this paper have implications for the theory of money, the study of business cycles, and the conduct and possibilities of monetary policy" (348). He is aware of policy implications even when the form and content of his writings is properly academic.

### *3.2. Stigler and the conservation of academic and political life*

The period from 1956 to 1960 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 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 monopoly as a theoretical and empirical issue and a paper on the history of economic thought, 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 some sense a part of both movements, there is his work on the Fabian socialists (Stigler, 1959a).

In both papers in which Stigler deals directly with political matters in the period 1956-1960 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. It 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. 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<sup>21</sup> 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.

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

21 This separation that Stigler identifies as a characteristic of economics seems to be reflected in his reading of the erroneous interpretations of Ricardo's theory of value as a labor theory of value, the topic of his one paper on the history of economic thought between 1955 and 1960 (Stigler, 1958c). Stigler identified the error of those who interpreted Ricardo as a labor-value theorist to be the absence of a clear distinction between analytical and empirical assertions. When both types of assertions are clearly demarcated, one can see more easily that just because a relationship is defended as an analytical assertion - which, in Ricardo's case, was the cost of production theory of value - it does not mean that it will be defended as empirically relevant - empirically, Ricardo understood labor to be the main determinant of value, even if analytically it was not the only one. Therefore, the clear demarcation between empirical and theoretical enterprises in economics is defended due to its impact on the relative isolation of economics from current events but also as an important distinction to interpret the correct meaning of scientific knowledge, depending on whether it is analytical or empirical.

two occasions, in 1950 and in 1960, and would appear one more time later on in his career<sup>22</sup>. 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 1956-1960 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 1956-1960, 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), refines the concept of competition (Stigler, 1957), and finally gathers and analyzes empirical evidence in a systematic way (Stigler, 1958a). 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 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. Stigler, on the other hand, did not seek to intervene as directly in society, not even through his academic writings. The way he coped with the ascent of the political movement was by studying it as an academic subject, particularly as it related to the discipline of economics and its practitioners. While at it, 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.

#### **4. Concluding remarks**

Milton Friedman and George Stigler were two of the most important economists in the postwar United States. They made important contributions to economic thought, but more than that, they contributed to the economists' self-understanding of their proper role in policy issues. When doing so, as first noted by Reder (1982) and more recently reaffirmed by Peck (2011) and Nik-Khah (2020), they did not converge completely. Their disagreement appeared more explicitly in the mid-1960s in that Friedman intervened heavily in public debate whereas Stigler restrained himself to academia. In this paper, we built on Reder's (1982, 25) comment that this disagreement had begun in their mid-1940s intellectual divergence to explore the roots of their differences in this realm.

We have shown how their divergence appeared first in their conception of the economists' proper role in general, in the first postwar decade, and then in their specific conceptions of the economists' proper policy role, in the latter half of the 1950s. In the first postwar decade, their differences were mainly strategic, as Friedman argued methodologically and Stigler argued through historical narrative building. This strategic difference, nonetheless, contained a fundamental divergence between them: Friedman thought it was possible and desirable to influence real world events directly, whereas Stigler thought economists could only achieve

<sup>22</sup> Leube (1986), in a short biography, says Stigler "served on several public committees" (xvii), but mentions only two, one in 1960 and the other in 1970. Two does not seem like several to us, neither does it to Stigler, at least if we trust his own recollections in his memoirs.

such a goal indirectly, mediated by the scientific community. In the latter half of the 1950s, then, when the political element gained relevance as 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, which he was actively participating in, would be sufficient to achieve his desired policy outcomes.

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