Essays on the Mathematization of Economics

Thiago Dumont Oliveira

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Supervisor:

Carlo Zappia

Examination Board:

Nicola Giocoli

Harro Maas

Ivan Moscati

Universities of Florence, Pisa, and Siena Joint PhD Program in Economics
Program Coordinator: Ugo Pagano
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Contents

Introduction

The mathematization of economics is a topic spanning many decades, authors, and external factors. Reconstructing the changing nature of economics during the past hundred years would be a daunting task even for scholars who have long dealt with the topic, and it would be impossible in a PhD thesis. What I have done instead was to focus on some aspects that, albeit crucial, seemed to be underdeveloped in the literature. My goal was to combine ideas from philosophy, sociology of science, history, and data science to assess how the language used by economists changed throughout time, to discuss some of the drivers of this process, and to shed some light on the methodological shift that happened in economics in the postwar period.

The thesis takes as its starting point the alleged “founder” of “scientific” economics, Lionel Robbins, and investigates how economics has evolved in time, detaching itself ever more from the richer texture of Robbins’ view which was more akin to classical political economy. I argue that the top journals of economics were key players in the significant epistemological changes the discipline underwent after the 1940s, first with the increasing mathematical formalization, and later with the rise of empiricism in an attempt to mimic the natural sciences. If the 1940s and 1950s marked the rise of formalization in economics, there has been a more recent shift from the Cowles econometric approach to quasi-experimental methods, which have grown considerably in importance since 1990. This shift originated with the disarray in which the simultaneous equations approach fell during the 1980s, with its contested reliance on economic theories based on the optimization of agents or firms. If economics has certainly been relying less on theoretical models and becoming more data-driven in recent decades, hence more applicable to policy matters, this does not necessarily mean that political economy is resurfacing. Economics may be turning into a more useful instrument for designing public policies, but it remains to be seen whether the social, political and institutional elements that were extruded in the transition from political economy to economics will have any important role to play in the new era of data-driven economics.

Chapter one argues that parallel to the mathematization of economics there was a redefinition of the methodology of economics. In order to do so I assess the influence of British classical economists on Lionel Robbins in what concerns the demarcation between the epistemology of the natural and the social sciences. With the mathematization of economics this distinction was displaced and economists increasingly started to mimic the methods of natural sciences, leading to the dehistorization and desocialization of economics. Lionel Robbins provides an interesting window into investigations of the changing nature of economics not only because he was concerned about the methodology of economics, but also because he wrote in a period when economics was quickly becoming more quantitative. In this sense the lingering influence of British classical economists on Lionel Robbins shows that the mathematization of economics after the 1930s happened in tandem with an epistemological change whereby economists increasingly endorsed the methods of the natural sciences as the proper way to approach their subject matter.

In recent years the role of top journals of economics in molding research has been widely debated and the chapters 2 and 3 of the thesis contribute to this debate by examining how the language used by economists evolved since the foundation of *Econometrica* in 1933 until recent years.

The second chapter focuses on the consolidation of mathematical economics between the 1930s and the 1960s. I argue that one of the drivers of the mathematization of economics was the ideological context of the period. As a response to totalitarian tendencies taking place in Europe, quantitative methods grew in importance because they were perceived as being value-free. Using latent semantic analysis I show that there was a convergence in the language used by the top journals of economics and in this sense the origins of the so-called top five journals can be traced back to the 1960s when they endorsed the methods championed by the Econometric Society.

Chapter three then investigates the role of the *American Economic Review*, the *Journal of Political Economy*, and the *Quarterly Journal of Economics* in the development of mathematical economics. Using bibliometrics it is shown that the papers published in these journals are strikingly similar between 1940 and 2010. In this sense I argue that mathematical economics was increasingly incorporated by the top general journals and the rest of the profession followed suit, not least because the creation of citation metrics in the 1960s created a hierarchy within the network of journals and most journals followed the developments taking place in the main journals in an attempt to increase their number of citations. Finally, the chapter investigates how economics has changed between 1990 and 2017 using co-word analysis and it is shown that economics has become less theoretical and more data-driven in recent years.

Therefore, while chapter 2 shows that the top journals have converged to paradigmatic core in the 1960s, chapter 3 argues that the top journals assumed a hegemonic position due to their centrality in the network of journals. These journals played a key role in the mathematization of economics since they were increasingly able to shape economic discourse after the creation of citation metrics in the 1960s. Hence, my claim is that as a consequence of the convergence in the language used by the top journals in the 1960s, they were able to increase their number of citations to such an extent that they became gate-keepers of knowledge, and the rise of the quantitative methods in these outlets ultimately spread to whole network of economics journals.

Chapter 1

Of Time, Uncertainty, and Policy-Making: The Classical Origins of Lionel Robbins' Epistemology of Political Economy

Co-authored with Carlos Eduardo Suprinyak (Federal University of Minas Gerais - UFMG)

The chapter argues that Lionel Robbins inherited from the British classical economists an understanding of the relationship between economics and political economy where uncertainty played a central role both at the theoretical and at the epistemological levels. From a philosophical perspective, he followed the classical economists' demarcation between the epistemology of the natural and the social sciences. Robbins saw uncertainty and the time element as fundamental to economics (as a science) if it were to achieve its goal of being a useful tool for political economy. His conception of economic science was thus tailored to his interests in political economy, rejecting attempts to mimic the methods of the natural sciences by preserving the human element that makes economics a social science.

1.1 Introduction

It is tempting to enquire into the fate of political economy by stepping into the shoes of Lionel Robbins. Both as a scholar and as an adviser, Robbins dealt widely with issues he himself regarded as pertaining to the domain of political economy but, ironically, has been long and repeatedly criticized for excluding political affairs from the concerns of the economist. A closer examination of his works raises important questions not only regarding Robbins' understanding of the relationship between economics and political economy, but also the epistemological assumptions underlying this relationship. By clarifying Robbins' conception of economic science, and the hopes he entertained regarding the future evolution of the discipline, we hope to highlight the extent to which economics has since considerably diverged from his epistemological tenets, and hence from the image he advocated for political economy.

Even though Robbins' indebtedness to the British classical economists has been explored in methodological surveys such as Blaug (1992 [1980]), Caldwell (1994), and Hands (2001), we wish to show how his vision of political economy was profoundly influenced by his understanding of the classical tradition, especially as elaborated in the works of John Stuart Mill. This involved more than simply adhering to Millian 'verificationism' as a methodological proposition. Robbins saw British classical political economy as a policy-oriented form of knowledge — a point he forcefully stressed in his incursions, later in life, into the history of economics. We thus add to the literature by claiming that Robbins' epistemology of political economy was consistent with his (in)famous definition of economic science: if the goal of economics was to serve as a tool to orient public policy, the means had to be suited to this end. Political economy logically preceded economics narrowly defined and Robbins set himself the task of defining the nature of economics (as a science) in accordance with its significance (as political economy). Since economics was a social science, however, policy-relevance required consideration of an essential aspect of human decision-making: uncertainty. This offered Robbins some fertile ground to assimilate his interests in Austrian economics and other interwar theories of cyclical fluctuations into the dynamic framework developed by the British classical economists. Uncertainty thus played the double role of orienting Robbins' theoretical elaborations and shaping his epistemological commitment to political economy as a source of useful knowledge about human society.

1.2 The Economist Quae Political Economist

Robbins entered the LSE in 1920, specializing in the history of political ideas under the supervision of Harold Laski (Howson 2013, 115). During his undergraduate years, however, he came increasingly to like economics, growing "sick of [the history of political ideas] because after a point it seemed so futile to go on studying it", while economics seemed "more fruitful in practical results and capable of yielding greater intellectual satisfaction".[1] Since then, he began to write extensively on political economy: from one of his earliest writings in 1927 until his address to the American Economic Association in 1981, Robbins advocated that economics was an important tool to orient public policies.[2] In this, he was simply following the distinction established by the British classical economists between political economy as an art and as a science, which "had become generally recognized" by the middle of the nineteenth century (Robbins 1963, 6). Robbins used the expression 'political economy' to refer to the art, while reserving 'economics' to designate the science. Apart from the different terminology, however, he closely followed the distinction suggested by Mill and his followers. From the likes of Senior, Mill, Cairnes, and Neville Keynes, Robbins absorbed the view that empirical 'verification' was not an instrument for testing the validity of the hypotheses advanced by the science of political economy, but rather a way of establishing the domain of application appropriate for the art of political economy (Blaug 1992 [1980], 51-82; Hands 2001, 14-37).

"In the realm of Applied Economics", Robbins argued in the *Essay*, "theory cannot be fruitfully applied to the interpretation of concrete situations unless it is informed continually of the changing background of the facts of particular industries" (Robbins 1935, 42). Later on, he stated that "before we apply our general theory to the interpretation of a particular situation we must be sure of the facts" (ibid., 81), thus strongly echoing the words of John Stuart Mill: "When the principles of Political Economy are to be applied to a particular case, then it is necessary to take into account all the individual circumstances of that case" (Mill 2008 [1844], 49). The parallel was not an isolated event. In the preface to the second edition of the *Essay*, Robbins characterized "an economist who is only an economist" as a "pretty poor fish", adding that "an education which consists of Economics alone is a very imperfect education" (Robbins 1935, viii-ix). This closely resembles Mill's pronouncement that "the mere political economist, he who has studied no science but Political Economy, if he attempts to apply his science to practice, will fail" (Mill 2008 [1844], 50).

In his later historical survey *The Theory of Economic Policy in English Classical Political*

[1] Robbins to Iris Gardiner, 24 June 1924 (cited by Howson 2004, 417)

[2] For a more detailed account of Robbins' involvement as a political economist, see Howson and Winch (1977), O'Brien (1988), Wright (1989), Howson and Moggridge (1990), Howson (2004, 2011), Masini (2009), Scarantino (2009), Oliveira and Suprinyak (2016).

Economy (1978), Robbins stressed how utilitarianism provided the classical economists with a bridge between positive and normative considerations that was indispensable for the success of their intellectual project:

> For you cannot build prescriptions on a mere knowledge of positive facts, however systematized and comprehensive. You need a goal as well — a general objective, a criterion of the expected results of action. It is all very well to know how the world works, why certain relations emerge in certain conditions, how these relations change when conditions are altered. But unless you have some test whereby you can distinguish good from bad, desirable consequences from undesirable, you are without an essential constituent of a theory of policy. [...] A theory of economic policy, in the sense of a body of precepts for action, must take its ultimate criterion from outside economics. This criterion the English Classical Economists found in the principle of utility, the principle that the test of policy is to be its effect on human happiness (Robbins 1978, 176-177).

Even though the British classical economists were not a homogeneous group, Robbins argued, they "shared a common interest in economic reform" and believed "that the application of certain methods of approach and analysis, the recently discovered science of Political Economy, offered superior hopes for what they would have called improvement" (ibid., 4). He thus challenged the notion that the members of this school were "indefatigable opponents of social reform" who "conceive no function for the state other than that of the night watchman" (ibid., 5). To understand their views on economic policy, one must bear in mind that the "system of economic freedom" they espoused was embedded in "a certain framework of law and order and certain governmental services" (ibid., 11). As Robbins explained,

> you get an entirely distorted view of the significance of this doctrine unless you see it in combination with the theory of law and the functions of government which its authors also propounded; the idea of freedom in vacuo was entirely alien to their conceptions (ibid., 12).

The struggle to clarify the nature of the relationship between the positive propositions of economics and their use at the service of certain social goals provided a subtext for most of Robbins' career. In one of his earliest papers from 1927, one already finds many of the arguments that would reappear in the *Essay*, and then be reiterated roughly fifty years later in his Richard T. Ely Lecture. "In the past", he said, "Economists have generally agreed that ethical criticism was not part of their business as economists" (Robbins 1927, 174). If economists had any hopes of producing useful knowledge to guide the formulation of public policies, and bring economics closer to the status of a science, they should exclude ethical and normative considerations from its scope. Robbins then ended the paper with the argument he would later painstakingly try to develop in the *Essay*, only to find himself subject to relentless criticism for decades to come:

> All that I am pleading for here is that we should preserve that separation of *science* from what at best must remain pure *opinion*, which has emerged so hardly from the irrationality of the pre-scientific era. By all means let us be willing to spill our opinions on the public. By all means let us try to make *our* categorical imperative *the* categorical imperative. But for the repute of that little area of knowledge which we can fence off from the wilderness of velleity and dogma, do not let us pretend to be talking economic science. Some day perhaps we may persuade the world that we understand those phenomena we call economic. Let us beware lest we jeopardise even this title to respect by claiming the same sanctions for judgments of value (ibid., 178, emphasis in the original).

In 1930, Robbins participated in a committee sponsored by the Economic Advisory Council, with Keynes as the chairman, to discuss the causes of the current economic crisis in England and propose remedies to accelerate recovery. The episode is enlightening for showcasing Robbins' direct involvement in policy discussion, something often repeated throughout his life. In this capacity, Robbins showed no qualms about resorting to knowledge that lay outside the realm of economics: he routinely combined economic and non-economic arguments to support his positions, and was quite explicit about this. Disagreeing with Keynes both on economic and broader political grounds, especially regarding the usefulness of tariffs and public works as tools for recovery, Robbins refrained from signing the committee's draft. His objections were a product of his theoretical background, on one hand, and of a "profound scepticism in the whole philosophy of economic nationalism", on the other.[3] About the latter, he then explained:

> I do not pretend, however, that this last objection is purely 'economic' in character.

[3]His economic arguments were further developed in Robbins (1934), while his political objections would resurface in Robbins (1937, 1939a, 1939b).

> If you want that kind of world then I suppose the economist, *quae* economist, has nothing to say about it. But I confess that I find it surprising that twelve years after the world war, rational beings should find the prospect of a series of right little, tight little national economies, busily engaged in reducing the volume of international exchange to a minimum, the sort of world they are willing to accept without a fight.[4]

The excerpt above — "the economist *quae* economist, has nothing to say about it" — points clearly to the demarcation between positive and normative economics later to be developed in the *Essay*. Economics (as a science) can only predict the effects of a policy; it cannot suggest whether or not it should be adopted, for this is a matter that lies in the realm of political economy. Apart from his economic objections to the use of tariffs, the political reasons he offered for rejecting this policy instrument already prefigured the tone of his works from the late 1930s:

> A tariff is an affirmation of separatism, a refusal to co-operate, a declaration of rivalry. That twelve years after a war which devastated civilisation and threatened to destroy the goodly heritage of European culture, we should even be discussing such matters, is a sad reminder, not only that some men lose faith in a good ideal when it is not realised quickly, but that most are totally blind.[5]

The *Essay* was about economics writ large – both economics narrowly defined as a pure science, and broadly understood to comprise political economy as well. We concur with Masini (2009) that the *Essay* was part of Robbins' lifetime project to demarcate the positive and normative spheres of economics, and to illuminate the extent to which the pure science of economics could shed light on practical questions. In this sense, the *Essay* should be understood as an effort to identify the boundaries between economic science and political economy. Robbins' ultimate goal was to develop a methodological justification for a scientific approach that could somehow orient the formulation of public policies:

> Far from undermining the role of the economic advisor, Robbins's *Essay* was actually the necessary manifesto of the profession. It attempted to define exactly what might be asked of economists when required to give policy advice (Masini 2009, 434).

[4]The Papers of Lionel Robbins, LSE Archives, London (Henceforth P.L.R), Robbins/5/3, E.A.C (E.) 13, p. 27, September 23rd, 1930, emphasis in the original

[5]P.L.R, Robbins/5/4, E.A.C (E.) 65, p. 4, October 22th, 1930

In his Richard T. Ely Lecture, nearly fifty years after the publication of the *Essay*, Robbins was still replying to criticism elicited by this work. Struggling to clarify his position, Robbins insisted on the demarcation between economics and political economy, arguing once more that both were complementary for the purposes of policy analysis:

> Needless to say I do not at all deny that, in the course of evolution of economics as we know it, there has been a good deal of intermixture of political and ethical discussion with the scientific discussion of fact and possibility. [...] provided the logical difference between the two kinds of propositions is clearly kept in mind, I am in the least hostile to the combination. (Robbins 1981, 4)

Economics was no panacea for Robbins. He did not claim that public policies could be fully determined based on strictly economic knowledge, only that the tools of economics were useful to predict the outcomes of alternative policies. The economist's task was to compare these in every possible objective basis, always guarding "against the appearance of feeling greater certainty than we have".[6] This alone, however, was not enough to rank alternative policies, since 'better' and 'worse' were normative judgements, not economic categories. People were free to choose less over more if they wished, and this was not for the economist to decide. By the time the crowd started deliberating, the economist should be long gone. He might, of course, join the deputation, but he was impeded from doing this solely in his capacity as an economist. Provided they did not "claim scientific authority for conclusions which clearly go beyond science", there was nothing wrong with "practitioners of scientific economics discussing such questions of policy". If the distinction between science and policy is kept in mind, Robbins concluded, "there is everything to be said for the discussion of policy to be conducted by those who are aware of the objective implications of the values on which policy rests" (ibid., 6).

1.3 From Statics to Dynamics: Uncertainty and the Role of Time

If Robbins' epistemology gave rise to much controversy and misunderstanding, his engagement with the lively interwar debates on economic dynamics and cyclical fluctuations has remained

[6] P.L.R, Robbins/5/3, E.A.C (E.) 13, September 15th, 1930.

virtually unnoticed. "In the 1920s and the 1930s", according to Bruna Ingrao and Giorgio Israel, "the common ground for intellectual confrontation (not excluding controversy) was a renewed interest in the economy's cyclical fluctuations" (Ingrao and Israel 1990, 223). This concern was shared by luminaries such as Robertson, Keynes, Hayek, Schumpeter, Ohlin, Lindhal, Myrdal and Hicks, leading to an emphasis on "economic processes taking place in time", and bringing back to the fore the role of uncertainty and expectations — another important element of the British classical tradition, which had led, in the hands of John Stuart Mill and others, to pioneering explanations of the trade cycle (ibid., 225).

In the second edition of the *Essay*, Robbins argued that the theory of profits was "essentially an analysis of the effects of uncertainty with regard to the future availability of scarce goods and scarce factors", while the demand for money "can be deduced from the existence of the same uncertainties" (Robbins 1935, 77-78). Phenomena as diverse as exchange, production, and fluctuations ought to be analyzed considering that "people do not know the full implications of what they are doing" (ibid., 92). Likewise, dynamics should be a central concern of the economist since "the world of reality is not in a state of equilibrium, but rather exhibits the appearance of incessant change" (ibid., 100). Nevertheless, even though static analyses were not suited for addressing practical problems and formulating public policies, they were still useful first steps toward incorporating dynamic considerations. "We study these statical problems", said Robbins, "not merely for their own sake, but in order to apply them to the explanation of change" (ibid., 103).

Although Robbins has been often criticized for being an advocate of homo economicus, he considered uncertainty to be the "main postulate of the theory of dynamics" (ibid., 79), whereas perfect rationality and perfect foresight were simply auxiliary assumptions that should be dropped as the discipline matured. As early as 1927, he had already expressed his wish that, "somewhere about 1950, economists may hope that journalists and others will discover that 'economic man' is no longer assumed in their discussions, and will cease to acquire cheap reputations by pompous denunciations of this obsolete fiction" (Robbins 1927, 176). In the early 1950s, when the axiomatization of economics was gaining momentum, he realized that his dream had not come true, and lamented that economics was becoming more static and less dynamic:

> It will surely come to be regarded as a paradox in the history of thought that, just at a period when the problems of economic dynamics were beginning to be

> successfully tackled by methods which can properly be described as extensions of the subjective theory of value, there should have developed a tendency to restate the statical foundations in terms which deliberately eschew any reference to the subjective at all (Robbins 1953, 102).

Robbins' *Essay* was a product of its times, and uncertainty, dynamics, and the time element were ubiquitous topics in the early 1930s. In 1933, Robbins spent his vacations in Austria, where he had conversations with Haberler and Machlup that were "to bear fruit in the second edition" of the *Essay* (Howson 2011, 240).[7] More important, however, was the seminar at LSE led by Hayek, Plant and Robbins himself, and attended by Allen, Hicks, Kaldor, Lerner and Rosenstein-Rodan. The golden years of the seminar took place between 1933 and 1936: among the papers discussed in 1933, for instance, were Hicks and Allen (1934), Hayek (1934), and Rosenstein-Rodan (1934) (Howson 2011, 250). If Hicks would later state that his work during his years at LSE was "in large measure a collective work" (Hicks 1979, 196), the debates taking place at LSE certainly brought Robbins closer to current discussions about uncertainty and dynamics. The revised version of the *Essay* bears testimony to his renewed concern with these topics.

According to Hicks' recollections from this period, he was then attempting "to make the Paretian system less static, so as to be able to incorporate planning over time, planning for a future which was not known in advance", following Hayek's suggestion to "think of the productive process as a process in time" (ibid., 199). Rosenstein-Rodan (1934) and Kaldor (1934)[8] also discussed the role of time, and Robbins credited the former's "illuminating article" for showing there were "initial configurations of the data, which have no total tendency to equilibrium, but which rather tend to cumulative oscillation" (Robbins 1935, 102). In the preface to second edition of the *Essay*, Robbins mentioned having added some passages related to statics and dynamics, and said he hoped these changes would be "acceptable to my friends Professor F. A. von Hayek, Dr. P. N. Rosenstein Rodan and Dr. A. W. Stonier, whose advice and criticisms on these difficult matters have taught me much" (ibid., xi).

[7]The second edition was written during the 1934-35 academic year, and Robbins sent a letter to Machlup in 1935 saying that he "owe[d] much more to conversations with you and Haberler on this matter than to anything which has so far been published in any journal" (quoted in Howson 2011, 271). In the *Essay*, when discussing some of the differences between the social and natural sciences, he acknowledges his indebtedness to Machlup (Robbins 1935, 112).

[8]See Setterfield (1998) for a thorough discussion of Kaldor's paper, to which he traces his later interest in path-dependency.

Robbins was also much influenced by Menger, whom he saw as the marginalist economist par excellence, and by Knight's *Risk, Uncertainty, and Profit* (1921), which he used in his lectures on risk since 1929 (Weintraub 1991, 30). Another of his early influences was Dennis Robertson, whom he credited with "constructions which penetrate further into the problems of modern economic dynamics than anything that had been written before" (Robbins 1971, 223). Having read Robertson's study on industrial fluctuations (1915) in 1923, Robbins felt he was responsible, alongside Keynes and Hawtrey, "for the revival and advancement of monetary and aggregate analysis in this country in our time" (ibid., 220). All these layers of commentary and conceptual refinement were superposed, however, onto his wide reading of the British classical economists, who laid the groundwork for his own understanding of the role of uncertainty and expectations in economics.

As a "Cannan pupil", in the words of Dennis O'Brien, Robbins had "not only an excellent grounding" in the history of economics, "but also an early love for it" (O'Brien 1988, 114).[9] In his survey of the classical literature, Robbins stressed that the British classical economists did not reason based on "mathematical or semi-mathematical conceptions of statical equilibrium", but instead constructed analyses that were "much more dynamic and real than these exquisite laboratory models". Equilibrium was not conceived as a solution to a system of equations toward which the economy necessarily tended, but rather as a force directing "the tumultuous forces of self-interest" (Robbins 1978, 16). Authors like James Mill, Say, and the Ricardians, for instance, "were inclined to attribute the occasional breakdowns of trade to errors of judgment and disproportionate developments of production" (ibid., 30). The idea of proving the existence of equilibrium as a fixed point toward which the economy moved was entirely alien to the classical economists — and hence to Robbins — since they did not assume that the conditions determining equilibrium would remain unchanged for long enough to permit such a state to be reached. Equilibrium was thus used to explain the conditions of change and the direction of movement, not a final position. As Robbins explained in the *Essay*: "Through history, the data change, and though at every moment there may be tendencies towards an equilibrium, yet from moment to moment it is not the same equilibrium towards which there is movement" (Robbins 1935, 62).

In an earlier paper, Robbins had tried to distinguish between the classical and neoclassical

[9]O'Brien claims that "Robbins made the greatest individual contribution to the revival of the study of the history of economic thought in Britain" (O'Brien 1988, 114).

interpretations of a stationary equilibrium. Whereas the neoclassical framework postulated a fixed amount of productive factors as a condition of equilibrium, in the classical approach, the constant supply of capital and labor was "simply one of the resultants of the equilibrating process" (Robbins 1930, 204). Robbins subscribed to this classical notion of economic dynamics, where equilibrium was not conceived as a final state, but rather as a force pushing the economy in a certain direction. Instead of proving the existence of definite equilibria, one could thus focus on studying the process of convergence, and how uncertainty and the formation of expectations interfered therewith. As we will show in the next section, this had broader implications for Robbins' epistemological stance, where once again, one can clearly discern his classical lineage.

1.4 Robbins' Lost Epistemology of Political Economy: Economics as a Social Science

So far, we have argued that Robbins maintained throughout his life that economics narrowly defined was an important part of the toolkit of political economy, but that it should incorporate dynamic considerations and reduce its level of abstraction to become increasingly more useful for investigating practical problems. This section will connect these two points by arguing that his narrow conception of economics as a science was consistent with his broader view of economics as political economy. If the significance of economics lay in its usefulness for orienting public affairs, then the nature of economics should accommodate the features that distinguish it from the natural sciences: since individuals are purposeful and uncertain about the future, the methods of the natural sciences are not applicable to economics. Our claim, in short, is that uncertainty and dynamics have performed a double role in Robbins' work. At the epistemological level, they implied that economics was a social science by its very nature, and thus could not mimic the methods of logic and mathematics. Additionally, if the significance of economics depended on its serving as the toolkit of political economy, then one could not evade such salient features of the subject matter as uncertainty and time.

Robbins defended deductive reasoning against criticism from the German historical school and the American institutionalists, while simultaneously trying to preserve the social dimension of economic theory by insisting that, since atoms do not think, economics cannot mimic the methods of the natural sciences. Robbins was thus restating "the Senior-Mill-Cairnes position in modern language", but adding an Austrian twist to it by endorsing the means-ends definition of economics (Blaug 1992 [1980], 76). As argued by Giocoli (2003, 90), "Robbins's reconstruction of the epistemological status of the key economic principles", conflating English verificationism and Austrian deductivism, "offered an effective methodological compromise between the competing approaches to the 'true' nature of economic theory".

Here it will be useful to return to Robbins' survey of British classical political economy. Against denunciations of metaphysical contamination, he argued that while classical analysis was "teleological in the sense that, like all analysis of conduct, it runs in terms of purpose", this did not deprive it of scientific status (Robbins 1978, 23). The fact that individuals were purposeful did not impart a metaphysical character to classical economics. Instead, one should distinguish between metaphysics and science based on whether arguments were stated "dogmatically a priori or by way of appeal to experience" (ibid., 24). Robbins came back to this point in his Ely Lecture, stressing that economics was different from the natural sciences given the purposefulness of individuals, and hence "our explanations must to some extent be teleological" (Robbins 1981, 2). Furthermore, unlike natural scientists, economists could not make predictions, since individuals were able to learn and adapt to the environment. Time series might thus be an interesting method for analyzing the past, but not the future.

Robbins' conception of science was in sharp contrast with logical positivism, since he rejected the idea of a unified science with logic and mathematics as the proper instruments for all inquiry and held introspection as a central tenet to his notion of the individual. While the Vienna Circle dismissed introspection as tantamount to metaphysics (Caldwell 1994, 16; Giocoli 2003, 30), Robbins saw introspection as preferable to hedonism and behaviorism, since — unlike behaviorism — it maintained the purposefulness of individuals and their capacity to learn and adapt to a system in perpetual change, and – unlike hedonism — it did not equate utility with utility maximization. "So far as we are concerned", he remarked in the *Essay*, "our economic subjects can be pure egoists, pure altruists, pure ascetics, pure sensualists or — what is much more likely — mixed bundles of all these impulses" (Robbins 1935, 95). Introspection thus allowed Robbins, at the same time, to portray economics as an empirical science and to reject the idea that it should follow the methods of the natural sciences (ibid., 104-105). Robbins regarded behaviorism as insufficient to explain economic phenomena such as the formation of prices precisely because of uncertainty. An objective explanation of prices based on observation was impossible because their determination depended on expectations about future prices, hence one could not dispense with subjectivism. This, he said, was "one of the essential differences between the social and the physical sciences", namely that the former dealt "with conduct, which is in some sense purposive" and therefore could "never be completely assimilated to the procedure of the physical sciences" (ibid., 89).

Robbins was aware of debates taking place in the Vienna Circle at the time and sent a letter to Machlup in 1934 saying he thought the second edition of the *Essay* could "meet Haberler and Kaufmann without sacrificing anything fundamental".[10] The revised version of his book, however, did not bring Robbins' position any closer to the Vienna Circle. He thought his use of introspection was not significantly different from logical positivism, since he saw introspection simply as a specific form of observation.[11] He endorsed introspection and observation as criteria to differentiate science from metaphysics, yet deemed that his appeal to inner observation did not imply "sacrificing anything fundamental". Inner experience was analogous to empirical observation from the standpoint of scientific legitimacy, since we could observe our minds with accuracy and arrive at true statements that needed no empirical testing. In his typical conciliatory style, Robbins refrained from engaging the philosophical debates taking place in the 1930s, as indicated in an unpublished appendix to the *Essay*:

> I have made little or no allusion to recent controversial discussions of the ultimate status of economic generalizations. Indeed the careful reader, prying behind the actual structure of my sentences, may even detect a deliberate avoidance of terms which commit me to one view or the other.[12]

Already in the preface to the first edition, he clarified the work was based on the practices of economists, and that he had "eschewed philosophical refinements as falling outside the province in which I have any claim to professional competence" (Robbins 1932, viii).

In *The Significance and Basic Postulates of Economic Theory* (1938), Terence Hutchison criticized Robbins for championing the old-fashioned view that "stressed subjectivism, methodological individualism, and the self-evident nature of the basic postulates of economic theory" (Caldwell

[10]Letter from Lionel Robbins to Fritz Machlup, January 1934, quoted in Howson (2011, 271).

[11]"introspection [...] was universally regarded in the past, whatever may be the fashion today, as an 'empirical' technique of investigation, and sharply distinguished from intuition or 'innate ideas'" (Viner 1958, cited in Blaug 1992 [1980], 74).

[12]Cited in Howson (2011, 272). She further notes that one of the reasons he presented for not picking sides on the controversy was his "philosophical incompetence".

1994, 99).[13] Even though Caldwell claims that Robbins did not dismiss the usefulness of empirical studies, simply limiting them to the "heuristical role [...] of suggesting new problems for theory to solve" (ibid., 102), we have already argued that Robbins saw empirical studies as essential to bridge the gap between theory and application.[14] In hindsight, it was Hutchison's rather than Robbins' views that were embraced by economists, positing that economics should investigate laws that are falsifiable in principle (though not necessarily in practice), and that introspection be abandoned in favor of empirical investigations of economic behavior.[15] Robbins' attempt to preserve volition and uncertainty — insisting that economists study flesh and blood individuals, while eschewing psychology (by clinging to introspection) and simultaneously affirming the scientific character of economics — was perhaps an unstable compromise. Still, we may admire his effort to defend social science against what he saw as the lesser social science practiced by the German Historical School and American institutionalists, on the one hand, and the social science of behaviorists and others attempting to mimic the methods of the natural sciences, on the other. While the soundness of his methodological position is debatable, his attempt to bridge different traditions should not be overshadowed by the inherent difficulty of the enterprise.[16]

An interesting outcome of Robbins' conflation of many different elements in the *Essay* is that, as maintained by Hands, "these philosophical tensions actually contributed to its influence". The double exclusion of hedonism and behaviorism "allowed Robbins' approach to accommodate, and steer a path through, the complex problem situation that confronted marginalist economics during the first third of the twentieth century" (Hands 2009, 831-832). Embracing introspection, however, ultimately meant reaffirming the difference between the social and the natural sciences. Robbins championed rational choice, but his conception of rationality — meaning simply that individuals were purposeful, could order their alternatives and choose between different means

[13]Hutchison spent a year at the LSE in 1934 and moved to Bonn in 1935, where he wrote his 1938 book (Howson 2011, 271).

[14]Indeed, his *The Great Depression* (1934) has 36 pages of statistical appendix, and chapter 5 of the *Essay* discusses the role of empirical studies, for instance, in determining which theory is appropriate to each problem.

[15]Whether economists consistently follow the tenets of positivism or simply pay lip service to it is, of course, an entirely different question.

[16]As an anecdote of his conciliatory prowess, Aaron Director commented on Robbins having written the Statement of Aims of the Mont Pelerin Society that "nobody else at the meeting [...] could have reconciled the differences in politics among the participants [...] as well as Robbins. After we had spent days discussing these issues and tried to draft a statement, Lionel finally took it over and drafted the one we all signed" (cited by Ebenstein 2001, 145). Howson (2013, 115) also notes that Robbins thought intellectuals should not engage with any party lest they lose their intellectual independence: "By the time he became a peer in 1959, he had voted for all three major parties; in the Lords he sat on the cross benches". Though his conciliatory stance may be partially a trait of his personality, the environment he found at the LSE helps to explain his views on the relationship between social science and politics (Suprinyak and Oliveira 2018).

— was very different from the one currently prevailing.[17] The fact that there are elements in Robbins compatible with later developments does not mean he would have supported such developments, or that he should be regarded as a forerunner of axiomatics or rational choice theory. As Backhouse and Medema (2009) have shown, his definition was only fully endorsed in the 1970s. Economics went through significant changes in the intervening decades, so that by the time his definition gained currency a different image of economic science was already established (Giocoli 2003). This new image, however, had little in common with Robbins' own conception. At any rate, his definition was embraced in the 1970s due to the rise of formal methods in economics, rather than the reverse. His views were much more connected to past economists (the English classical tradition) and contemporary ones (especially the Austrians) than to future developments in economics, which he would have almost certainly rejected.

Robbins did not champion purely formal relations as abstract schemata, arguing instead that the propositions of economics must refer to reality. More broadly, he insisted on the differences between the social and the natural sciences: individuals are rational since they are purposeful, yet they make choices in an uncertain and constantly changing world. The regularities studied by natural scientists hardly occur in the social realm:

> In the natural sciences the transition from qualitative to quantitative is easy and inevitable. In the social sciences [...] it is in some connection almost impossible, and it is always associated with peril and difficulty. It seems clear, from what has happened already, that less harm is likely to be done by emphasising the differences between the social and the natural sciences than by emphasising their similarities (Robbins 1935, 111-112).

Even though Robbins became famous for his definition of economics, conceiving economics as the science that studies the choices of individuals does not tell us much about Robbins' conception of science and of how individuals make their choices. Within his general definition there is scope for different approaches such as hedonism, behaviorism and introspection, which significantly influence how economists theorize. Hence, while Robbins is often thought of as a precursor to rational choice theory and axiomatization, one must recognize that his rejection of hedonism and behaviorism, in favor of introspection and subjectivism, implied he did not

[17] For a discussion see Brown and Spencer (2007), Falgueras-Sorauren (2007), Ross (2007), Oliveira and Suprinyak (2018).

conceive economics on a par with neither mathematics nor physics. His understanding of 'economics in time', and the central role of uncertainty, ruled out determinism and hence the possibility of establishing mechanical analogies or building a strictly formal system of relations.

Individuals err and learn, hence economics is the science that studies the choices of individuals in a system of perpetual change. Introspection, for Robbins, also meant attachment to reality, for he saw what he described as human faculties as obviously true, not requiring any experiments to validate them. It followed, so he thought, that deductions from assumptions which were trivial would themselves also be true. Robbins claimed he was simply stating the main principles of economics as they already existed, but the conflation of different traditions in his thought resulted in a peculiar way of conceiving economics that is difficult to label as belonging to any specific 'school'. While his definition of economics gained currency, his eclectic epistemology of political economy, firmly grounded in the classical tradition to which he belonged, went lost.

1.5 Conclusion

Uncertainty, time, and dynamics were central to the second edition of Robbins' *Essay*, and this may be partially explained by the economic context of the 1930s, with discussions of business cycles and economic fluctuations coming to the fore. More importantly to our argument, however, uncertainty was not only important for the construction of theories — it also reverberated on Robbins' epistemology. Even though his lost epistemology of political economy may be criticized on many grounds, the importance he attributed to time, uncertainty, and dynamics, together with his conception of economic science as a toolkit to orient political economy, are all important questions that are not often addressed by contemporary economists. Not only were theoretical concepts essential to analyze a system in perpetual change thus gradually lost from sight: the nature of the relationship between economics and political economy suggested by Robbins also went unnoticed.

Whether Robbins' conception of a value-free science is indeed achievable is still an open question among philosophers of science (Scarantino 2009). Another possible criticism of Robbins is that his hope that economics would become increasingly more useful for practical problems was hindered by the theory of value he endorsed (Brown and Spencer 2012). These are valuable objections and important elements to assess the contemporary state of economics and political economy. Our main point, however, is not to argue that Robbins' nuanced program offers an effective solution for contemporary conundrums, but rather that revisiting his works and their intellectual context may provide suggestions about how economics and political economy could be brought closer together after a long period of drifting apart from one another, while also offering a window into what happened to political economy when seen through the lenses of postwar economics.

Chapter 2

From Divergence to Convergence: The Making of Mathematical Economics between 1933 and 1967

The chapter argues that the mathematization of economics between the 1930s and the 1960s was propelled by the political tensions in Europe. My claim is that quantitative methods grew in importance during the period because they were perceived as politically neutral and therefore able to orient public policies by eschewing ideological charges. Moreover, I provide an empirical account of the changing nature of economics using Latent Semantic Analysis. It is shown that between 1933 and 1967 a shift took place from divergence of ideas to convergence of beliefs such that in the 1960s the language used by the top journals of economics had become considerably more homogeneous. Not only the top journals became more similar throughout the period, but also the papers published in these journals became more alike. The evidence suggests that by the mid-1960s the main journals of economics had converged to a paradigmatic core.

2.1 Introduction

Whiggish explanations of the mathematization of economics abound among practitioners, who often and unabashedly conflate Adam Smith's conception of equilibrium with the proof of the existence of equilibrium by Arrow and Debreu (1954). Even scholars working on the history of economic thought have overstretched the predominance of mathematical methods beyond what is empirically observable (e.g. Schabas 1989, Lawson 2012). As argued by Morgan and Rutherford (1998), however, if economics was plural previous to World War II and different traditions as historical studies, empirical research, and deductivism were perceived as rigorous, then it does not make sense to argue that economics became increasingly more rigorous since there was not a single criterion to identify what counted as scientific research. My claim is that due to the political circumstances of the 1930s / 1940s, quantitative methods became increasingly associated with rigorous research since they offered protection against accusations of ideological biases. Contrary to Lawson's (2012) claim that the mathematization of economics has been underway since the times of Newton and that society has enshrined mathematics at least since the Enlightenment, my argument is that American society only gazed in awe at mathematical economics after 1930. Economists sought refuge in the supposedly ideologically neutral character of quantitative methods as a response to totalitarian tendencies happening in Europe.

There is a consensus that quantitative methods raised to a dominant position within economics following World War II, and the extant literature shows that the pluralism that characterized economics in the 1930s gave way to a more homogeneous program centered on the concepts of rationality, maximization , and equilibrium. Scholarly attention has typically proceeded along two lines, either focusing on intellectual trajectories of isolated individuals and their effects on the transformation of economics, or discussing the evolution of specific sub-disciplinary branches (e.g., Monetary Economics, Consumer Theory). Although there is undoubtedly much value in such investigations, the literature offers inconclusive explanations about how and when the community of mathematical economists redefined the nature of economics. Moreover, the effect of the historical context on these changes was as important as theoretical issues themselves. By isolating parts of a multifarious story, or focusing on individuals who would only become central figures later, one risks exaggerating the importance of economists or theories and losing sight of the context in which they developed.

The present chapter makes two contributions to this literature. First, I argue that the shifting disciplinary boundaries of economics between the 1930s and the 1960s was a historically contingent process. The normative / positive divide gained stronger contours in the United States since the 1930s because of the politically charged environment of Europe and quantitative methods gained momentum because they were perceived as a politically neutral way out of ideological conundrums. The second contribution of the chapter is a Latent Semantic Analysis using the full papers of twelve journals in order to compare 1933-1937 and 1963-1967. My goal is to investigate questions such as: the extent to which the language used by economists became more homogeneous during the period, whether a shift from divergence of ideas to convergence of beliefs can be observed, and, if so, which journals formed the network's core in the 1960s. My account is centered on the United States because of the leading role the country was to assume in the mathematization of economics following the Second World War.

2.2 From Divergence to Convergence? The Quest for Objectivity and the Development of Mathematical Economics

During the 1930s and 1940s mathematical and econometric theories were developed, and the seeds of an international network of economics were sowed. Mathematical economics, however, was restricted to a small group among the profession and the stabilization of mainstream economics was a process spanning many decades and driven by a number of political, economic, methodological, and theoretical issues. There is a wide set of intellectual factors that help to explain the transition from interwar pluralism to neoclassical economics. Although discussing such issues is beyond this chapter's scope, a fuller account of the mathematization of economics would involve an assessment of the role of metaphors from physics in the development of economics (Mirowski 1989), the interconnections of mathematical economics and the history of General Equilibrium Theory (Ingrao and Israel 1990), the long-struggle for defining the relationship between psychology and economics in the first half of the twentieth century and the stabilization of consumer theory in the post-war period (Giocoli 2003, Bruni and Sugden 2007, Moscati 2007, Hands 2010), and von Neumann and Morgenstern's saga from a politically charged interwar Europe to Princeton and the Cold War atmosphere (Leonard 2010).

The political and economic turmoil that characterized the 1930s led to the emigration of many scholars to the United States who would later be key players in the mathematization of economics. More importantly, the increasing ideological tensions during the period would lead to an increasing demarcation between normative and positive policy recommendations which benefited quantitative studies due to its allegedly political neutrality. As discussed in the next section, ideological issues would become stronger than ever during the Cold War, intensifying the anti-socialist stance that had long characterized American society. The Second World War and the Cold War cleared the ground for neoclassical economics to develop, by favoring discourses that were perceived as ideological neutral, on the one hand, and by the successful engagement of economists during the war effort, allowing economists to assert themselves as detached experts at the service of the state, on the other.

The professionalization of economics can be traced back to the late 19th century creation of the American Economic Association, but it was still unresolved in the interwar period. Since the origins of the Association, the limits of pluralism were evidenced by the necessity to strike a balance between social reform and impartial expertise. Yearning for the status of experts that could orient public debate, economists often frowned upon politically tinged approaches, and the debates about the scope of the Association are marked by appeals to impartiality and objectivity (Bernstein 2001, Fourcade 2009). Although Richard T. Ely had originally conceived the Association to be the home of economists who "repudiate *laissez-faire* as a scientific doctrine", when he actually drafted a proposal for the Association he toned down his message (quoted in Coats 1960, 556). At stake was the perception of many economists that the religious overtones of social reform were incompatible with the scientific requirements of impartiality and objectivity. Although the founding members of the Association had imagined it as a forum for social reform, their project was frustrated by the orthodoxy, who portrayed advocacy as the nemesis of science (e.g. Laughlin, Taussig, and Hadley).

The quest for scientific status and the effect of politico-economic conditions were fellow travelers in the transition from divergence of ideas to convergence of beliefs, and the criterion of what counts as scientific research proper became increasingly equated to mathematical and quantitative methods since the 1930s as a response to the heightened ideological disputes of the period. As shown by Fourcade (2009), differently from France and England the professionalization of American economics was forged outside of the state. The profession developed its status as technical experts by maintaining a self-referential discourse, which in turn was a response to the power of business elites and the fear of political radicalism.

Until the interwar years, however, objectivity was not equated to quantitative methods.

Pluralism was a defining feature of economics prior to the Second World War and it was manifested in the coexistence of multiple schools of thought and in the interdisciplinary character of research. Underlying such diversity was a broad conception of scientific research which encompassed historical, logical, inductive, and other methods. Institutionalism, arguably the main school in the interwar period, was united by its vision of science rather than its theoretical agenda. It comprised different methods and research programs spanning business cycles, employment, and legal issues, and its coherence stems from its commitment to empirical research, from its pursuit of sound theories that avoid simplifying assumptions, and due to the importance attributed to the effect of institutions on human behavior. Institutional reforms were seen as paramount to curb the enthusiasm of the market system. Different theoretical and methodological positions also existed within neoclassical economics, with the marginalist J. B. Clark criticizing the static character of the theory of competition and pleading for a dynamic theory, while Allyn Young insisted on the need for greater realism. Even comparing approaches that would later form the core of the profession, such as price theory at the Chicago School, Walrasian economics at the Cowles Commission, and the revealed preferences of Samuelson at the MIT one finds considerable variety (Mirowski and Hands 1998, Morgan and Rutherford 1998).

As argued by Mata and Medema (2013), the development of social science metrics following the Progressive Era represented new epistemic standards where the uses of social science for policy matters became increasingly reliant on mathematics and statistics. An early example of the uses of economic indicators for public interventions was the development of statistical series at the National Bureau of Economic Research (NBER) in the 1920s. Requested by the US Senate, Kuznets' (1934) estimation of the contraction of the economy during 1929-1932 drew on the techniques developed at the NBER and helped to guide policy-makers in their strategies for coping with the recession: "There is a tight linkage, indeed, between the GDP, the application of accounting methods to economic analysis, Keynesianism, and the invention of the 'economy' as an object of knowledge and government" (Eyal and Levy 2013, 237). The 1920s creation of organizations that focused on empirical research was financed by the Carnegie Corporation and the Laura Spelman Rockefeller Memorial due to their interest in "intelligent social control" (Fourcade 2009, 67). The NBER was an important landmark in the mathematization of economics because it embodied the technical and impartial requirements long sought by economists; it "served as an exemplar to many, in government, in the academy, and in the wider community, of the deployment of professional expertise in the solution of public problems". The Brookings Institution, created in the same period, was less able to claim impartiality as it became increasingly associated with the Democratic Party (Bernstein 2001, 43). In spite of the growing importance of the NBER, the public recognition of the virtues of economics in the management of public affairs was hindered by the profession's failure to adequately deal with the 1930s recession.

Economic circumstances and the development of economic theories walked hand-in-hand; throughout the 1930s economists were called by the government to solve socio-economic problems and few economists sought refuge to from those problems during the Great Depression by studying technical issues related to utility maximization. Indeed, "a large contingent believed precisely the opposite: that recent experience had effectively repudiated any economic theory based on such abstract principles" (Mirwoski and Hands 1998, 262). Policy problems changed after 1940 with the recovery of the economy coupled by the imperatives of the war. Economists were now being summoned by the government to solve military problems, which were more amenable to technical tools. The Second World War was a watershed moment due to economists' increasing use of mathematical optimizing models, linear programming techniques, and statistical measurement devices (Bernstein 2001, chap. 3, Fourcade 2009, Leonard 2010, chap. 12). The "good war" fought by economists who became leaders of the profession (e.g. Arrow, Friedman, Hotelling, Koopmans, Samuelson, and von Neumann) thus vindicated their claim as rightful experts in orienting public policies (Weintraub 2002, 224).

Military and resource allocation problems were successfully tackled by neoclassical economics' arsenal since they could be framed as optimization and choice-theoretic problems. Moreover, in the preparation for the war the government created a number of agencies to an unprecedented extent, offering venues where the economist could participate directly in public affairs. The historical contingencies of the 1940s therefore enabled the public recognition of the economist's value since public spending and the state apparatus considerably grew in the period, creating opportunities for economists to engage in the public sphere, and the solutions devised by neoclassical economists were effective given the suitability of their toolkit to the nature of the problems involved. Prior to the Second World War most economists were recruited temporarily by the government and only in the late 1940s the number of permanent positions in the government increased (Fourcade 2009). Following the war the international dispute with the Soviet Bloc provided further impetus for developing linear and dynamic programming techniques, and the government also invested heavily in promoting game theory since it was perceived as capable to address strategic problems associated with the Cold War (Bernstein 2001 chap. 4, Leonard 2010 chap. 13, Erickson et al. 2013).

The transformation of economics gained momentum with the political context of the late 1950s. In 1957, the Sputnik represented a triumph of communism since the United States had not yet managed to get a satellite into orbit. This was a political and cultural shock that led to a federal revaluation of the importance of science and mathematics in the country. The best students increasingly focused on these disciplines thereafter: "By 1960, all those who wished to go into a social science after graduation were encouraged to take more rather than less mathematics". If heated debates about disciplinary boundaries and the relationship between economics and mathematics characterize the period from 1930 to 1960,

> [b]y the 1960s, there was no contest at all. Economics had changed its character, its language, its way of representing its own concerns. Economics by the 1960s had become a science of building, calibrating, tuning, testing, and utilizing models constructed out of mathematical and statistical-econometric-materials (Weintraub 2002, 252-255).

That neoclassical economics constituted a hegemony in the 1960s can be seen by the fate of the Union of Radical Political Economists, founded in 1968. Radical economists objected that standard economics no longer inquired into the pressing issues of the day such as racism, poverty, and imperialism. The counter-reaction of neoclassical economists came in the form of a series of dismissals and denials of tenure to young radical economists, who were marginalized by being forced into less prestigious centers in the early 1970s, where their message was less likely to be effective. Harvard had a leading role in the campaign against neoclassical economics because of the visibility the center enjoyed, but after Samuel Bowles and Arthur MacEwan were denied tenure the group of radical economists based in Harvard was dismantled (Mata 2009).

The marginalization of the Harvard group is symptomatic of the shades that economics had acquired in the Cold War period. If radical economists could not forward their project even in a prestigious center, the outlooks for the movement as a whole were truly dim. Although dismissals and denials of tenure for political reasons have happened since the late nineteenth century (Goodwin 1998), the radical economists episode in the 1960s was different from previous dismissals in an important sense. Whereas economists had lost their job in the past for their criticism of the economic system and their alleged support of socialism, in the 1960s radical economists questioned the economic paradigm centered on methodological individualism arguing that it left no scope for broader societal problems related to power and class conflict. The young radicals of the 1960s were marginalized not only because they criticized the economic system, but also because they pointed their fingers at the limitations of the economics paradigm. Previous dismissals had not been justified on methodological grounds simply because there was no paradigm to defend.

2.3 In Math we Trust: The Consolidation of Mathematical Economics

In the aftermath of World War II the importance of the patrons of economics grew to an unknown scale due to the sharp increase in the number of university students with the return of veterans and the rising population. The imperative of patronage coupled with the tensions of the Cold War led to a "doctrinal and methodological cleansing" in the universities during the period, with neoclassical economists having the advantage of speaking a more inaccessible language, which appeared to be politically neutral to the layman (Goodwin 1998, 61). In this climate, the translation of Keynesianism and old-fashion institutional monetarism into the neoclassical framework gained currency (Balisciano 1998, Mehrling 1998). Likewise, although Coase's message in "The Problem of Social Costs" (1960) was closer in spirit to "old institutionalism", it was selectively absorbed into mainstream economics (Medema 1998). This methodological homogenization was favored by the AEA's recommendations in the early 1950's concerning the need for a standard curriculum focused on mathematical and quantitative methods, and by external funding from the Ford Foundation, the Alfred P. Sloan Foundation, and the National Science Foundation, which privileged formal methods due to the scientific competition with the Soviet Union (Fourcade 2009).

One of the centers that adapted earlier to the changes taking place in economics was Chicago. Emmet (2011) argues that part of the success the department enjoyed following World War II can be attributed to the curricular changes happening there during the 1940s. The boundaries of economics narrowed in the department during this decade, with students starting to be trained to become specialists, shifting away from the defining features of the Progressive Era. Education became more focused in developing new methods and in disciplinary self-critique, a process that was reinforced by the introduction of workshops and research groups in the late 1940s. The transformation of the University of Chicago had repercussions on the relationship between law and economics, which acquired "a distinctly different flavor" since the times of Henri Simon's *A Positive Program for Laissez Faire* in 1934 (Medema 1998, 208). Chicago also played a pivotal role in the monetarist-Keynesian debate after WWII, with Friedman taking the lead. Friedman was not a champion of the Walrasian monetarism that would eventually become the mainstream. Instead, he rejected static equilibrium, seeing the economy as marked by dynamic disequilibrium processes, and favored inductive processes as he had learned from his teacher Mitchell. However, the language of monetary economics was changing, and so was Friedman. If in the beginning of the century economists had to compromise in terms of their political positions if they were to secure their positions in the academy, Friedman's coming to terms with Walrasian economics suggests that in the 1950s there was an increasing tendency of economists having to compromise in terms of their methodological precepts if they wanted to belong to the emerging mainstream (Bateman 1998, Mehrling 1998).

A few anecdotes illustrating that in the 1940s times were a-changin' are the fact that Hayek's *The Road to Serfdom* (1944) was published as a cartoon to increase its reach, and that the National Economic and Social Planning Association was renamed National Planning Association after the war (Balisciano 1998). Balisciano concludes that macroeconomic planning thrived during the period because it did not require big institutional reforms, and because it was perceived as both economically advantageous and politically feasible. Even macroeconomic planning was nevertheless kept in check and viewed with suspicion due to the increasing ideological extremism of the Cold War, as can be seen by a comment about Samuelson in the right-wing *Educational Reviewer*:

> Now if (1) Marx is communistic, (2) Keynes is partly Marxian, and (3) Samuelson is Keynesian, what does that make Samuelson and others like him? The answer is clear: Samuelson and the others are mostly part Marxian socialist or communist in their theories (quoted in Goodwin 1998, 58).

Giraud's (2014) account of the making of Samuelson's *Economics: An Introductory Analysis*

(1948) nicely illustrates the issues at stake. Originally conceived as a policy-oriented textbook, the book faced strong opposition from MIT's board of trustees since in its initial version the manuscript emphasized the pitfalls of the free-enterprise system such as imperfect competition and involuntary unemployment. This led him to frame the argument in a more theoretical mode of expression and remove several passages from the original manuscript, which allowed him to keep the broader policy implications and substance of the original version, but made it appear less ideological beneath the public's eyes. Criticism to Samuelson was not restricted to the patrons of economics, and scholars such as Axel Leijonhufvod, Hyman Minsky, Paul Davidson, and Robert Clower argued that Keynes was lost in translation with the neoclassical synthesis. Leaving aside the issue of where Keynes lives in Samuelson's textbook, what is more important to my argument is what Samuelson actually accomplished. As a bestseller that influenced generations of economists, Samuelson's *Economics* managed to convey pro-interventionist statements in the *only* language that could lend credibility to his message at a politically charged period. In the public imaginary there was a very thin line between state intervention and socialism in the 1940s; it was precisely because of the mathematical and allegedly neutral nature of neoclassical economics that Samuelson was able to advocate public intervention without being dismissed. Indeed, he became a close adviser to president Kennedy (Fourcade 2009).

In Samuelson's (1997) memoir of the writing of his textbook he discusses the ideological pressures during the period. He recalls that Lorie Tarshis' *The Elements of Economics* had a good sale in its first year, but was harshly attacked as being "Keynesian-Marxist" and consequently the book was driven out of the market. When Samuelson was criticized on the same grounds, he had to adopt a middle-ground position so that his book could see the light of the day:

> I had considered it good business to articulate carefully just when and why an unorthodox paradigm might make sense under certain conditions, whereas at other times orthodox paradigms would commend themselves ... I could only gain from being eclectic and centrist (Samuelson 1997, 158-159).

He further notes that prior to publication of his book, MIT businessman alumni and board members called him heretical and said he was not practicing sound economics, such that even prior to McCarthy's era pro-interventionist arguments had to be crafted in a politically acceptable way. As argued by De Vroey and Duarte (2013), Samuelson saw the "neoclassical synthesis" as a political consensus, not a theoretical one. It was not a synthesis of the

Walrasian and Keynesian programs, but rather an acknowledgment of the fragmentary nature of macroeconomics. Samuelson was trying to convince microeconomists that the economy behaved differently in the short and in the long-run such that there was a legitimate reason for state intervention, while, at the same time, general equilibrium theory was also a useful program to illuminate other sorts of questions.

Klein was another prominent mathematical economist who struggled with the ideological conundrums of the McCarthy era. He worked with Samuelson in the project of applying Keynesian theory to policy matters, and because of his left-wing inclinations he was denied tenure at the University of Michigan in 1954. After spending four years in Oxford where he found greater academic freedom, he moved to the University of Pennsylvania in 1958 (Bjerkholt 2014). Like Samuelson, he mentions the need to adopt a mid-ground position in order to advance one's research agenda: "[Leo Szilard] taught us a lot about the strategy of intellectual research and the blending of politics and science" (Spencer and Macpherson 2009, 4). As argued by Pinzón Fuchs (2003), in the mid-twentieth century economics became a "tooled" discipline and one significant change that happened in macroeconomics was that the distinction between theory, application, and policy became blurred within macroeconometric modeling. Contrasting Friedman and Klein, he argues that while the former's models were geared to understanding rather than acting upon the economy, Klein was concerned with economic planning and saw the construction of macroeconometric models as a necessary element to persuade policy-makers. The necessity to use language carefully is also expressed in the Cowles Commission initially labeling its research program as "social engineering", but quickly substituting the term for "economic policy, probably to avoid connotations of 'central planning'" (Epstein 1987, 61-62).

The nature of economic models and their role in orienting public policies has been dealt extensively by Morgan (2003, 2012). Models are a reasoning style adopted by economists to tell narratives by working out the implications of the underlying theories. In the first stage, the model is crafted by drawing on theories about the world, and, in the second stage the researcher analyses what happens when the parameters of the model change. In this sense, although at the initial step the model is designed to represent certain aspects of reality, in the subsequent stage the manipulation of the parameters of the model can be used to inform previously unknown situations about the world. Hence, Morgan argues that models have played an important role in history of economics by illuminating possible realities that could not be foreseen with theory alone. Narratives are then the nexus between the inductive stage where information flows from the world to the model and the deductive stage when the outcomes of the modeling effort are used to tell something potentially relevant about the world: "[N]arratives provide the possible correspondence links between the demonstrations made with the model and the events, processes and behaviour of the world that the model represents" (Morgan 2012, 243).

With the introduction of models in the mid-twentieth century the old classical distinction between science and art became intertwined since economics evolved into a "tool-based discipline". Prior to the 1930s the government played a smaller role and restricted its actions to areas such as trade and exchange rate policies, but with the Great Depression and the Second World War the size of the state increased substantially. Between the 1930s and 1950s politicians demands for economists expertise led models to became a central tool in managing the economy, and the 1950s and 1960s were "perhaps the high period of the economist as engineer, advising the government on how to set the levers of economic control." (Morgan 2003, 294).

According to Mankiw (2006), while it has become more theory-oriented and less useful to address policy issues after the 1970s, macroeconomics was in its early years (the 1930s) geared towards practical concerns and therefore more akin to engineering. By the 1960s there were a number of econometric models drawing on the IS/LM model, which were widely used for forecasting and policy analysis, for example by the Federal Reserve. Mankiw argues that when he was the chairman of the Council of Economic Advisers between 2003 and 2005 the macroeconometric models they used were very similar to those of Klein, Modigliani, and Eckstein because post-1970 developments in macroeconomics had very little to offer in orienting his work at the government.

In this connection, economists are products of their times and many scholars involved in the neoclassical synthesis have explicitly noted the effect of the Great Depression in their own thinking. Klein, for instance, says that "I entered economics because, as a youth of the Depression, I wanted intensely to have some understanding of what was going on around me". Samuelson, likewise notices that the "year 1932 was the trough of the Great Depression, and from its rotten soil was belatedly begot the new subject that today we call macroeconomics". Tobin says he entered economics not only because it was intellectually challenging, but also because of the "obvious relevance of economics to understanding and perhaps overcoming the Great Depression and all the frightening political developments associated with it throughout the world". He goes on to argue that the following generation of economists, not having experienced the Depression, became more interested in mathematical puzzles for their own sake (Spencer and Macpherson 2009, 1, 30, 75).

Whatever criticisms may be levelled at neoclassical economics, it can be argued that the rise of mathematical economics allowed economists to make pro-intervention arguments during a period when the patrons of economics were highly suspicious of ideological biases. Institutional economists, lacking mathematical models, could not push forward their agenda under such circumstances. That mathematical economics made economists' pro-state claims impervious to ideological accusation can be seen by the development of welfare economics in the period.

The field derives mostly from Pigou's argument that the marginal social net product and the marginal private net product are not identical in the presence of externalities. Drawing on Marshall, Pigou cast the analysis in a marginalist cost-benefit framework and showed that although self-interested behavior leads to the equalization of private net product, it does not follow that the national dividend is maximized since this depends on the equalization of marginal social net product. He thus opened an avenue of research for scholars interested in making the neoclassical framework compatible with state intervention, laying the foundations for the development of the neoclassical theory of market failure which was carried on with increasing mathematical sophistication between the 1930s and the 1960s. The rhetorical power of the analysis by neoclassical economists proving that optimal conditions could be achieved by means of taxes and subsidies was that the "role of government vis-à-vis the market was no longer an a priori set of assertions nor an opinion based upon casual empiricism; it was demonstrable, in a 'scientific' sense" (Medema 2009, 76).

Although Pigou's use of marginal returns and marginal costs was a far cry from British classical economists from a methodological perspective, he followed the classical position that laissez-faire does not imply a stateless world. According to Glory (2019, 6), the caricature of Smith as a champion of a flawless free market was a product of the political views of the Chicago School, especially in the pens of Stigler and Friedman who "distilled, amplified, and disseminated an image of Adam Smith that was more in tune with the ascendency of rational-choice theory and strident market advocacy". She argues that although Knight and Viner are partly responsible for the uses and abuses of the invisible hand, their interpretation was more rounded than the younger generation's and left more room for issues in political economy. In this sense, while it is true that in the late 1970s the neoclassical synthesis would lose currency and a much more virtuous vision of the market came to dominate economics, the stabilization of mathematical economics between the 1930s and the 1960s took place during the "golden age" of the welfare state: "Expansion in public welfare has never been more far-reaching than during the years 1945-1975" (Schustereder 2010, 45).

Equating mainstream economics with an anti-state stance thus does not hold up to historical scrutiny, and in the period under analysis it certainly was not the case. Methodological and ideological issues must be distinguished since a number of liberal writers had strong reservations to the use of mathematics in economics (e.g. most of the classical and the Austrian economists, and some members of the Chicago School, such as Knight). Muddying the waters of the relationship between political inclinations and methodological preferences, one finds attempts to conciliate socialism and Walrasian economics in the likes of Lange. The intricacies of making such connection are exemplified by Patinkin's (1995, 132) remark that

> it was the socialist Oskar Lange who extolled the beauties of the Paretian optimum achieved by a perfectly competitive market — and Frank Knight who in effect taught us that the deeper welfare implications of the optimum were indeed quite limited.

In this sense, both camps of the ideological divide hosted supporters and critics of mathematical economics. Economists do not develop their theories in a vacuum and there is an interplay between politico-economic circumstances and the evolution of economic ideas. Economists help push forward governmental agendas by formulating theories that support them, but, reciprocally, historically contingent social processes shape economists beliefs. The two major paradigmatic shifts in economics in the 1930s and 1970s were not merely the product of intellectual battles, they reflected the economists' engagement with external circumstances. It may be true that mainstream economists usually avoid questions related to class struggle and power relations, restraining their analysis to optimality conditions and the role of the state in correcting deviations from equilibrium. What is less often recognized is that precisely because neoclassical economists melded state intervention and mathematical discourse, refraining from polemical debates that would hardly find an audience in America anyway, mathematical economics moved from a marginal to a central position between the 1930s and 1960s.

2.4 Data and Methods

The empirical exercise investigates how the language used by economists changed between 1933 and 1967, and whether the divergence of ideas in the 1930s gave way to a convergence of beliefs in the 1960s as discussed in the previous sections. The first wave covers the years 1933-1937: the starting date was chosen because of the foundation of *Econometrica* in 1933, with the explicit goal of promoting mathematical economics. The years 1963-1967 were chosen as the second wave because, as shown by Backhouse (1998), although in 1960 the use of mathematics was still not pervasive, there was a significant mathematization of economics in the period 1955-1960 which suggests that the consolidation of mainstream economics was on its way.

The five-year interval for each wave was chosen for technical reasons since Latent Semantic Analysis (LSA) requires a large number of documents to be effective. Full papers were downloaded from JSTOR and rejoinders, comments, and such like were excluded. I also chose not to include special issues, even though the number of papers is so large that including them would not make much difference. The list of journals is presented in Table 2.1, and it consists of nine journals of economics, two journals of sociology, and the interdisciplinary Marxist journal *Science and Society*. The journals of sociology were used to assess whether any significant difference in the language used by economists and sociologists existed, and whether this changed during the period. *Science and Society* was included to see whether the context of the Cold War led to the marginalization of Marxist scholarship. The *Southern Economic Journal* was included as a control for second-tier journals, to examine whether or not all journals converged during the period and whether any difference between the top journals and less prestigious outlets existed. 1589 papers were downloaded for the period 1933-1937 and 1916 papers were downloaded for the period 1963-1967.

LSA is a method to determine the similarity of words and documents.[1] Similar to factor analysis, LSA identifies the underlying factors that help to explain the correlation of the observed variables. Even two words that do not co-occur in a document may be conceptually related, as the method consists of searching for latent concepts that these words share by comparing all other words that are associated with these two words. For example if one paper uses Bayesian equilibrium and a second paper uses Nash equilibrium, the algorithm is able to identify that

[1] For an overview and technical considerations, see the papers of the 1998 special issue of *Discourse Processes* on Latent Semantic Analysis.

Table 2.1: List of Journals and Number of Papers

Journals	Obs. 1933-1937	Obs. 1963-1967
American Economic Review	138	114
American Journal of Sociology	206	197
American Sociological Review	236	239
Econometrica	109	183
Economic Journal	130	131
Economica	115	126
Journal of Political Economy	156	232
Quarterly Journal of Economics	168	174
Review of Economic Studies	99	125
Review of Economics and Statistics	107	192
Science and Society	56	56
Southern Economic Journal	54	147
Total	1589	1916

the concepts Bayesian and Nash are related since both relate to the concept of equilibrium. In this sense, correlations between words reflect shared latent ideas, such as common theoretical frameworks, methods, topics, and so forth. Words and documents are then projected into a semantic space based on their loadings in each dimension.

Formally, LSA works by running a Singular Value Decomposition on the term-document matrix M, which consists of an approximation of the original term-document matrix as the product of three matrices:

$$M \approx U_k \Sigma_k V_k^T$$

Where U is the matrix of the terms, V is the matrix of the documents, Σ is a diagonal matrix with the strength of the dimensions, and k is the number of dimensions. Therefore, it is a data-driven algorithm that approximates as close as possibly the original matrix; the elements in the diagonal of Σ are the latent factors that explain the correlations between words: these may be underlying concepts, topics, or methods that explain why words co-occur in the papers. In this sense, rather than predetermining a set of concepts or topics, the method uses the matrix decomposition to identify abstract dimensions that best fit the data, and the similarity of the papers represents their loadings in these dimensions.

Given the size of the sample I used 50 dimensions for the LSA and the term-document matrix was weighted using the Term Frequency–Inverse Document Frequency (TF-IDF) given by:

$$TF_{td}log(N/DF_t)$$

Where TF_{td} is the frequency of term t in document d, i.e. the total number of occurences of each term divided by the total number of words in each document. N is the total number of documents, and DF_t is the number of documents containing the term t. TF-IDF selects terms that are meaningful by penalizing terms that are common to too many papers, i.e. the highest is the number of documents in which a term appears the lowest is the weight given to it. Therefore the weight of a term increases as its frequency increases in a particular document, but it decreases the more common it is across documents.

Since each paper is a vector with 50 dimensions, I then calculated the cosine similarity between all papers. The cosine similarity between two papers is given by the dot product of the vectors over the product of their magnitude:

$$\frac{\mathbf{u} \cdot \mathbf{v}}{\mathbf{u}\mathbf{v}}$$

In order to compare how similar are the papers and journals of my sample and plot the journals in a two-dimension semantic space I reduced the dimensionality using Multidimensional Scalling (MDS). This was done by first computing the cosine similarity of all pairs of papers and then using MDS to reduce the matrix from 50 to 2 dimensions. By doing so all elements of language are kept until the cosine matrix is created and MDS only discards the inconsistent parts of the cosines. The documents went through the standard pre-processing procedures: numbers, punctuation, and stop words were removed, stemming was used to remove common suffixes, and all letters were converted to lower case. The code is available under request.

2.5 Results

The results of the Latent Semantic Analysis are presented in Figures 2.1 and 2.2. Each point in the semantic space is the average of all papers published in each journal. First, there is a considerable distance between the journals of sociology and the journals of economics, but the distance has decreased in the 1960s, possibly because the journals of sociology have started to use more quantitative methods. As argued by Porter (2003), the trust in numbers was a phenomenon that swept the social sciences in the United States after the Second World

War. The interdisciplinary Marxist journal *Science and Society* was already an outsider in the 1930s, suggesting that Marxism did not receive much space within economics. In the 1960s, as Figure 2.2 shows, the journal was further marginalized and its distance to the journals of economics increased relative to the 1930s. Indeed, Marxism became such a taboo that the distance between the journals of sociology and the journals of economics became smaller than the distance between *Sciency and Society* and the journals of economics.

The second result is that in the 1930s *Econometrica* was at the extreme right end of the economics journals, since it spoke a completely different language compared to the other journals, but in the 1960s the journal occupied a much more central position. In the core of the network one now finds the *American Economic Review*, *Econometrica*, the *Economic Journal*, the *Journal of Political Economy*, and the *Quarterly Journal of Economics*. Notice that excluding the *Economic Journal*, these are the journals that would later become the main journals of economics — the so-called top five. Hence, the origins of the top five journals of economics can be traced back to the 1960s, when four of these outlets had already become quite similar. My results suggest that the *Review of Economic Studies*, which would later become one of the top five, was not yet speaking quite the same language as the other four journals. In hindsight, it seems that during the 1960s the *Review of Economic Studies* was trying to push forward its own research agenda in the context of the Cambridge Controversy, but later the journal would abandon this project and converge to the mainstream.

Therefore, it is not so much that in the 1960s everyone spoke the same language; what was novel was that there was now *the* language. The movement from divergence to convergence is not a story of economics becoming a monolithic science without contest, for there will always be dissenters. What happened instead was that the prestigious journals and centers converged towards a paradigm in the 1960s. Mainstream economics had consolidated in the main departments and institutions (Harvard, Chicago, the American Economic Association and the Econometric Society), while competing approaches and methods could still be found in journals that were not in the center of the network. Nonetheless, after the formation of a consensual language in the elite institutions, the likelihood that alternative approaches could thrive became minimal. If heterodox economics is nowadays divided in a number of groups that barely communicate with each other, and even within marginalized fields one finds dissenters among the dissenters, the data show that the cohesiveness of neoclassical economics around core concepts was one of its distinguishing features that help to explain the success of mainstream economics.

Figure 2.1: Latent Semantic Analysis, 1933-1937

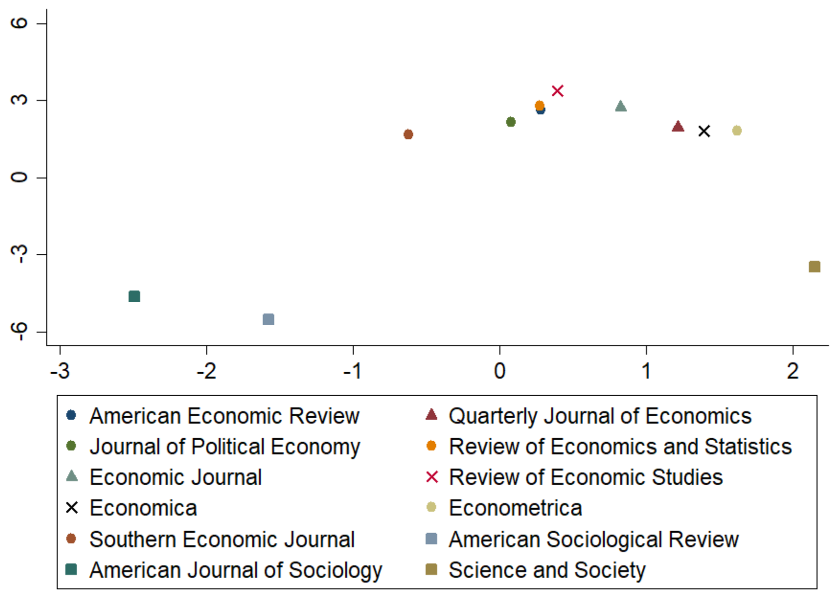
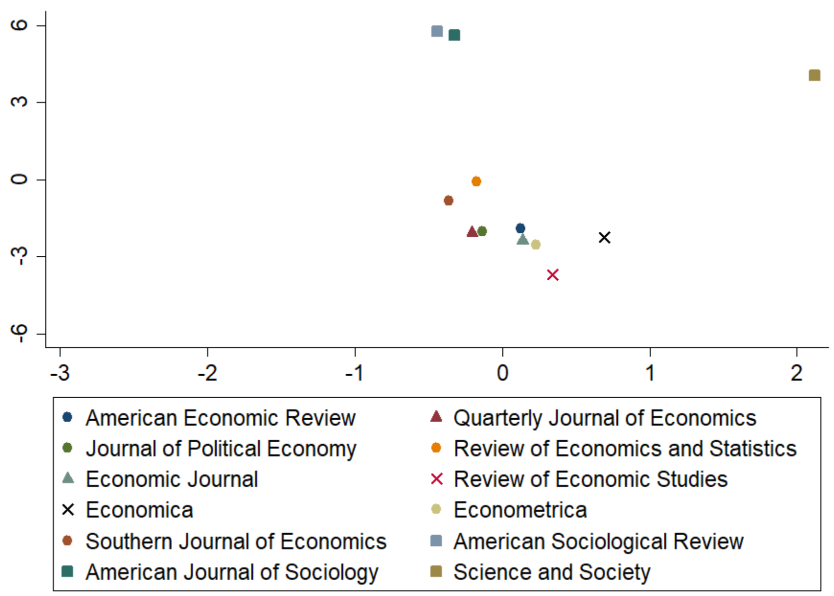


Figure 2.2: Latent Semantic Analysis, 1963-1967



That the Econometric Society had accomplished its project can be seen in Figures 2.3 and 2.4, which plot all papers published in the would-be top five journals. Not only are the papers more scattered in the 1930s, but the papers published in *Econometrica* are mostly in the right hand side of the plot. In the 1960s, on the other hand, the language used by papers published in *Econometrica* becomes very similar to that of the *American Economic Review*, the *Journal of Political Economy*, and the *Quarterly Journal of Economics*. One also notes that the papers published in *Review of Economic Studies* are somewhat out of the core, mostly on the lower part. Hence, the journal would only later follow the lead of the main journals.

Figure 2.3: Top 5 Journals of Economics, 1933-1937

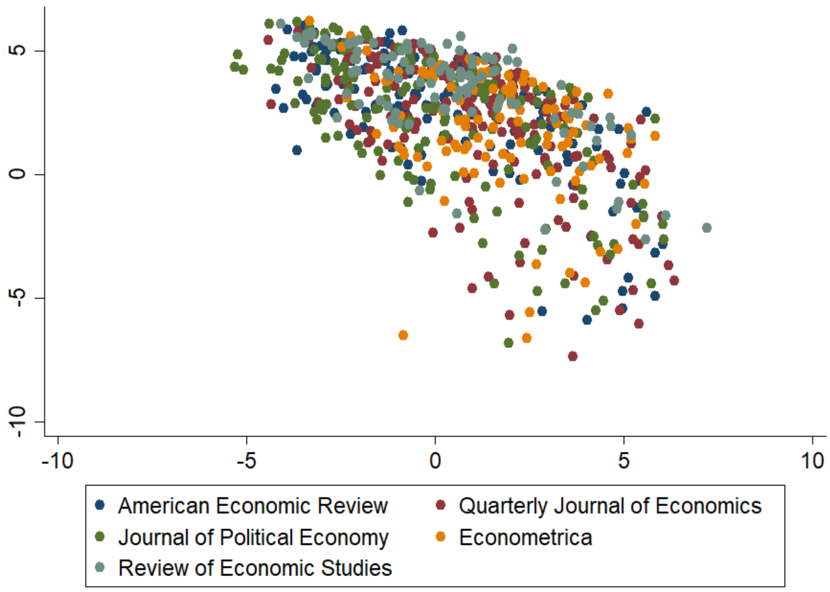
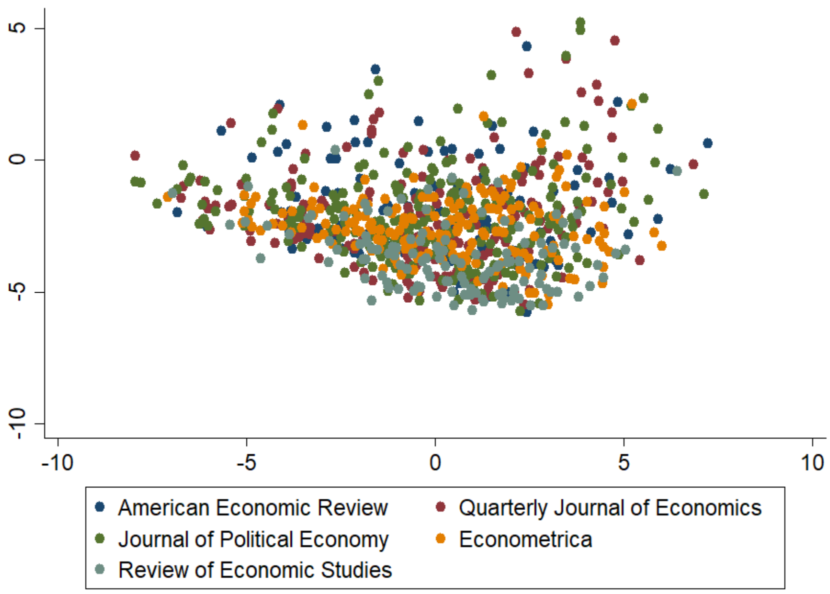


Figure 2.4: Top 5 Journals of Economics, 1963-1967



One could of course counter-argue that the convergence of the averages of the main journals does not necessarily mean the consolidation of a mainstream discourse, since it is possible that the variance of the papers has increased throughout the period. If this were true, then although the main journals had become more similar, the papers published in the main journals could have become more dissimilar, and it is even possible that in the 1960s a wider variety of topics were covered or that economics had become more diverse in terms of the methods it employs. That this is not the case is seen in Table 2.2. The variance of most journals of economics decreased during the period, and it decreased for the top five journals. Hence, not only have the top journals become more similar (excluding the *Review of Economic Studies*), but the papers published in each journal have also become more similar. There was a convergence both across and within the top journals.

Table 2.2: Variance of Papers by Journal

Journals	Obs. 1933-1937	Obs. 1963-1967
American Economic Review	13.65	11.13
American Journal of Sociology	14.46	11.30
American Sociological Review	10.40	13.62
Econometrica	9.73	7.95
Economic Journal	11.02	13.04
Economica	14.55	12.28
Journal of Political Economy	15.53	12.05
Quarterly Journal of Economics	12.64	12.59
Review of Economic Studies	9.76	7.16
Review of Economics and Statistics	8.68	18.98
Science and Society	11.67	12.62
Southern Economic Journal	12.90	16.23

Table 2.3 shows the euclidean distance between the average of each journal and the average of all papers in economics. Out of the nine journals, six have become closer to the average of all papers in the 1960s than they were in the 1930s. The only exceptions are the *Economic Journal*, which was the most central outlet in the 1930s and observed a slight increase in the 1960s, the *Review of Economic Studies* for their engagement in the Cambridge Controversy, and the *Review of Economics and Statistics*. The latter case is interesting because the outlet was (and still is) mostly focused on empirical research. Hence, its increasing euclidean distance is a signal that the main journals of economics were shifting towards greater emphasis in theoretical research. Moreover, the journal also observed the greatest increase in variance, which probably

reflects the easier access to data and computers in the 1960s and hence a wider variety of topics that could be explored.

Table 2.3: Euclidean distance between the average of economics journals and the average of all economics' papers

Journals	Obs. 1933-1937	Obs. 1963-1967
American Economic Review	0.50	0.09
Econometrica	1.06	0.68
Economic Journal	0.40	0.49
Economica	0.87	0.74
Journal of Political Economy	0.63	0.20
Quarterly Journal of Economics	0.65	0.28
Review of Economic Studies	1.09	1.79
Review of Economics and Statistics	0.61	1.81
Southern Economic Journal	1.45	1.12

In order to investigate in more detail how the language changed between 1933-1937 and 1963-1967, Table 2.4 contains the average number of appearances of each word per paper in the five journals at the core of the network, namely the *American Economic Review*, *Econometrica*, the *Economic Journal*, the *Journal of Political Economy*, and the *Quarterly Journal of Economics*. Since these journals would take a leading role in the dissemination of ideas after the Second World War, an analysis of what concepts were used in these journals sheds some light on the changing nature of economics throughout the period and on the distinguishing features of mainstream economics.

The observed changes in language during the period reflect both the socio-economic and the intellectual factors discussed in the previous section. While *bank*, *state*, and *money* figure at the top of the list in the 1930s, in the 1960s the most common words were *model*, *growth*, and *market*. Due to the crisis in the 1930s, words as *govern* and *public* figured high among economists' priorities, both losing importance in the second wave. In the 1960s, the core journals were much more concerned with quantitative methods as can be seen by the substantial increase in the use of words such as *model*, *variabl*, and *equat*. This becomes clear looking at column 5, which shows the variation of each word. Out of the 10 words that most increased throughout the period, only one (*growth*) does not refer to quantitative methods. During the period there was an increase in both formal research (*assum*, *optim*, *theorem*, *equilibrium*, and *hypothesis*), and empirical research (*tabl*, *data*, and *empir*).

Table 2.4: Average Occurrence of Words per Paper in the Core Journals

Words	Obs. 1933-1937	Words	Obs. 1963-1967	Words	Variation (p.p.)
bank	17,54	model	11,86	model	11,58
state	12,18	growth	11,44	growth	9,24
money	9,23	market	10,51	variabl	8,01
invest	8,81	variabl	10,05	equat	5,91
market	8,73	equat	9,95	assum	3,54
labor	8,19	invest	9,82	optim	3,31
govern	5,48	labor	9,82	tabl	2,93
polici	5,41	money	8,35	data	2,71
power	5,15	state	8,1	theorem	2,46
public	4,8	assum	7,47	equilibrium	2,23
law	4,64	tabl	7,43	hypothesi	1,87
tabl	4,5	polici	7,2	polici	1,79
equat	4,04	data	6,39	market	1,78
assum	3,93	equilibrium	5,21	labor	1,63
data	3,68	bank	4,66	empir	1,59
class	3,19	govern	4,61	welfar	1,54
worker	3,18	optim	3,45	aggreg	1,37
plan	3,14	plan	3,39	invest	1,01
equilibrium	2,98	public	3,31	wealth	0,61
monetari	2,68	monetari	3,16	keynesian	0,49
growth	2,2	aggreg	2,87	monetari	0,48
variabl	2,04	theorem	2,66	redistribute	0,32
histor	1,89	power	2,34	plan	0,25
regul	1,72	hypothesi	2,3	socialist	-0,1
aggreg	1,5	empir	2,18	imper	-0,17
wealth	1	welfar	1,96	capitalist	-0,29
capitalist	0,76	worker	1,82	histor	-0,68
empir	0,59	class	1,71	govern	-0,87
socialist	0,44	wealth	1,61	money	-0,88
hypothesi	0,43	law	1,36	regul	-1,15
welfar	0,42	histor	1,21	worker	-1,36
model	0,28	regul	0,57	class	-1,48
imper	0,23	redistribute	0,49	public	-1,49
theorem	0,2	keynesian	0,49	power	-2,81
redistribute	0,17	capitalist	0,47	law	-3,28
optim	0,14	socialist	0,34	state	-4,08
keynesian	0	imper	0,06	bank	-12,88

Although words such as *govern*, *regul*, *public*, and *state* were less common in the 1960s than in the 1930s, it is unclear whether or not economics became less applicable to policy matters because of the increase in empirical research. This ambiguity is further reflected by the increasing importance of growth theory and welfare economics during the period, and also by the growing use of words such as *polici*, *invest*, *wealth*, *keynesian*, *monetari* and *plan*.[2]

What is clear nonetheless is that there was a significant change in the nature of economics between the 1930s and the 1960s. Regardless of the extent to which academic papers oriented public policy, what changed during the period is that in order to publish in the main journals and command the respect of the profession arguments had to be framed using mathematical models and econometric methods. By subscribing to quantitative methods economists could claim impartiality and assume the role of experts. In this connection notice that the words *socialist*, *imper*, *capitalist*, *worker*, *class*, and *power* lost importance, which suggests that economists were toning down their discourse with respect to concepts that may be perceived as politically tinged. If this is indeed the case, one could then argue that even if economics became more abstract and less applicable to policy directly, it became more important to influence politics indirectly, since the mastering of the most recent techniques gave status to economists and this in turn led to positions in the government (e.g. as economic advisors).

Table 2.5 shows that the so-called interwar pluralism is also observable when looking at which economists were more debated. In the 1930s, references to economists of different persuasions were fairly distributed. Excluding Keynes, who is the most cited economist in the list, references to Walras, Marshall, Smith, Pareto, Commons, Hayek, Knight, Ricardo, Marx, Hicks, Jevons, Veblen, Mitchell, and Menger range in the interval between 0.14 and 0.71. This suggests that the influence of schools as diverse as Classical, Austrian, Institutional, and Neoclassical was roughly the same in the 1930s, with no group clearly dominating the debate. The picture for the 1960s is rather different; the main evidence that a consensus had emerged is that nearly all economists born before 1900 lost importance (except Schumpeter and Mitchell), and all economists born after 1900 became more important (except Keynes). Moreover, in the 1960s the top five economists were Friedman, Samuelson, Solow, von Neumann, and Arrow. Economists in the 1960s thus seemed less concerned with the history of the discipline and more focused in the

[2] It is out of the scope of this chapter to discuss the virtues and vices of the new theories developed during the period. The contentious debate about the relationship between mathematics and economics has a long history and goes on unabated. However I note in passing that since economic models often underlie empirical research, the usefulness of the latter stands or falls with the theories orienting the former.

development of mathematical economics. Notice that Commons, Hayek, Knight, Marx, Jevons, Veblen, and Menger were nearly forgotten ranging from 0.01 to 0.06 average mentions per paper in the 1960s. The number of references to Keynes, the most cited economist in the 1930s, decreased from 1.42 to 0.41. On the other hand, the word *Keynesian* gained currency during the period and became more common than *Keynes* in the 1960s (see Table 2.4), suggesting that with the neoclassical synthesis economists increasingly stopped reading and referring to Keynes' own thought and endorsed Samuelson's version of it.

Table 2.5: Average Mentions to Economists per Paper in the Core Journals

Words	Obs. 1933-1937		Obs. 1963-1967	Variation (p.p.)
Keynes	1.42	Friedman	1.09	+1.07
Walras	0.71	Samuelson	0.87	+0.87
Fisher	0.68	Solow	0.85	+0.85
Marshall	0.68	von Neumann	0.67	+0.67
Smith	0.56	Arrow	0.66	+0.66
Pareto	0.45	Leontief	0.51	+0.38
Commons	0.37	Marshall	0.47	-0.21
Hayek	0.34	Smith	0.44	-0.12
Knight	0.34	Keynes	0.41	-1.01
Jevons	0.31	Fisher	0.40	-0.28
Veblen	0.25	Pareto	0.38	-0.07
Ricardo	0.22	Hicks	0.35	+0.15
Marx	0.21	Modigliani	0.35	+0.35
Hicks	0.20	Schumpeter	0.32	+0.20
Mitchell	0.15	Walras	0.28	-0.43
Menger	0.14	Mitchell	0.19	+0.04
Leontief	0.13	Ricardo	0.13	-0.09
Schumpeter	0.12	Hayek	0.06	-0.28
Friedman	0.02	Knight	0.05	-0.29
Arrow	0	Marx	0.05	-0.16
Modigliani	0	Jevons	0.04	-0.27
Samuelson	0	Commons	0.02	-0.35
Solow	0	Menger	0.01	-0.13
von Neuman	0	Veblen	0.01	-0.24

2.6 Conclusion

After the Second World War the United States assumed a hegemonic position and economists around the world eventually followed the tenets of American economics. My contribution to the understanding of why quantitative methods became a central aspect of economics was to investigate how mathematical economics came to dominate the profession inside the United States in the first place. I have argued that the stabilization of mathematical economics after 1930 was a consequence of the ideological disputes taking place in Europe. American economists increasing use of quantitative methods was favored by their allegedly ideological neutrality, allowing them to cast arguments in a language that was perceived as rigorous and deprived of political content. The stabilization of mathematical economics happened during the golden age of the welfare state in America and at least until the 1970s mainstream economists saw an important role for the state. I have illustrated this with a brief discussion of the neoclassical synthesis and the development of welfare economics. The mathematization of economics was a historically bounded process, where the interaction of socio-economic circumstances and theoretical developments after 1930 led to the emergence of a paradigmatic core. Economic conditions improved after 1940, economists were summoned by the government to tackle military problems during the war, and the Cold War with its increasing ideological extremism and focus on science and mathematics provided further impetus to mathematical methods.

The results of the empirical analysis suggest that the rise of quantitative methods has not led to a significant change in what concerns economists engagement with public affairs. Notwithstanding considerable differences in the methods and topics of the interwar years relative to the postwar period, the consolidation of mainstream economics in the 1960s did not change economists' perception as to their role as technical experts that should orient public policy. Although some expressions such as *govern*, *regul*, *worker*, and *class* have become less prevalent in the 1960s relative to the 1930s, other expressions such as *polici*, *labor*, *welfare*, and *invest* have grown in importance. Therefore, as discussed in the first two sections and in the empirical exercise, what seems to have happened was that the methods and language used by economists changed as a response to the political climate.

Moreover, the results of the Latent Semantic Analysis suggest that there was a shift from divergence of ideas to convergence of beliefs between the 1930s and 1960s. While in the 1930s there was no observable core, by the 1960s the network of journals was organized as a core and a periphery. In the 1960s there were still competing approaches to the true nature of economics, but there was now a paradigmatic core with mathematics as the dominant language. That mathematical economics became the language of economics can be seen by looking at the position of *Econometrica* in the semantic space; between its foundation in 1933 and the 1960s it moved from the extreme right end of the economics journals to the core of the network. The empirical exercise indicates that it was not the case that economics as whole had converged by the 1960s and dissenters could no longer be found. What happened instead was the consolidation of a paradigm in the main institutions and departments making it nearly impossible for dissenters to advance their research programs. Already in the 1960s the would-be top five journals (with the exception of the *Review of Economic Studies*) had converged, endorsing the methods and theories championed by the members of the *Econometric Society*. Not only have these journals become more similar, but also the papers published in each of these journals became more alike. With the convergence of the top journals of economics to a common language, mainstream economists could make strong claims about what counts as economics proper, making it nearly impossible for heterodox approaches to thrive.

Chapter 3

From Modelmania to Datanomics: The Top Journals and the Quest for Formalization

The chapter uses bibliometric data from JSTOR and Web of Science to assess the rise of mathematical and quantitative methods in economics between 1940 and 2017. My claim is that the top journals of economics have impacted the formalization of economics, since the publication of new models and methods in these journals has positive externalities on the network of economics journals in the sense that it opens new avenues for research, such as new applications and extensions of the models and methods. Moreover, it is argued that the role of the top journals as gate-keepers of economic discourse is not only a possible explanation for the formalization of economics, but it also has implications for contemporary economics. More specifically, I compare three leading journals (*The American Economic Review*, *Journal of Political Economy* and *The Quarterly Journal of Economics*) with economics as a whole and show that the use of mathematical and quantitative methods has grown sharply between 1940 and 1980 and has remained constant thereafter. Analyzing separately theoretical and applied papers, the results indicate that while the proportion of applied papers has grown steadily in the three journals since 1955, the proportion of theoretical papers has grown sharply between 1940 and 1980, and has declined afterwards. Furthermore, I employ co-word analysis to investigate trends in economics between 1990 and 2017: using abstracts from fifteen journals, it is shown that the shift from theoretical to applied research has intensified in recent years.

3.1 Introduction

The formalization[1] of economics has been widely debated and it is commonly held that the Second World War was a watershed in the history of economic thought. The so-called "formalist revolution" (Blaug 2003), so the story goes, marks the transition *From Interwar Pluralism to Postwar Neoclassicism* (Morgan and Rutherford 1998). Others have approached the subject through an internalist perspective, highlighting different sides of this story. Ingrao and Israel (1990, 289) argue that the history of general equilibrium theory is continuous in terms of its core issues (existence, uniqueness, and stability) and that one should "speak of shifts rather than radical changes or turning points". Giocoli's (2003) reconstruction of the transformation of economics' image from a "system-of-forces" to a "system-of-relations", likewise, rejects the idea of a "formalist revolution", since this concept "brings with it the idea of a sudden modification, while the actual process took almost three-quarters of a century to complete" (2003, 6). Weintraub (2002) explains how changes taking place in mathematics since the early twentieth century played a significant role in the transformation of economics. Yet, others argue that the formalization of economics "was already well under way in late Victorian England" in the works of Jevons and Marshall (Schabas 1989, 60). A further point of contention is the relationship between ideology and neoclassical economics (Lawson 2012; O'Boyle and McDonough 2017; Milonakis 2017).

The list could go on, but my goal here is not to provide a comprehensive account of the formalization of economics. Instead, my argument is that regardless of the interplay between internal and contextual elements in the rise of mathematical and quantitative methods, there is yet another important side of this process, namely the role of economics journals in the dissemination of ideas. Hence, my goal is to offer an additional and complementary explanation to historians addressing the formalization of economics. Instead of focusing on the impact of specific authors (e.g., Hicks, Samuelson, Arrow, Debreu, von Neumann, Nash), institutions (e.g., Cowles Commission, RAND), or the political and economic context, which have certainly contributed to the phenomenon under investigation, I focus on a less discussed side of this

[1] I follow Backhouse's (1998) definition of formalization as comprising mathematization, axiomatization, and methodological formalism. I thus use formalization in a different sense than that associated with Hilbert's programme, i.e., I use it as a synonym of mathematical and quantitative methods since I address both theoretical and applied models and methods.

story, namely that ideas do not gain widespread acceptance only because they are brilliant or because there are patrons favoring them (Goldstein 1993). Hence, I assess the formalization of economics through the lenses of the sociology of the economics profession, focusing on the institutionalization of three top economics journals — *The American Economic Review* (AER), *Journal of Political Economy* (JPE), and *The Quarterly Journal of Economics* (QJE) — as gate-keepers of economic discourse and on their impact on the dispersion of mathematical and quantitative methods post-1940. Given the leading role of these three journals, I claim that an empirical investigation of the content of the papers published in these outlets may shed light on the formalization of economics as a whole. The top journals of economics influence research directly, since they are widely read and cited, but also indirectly, since publishing in these journals affects researchers' tenure and promotions (Oswald 2007; Card and DellaVigna 2013; Fourcade et al. 2015).[2]

My working hypothesis is that, given the prestige of these three top journals and the mutual influence they exert on one another, they have played a pivotal role in the formalization of economics. In particular, the co-evolution of formal content in these journals sheds some light on the idea that journals can be thought of as nodes in a network, with ideas traveling across space. In this sense, the publication of new models and methods in these three journals has positive externalities on the other journals in the network, leading to debates, extensions, refutations, and applications. The perniciousness of the top journals' hegemony as gate-keepers of economic discourse has the unfortunate implication that researchers more often than not ask themselves what arguments and methods are more likely to be accepted in the top journals, rather than what is the most relevant question and the most appropriate method to answer it. Hence, in looking to the future of economics, one must reflect on the extent to which the dominant position of few journals and institutions may hinder the emergence of novel ideas.

The main contributions of the chapter can be summarized as follows: Firstly, it is argued that the top journals were important players in the formalization of economics, and that this not only offers an explanation for the formalization of economics from a historical perspective, but also has implications for contemporary economics. Secondly, I compare the co-evolution of mathematical and quantitative methods in three main journals — AER, JPE, and QJE —

[2]As Morin (1966, 403) nicely put it, "theirs not to reason why, theirs but to write or die". According to him, George Stigler estimated that publishing in a top journal in the 1960s was worth between \$10,000 and \$20,000 in increased lifetime earnings.

vis-à-vis the economics profession as a whole for the period 1940-2010. Thirdly, recent trends in economics are discussed using co-word analysis to investigate the abstracts of fifteen leading journals in 1990 and 2017.

3.2 The Hegemony of the Top Journals of Economics

Recently there has been some debate on pluralism within economics and whether a process of "de-formalization" is underway. Economic theories are not conceived in a vacuuum, as politico-economic conditions influence the topics and methodologies of economists. For instance, "[i]n the 1920s and the 1930s, the common ground for intellectual confrontation (not excluding controversy) was a renewed interest in the economy's cyclical fluctuations" (Ingrao and Israel 1990, 223). As Goldstein (1993) explains, for ideas and interests to translate into political outcomes they must be "politically salient", in the sense that there are shared beliefs between the political community and its sponsors. Regarding Keynesianism, she argues that "the decision whether governments should adopt Keynesian policies in the 1930s or 1940s was not settled by objective facts" (1993, 2), but rather stemmed from the political community's imperative to respond to the economic turmoil of the 1930s.

However, the effect on the development of economic ideas of the economic and political context seems to be smaller nowadays due to the greater homogenization of the methodology of economics and the concentration of economic discourse in few journals and institutions. Although the current crisis has led to speculations whether the time is ripe for changes, Aigner et al. (2018) show that comparing the content of papers published before and after the crisis no significant change emerges, except for an increase of discussions regarding financial instability. Yet, even the explanations offered for such instability seem to rely on standard arguments, so much so that there has not been a paradigmatic development in recent years as was observed in the decade following the Great Depression:

> [T]he financial crisis and its consequences have, by and large, been rationalized with reference to existing theoretical concepts [...] the financial crisis is seen by economists as a major external shock, unforeseen because of the limits imposed on rational behavior by asymmetric information, and not as something intrinsic to the economic process (ibid., 18).

The absence of change in economics due to the recent crisis when compared to the Great Depression may be a consequence of the internal hierarchy of the economics profession and of how economists relate to fellow social scientists. As Fourcade et al. (2015) have argued, economists live in a bubble: dialogue with neighboring sciences is virtually absent, due to the economists' self-proclaimed superiority with respect to other fields, such as sociology and political science — a feeling fed by the very rise of formalism. Contrary to sociology and psychology, economics "is characterized by far-reaching scientific claims linked to the use of formal methods". Economics has risen to a dominant position in the hierarchy of the social sciences, and well-defined power relations exist both across the social sciences and within economics: "The authority exerted by the field's most powerful players, which fosters both intellectual cohesiveness and the active management of the discipline's internal affairs, has few equivalents elsewhere". These authors argue that after the Second World War the "intellectual trajectories of the social science disciplines have diverged importantly", with economics embracing mathematical and statistical methods. The "insularity" of economics is said to be a consequence of its epistemological differences when contrasted with other social sciences, two crucial factors being the economists' penchant for methodological individualism and formalism (ibid., 90-93).

Within economics, an explanation of the lack of innovation may be the "'oligopoly' of U.S. institutions dominating leading journals in economics and economics research throughout the world" (Hodgson and Rothman 1999, 172). These authors argue that such concentration may reduce diversity in approaches, and that although economists will often diverge on policy issues, there is much more agreement about "fundamental theoretical and methodological assumptions — such as utility-maximization and the ubiquitous, axiomatic-deductive method". Taking an evolutionary perspective of this process of concentration, they claim that "it may be difficult for further change to take place. 'Lock-in' may occur, where specific institutions defend specific, and possibly outdated, ideas and approaches". Although some level of concentration of personnel and resources is beneficial to innovation because of institutional scale, extreme levels of concentration as currently experienced in economics journals hamper the development of new ideas (ibid., 180-183).

The idea that path-dependency helps to understand why economics is locked-in a paradigmatic core has also been discussed by Dobusch and Kapeller (2009). They identify a process of path formation in the immediate post-World War II which was open-ended, in the sense that multiple equilibria could emerge. Yet, a number of individual contributors (Popper, Hayek, Friedman, Samuelson, Arrow, and Debreu), but also the Mont Pelerin Society, helped to stir economics away from the pluralism that characterized interwar economics. These authors discuss a number of "mechanisms and amplifiers" that help to understand the "interplay between the subject matter and the institutional and social structure of the scientific community in economics" (ibid., 877), highlighting how citation metrics and the higher unity of mainstream economics when compared with heterodox economics may lead to a lock-in. They argue that this dynamic "has become institutionalized in the 1960s onward by 'objective' quantitative measures, like citation indexes" and that, as a consequence, the highest ranked journals "are very easily able to reproduce their top position, leading to a stable cluster of journals that mutually refer to one another and make it quasi impossible for new or dissident journals to succeed" (ibid., 881).

As argued by Kapeller (2010), citation metrics shape the perception of economists about the quality of papers and act as a "self-fulfilling prophecy leading to a scientific elite, which is able to reproduce its position via the mechanics of citation ranking". It is a self-reinforcing mechanism in that papers that are published in top journals or by an author who is often cited are perceived a priori as being of high quality, and, as a consequence, such papers tend to be often cited thereby increasing the perception that they are indeed high-quality papers. Citation metrics thus operate as a "conservation mechanism within science, but also have a societal role by indirectly influencing the public discourse and thus making them a *hegemonial device*" (2010, 331-332). The author has shown that roughly half of the citations of the top thirteen heterodox journals are to orthodox journals. Thus, ironically, although many of these citations are critical of the ideas of mainstream economics, they end up boosting up the citations (and consequently the rankings and the hegemony) of the very journals and ideas they intend to defeat. Citation metrics are decisive in tenure, promotions, and the distribution of resources among departments. They may be hazardous to the development of new ideas because they overestimate the quality of the dominant institutions due to the more fragmentary nature of heterodox economics with its many schools, which reduces the number of citations to heterodox journals, not to mention the fact that heterodox economists tend to cite older papers than orthodox economists (which also negatively affects the ranking of heterodox journals).

What is at stake is not only that "to the victor go the spoil", but also that citation metrics favors the idea that any publicity is good publicity. If the top journals are gate-keepers of economic discourse and influence the direction of future research, one might be skeptical whether the current crisis and recent pleas for pluralism may lead to considerable changes in the economics profession. Indeed, this concern has been recently voiced by Akerlof (2017) in his panel address to the ASSA, when a section was dedicated to the top five journals:[3]

> What I am worried about most of all is what we don't see. So, I am worried about the analysis that is never seen, that never becomes a paper. And it doesn't become a paper, because it can't become a paper. And it can't become a paper, because that's not what a paper in economics is all about.

That the ASSA dedicated a section where prominent economists have debated the "the curse of the top five" is highly symptomatic of the negative effects of the economics journals hierarchy. Whether or not they will continue to engage in the debate and help to bring the issue to the fore remains to be seen. The point though is that historically the top journals have been important players in molding economic discourse and that the rise of mathematical and quantitative methods cannot be understood without taking into account the communication function of economics journals.

3.3 The Formalization of Economics, the Top Journals, and their Editors

Several papers have used citation analysis to investigate the concentration of economics journals and some of them have discussed the formalization of economics, the idea that journals form a communication network, and the extent to which editors influence the content published in the journals. This section overviews these issues since they provide some insight into the formalization of economics after the Second World War and complement my empirical analysis. The literature has documented the increasing importance of empirical and mathematical methods, especially the former. The focus is usually on empirical papers, rather than papers that use econometrics, and there is no comprehensive work that addresses the formalization of economics using citation analysis; most of the literature is restricted to surveying a small sample of published material, and none of the papers examines the whole period from 1940 to 2010.

[3] https://www.aeaweb.org/conference/2017/preliminary/%202153?page=8per-page=50. The so-called "top five" are AER, JPE, QJE, *Econometrica* (Ecmca), and *Review of Economic Studies* (RES).

Moreover, as far as I am aware no research exists on the specific role of the top journals as key players for the formalization of economics.

3.3.1 The Rise of Empirical and Mathematical Methods

In 1970 empirical methods were on the rise and, at the time, one third of the most cited economists were econometricians, while in previous decades the proportion of econometricians among the most cited economists was much smaller (Quandt 1976). Figlio (1994) has shown that whereas in the 1970s the top five journals published less empirical papers than journals ranked between sixth and tenth place, this gap has closed between the 1970s and the 1990s, when AER, JPE, and QJE started to publish more empirical papers than the bottom half of the top ten journals. One of the explanations offered for the increasing use of empirical methods is "the relationship between theoretical paradigm shifts and publishing patterns" (Figlio 1994, 185), in the sense that the development of a number of macroeconomic theories in the 1970s and 1980s may have led to several attempts to test such theories. The point raised by the author is important because it highlights that theoretical models may have spillovers over empirical applications that I have not taken into account. Therefore, while I address the evolution of theoretical and applied papers separately, they are not completely independent. Further research would be needed to investigate whether the development of theoretical models have influenced the rise of applied research.[4]

Aigner et al. (2018), analyzing the abstracts of the top 560 cited papers between 2001 and 2013, notice that the term *model**[5] appears on average approximately once in each abstract and that this frequency has increased after 2008. Moreover, they argue that empirical methods are growing in importance given that terms like *theor** and *equilibri** have either stagnated or declined after 2008, while the occurrence of terms such as *data*, *estimat**, and *test** has increased. Kim et al. (2006) have investigated papers with over 500 citations, showing a clear picture of the rise of econometrics, which accounted for 10% of highly cited papers between 1970-1974 and 22.9% in 1995-1999 if one classifies the papers according to their JEL primary code. Assessing

[4] The effect of empirical evidence upon theoretical models is likely to be less important. Historically, empirical evidence has had little effect in driving the content of theoretical models, yet one cannot rule out that there might have been some instances in which empirical evidence not in accordance with the results of theoretical models led to the formulation of new theories that could rationalize such findings. As pointed out by Milgrom and Roberts (1987, 195), "no mere fact ever was a match in economics for a consistent theory".

[5] The symbol " * " is a wildcard operator that captures any word derived from *model*.

the main contribution of these papers, instead of their JEL primary code, the proportion of theoretical (empirical) papers fell (rose) from 76.7% (13.3%) in 1970-1974 to 11.4% (60%) in 1995-1999.

Hamermesh (2013) classified over seven hundred papers published in AER, JPE, and QJE between 1963 and 2011. While the proportion of theoretical papers fell from 50.7% to 19.1% between 1963 and 2011, the proportion of empirical papers (using either borrowed or self-generated data) has increased from 47.8% to 63.9%. The most thorough investigation of the rise of empirical methods is Angrist et al. (2017). They analyze over one hundred thousand papers between 1980 and 2015 and show that not only did empirical papers become more common, but they have also grown in importance considering the outlets in which they are published and the share of citations they reap.

Card and DellaVigna (2013) argue that there has been a decrease in the impact of papers that are mainly theoretical and econometrical after 1990, while papers in international and development economics and macroeconomics have gained momentum. Hence, while papers in econometrics (say a paper discussing the properties of an estimator) have decreased in importance, the opposite happened to papers using econometrics (applied research). Comparing empirical and theoretical works, Johnston et al. (2013) have shown that the former are more cited than the latter, and this may be a further explanation for the increasing use of empirical methods.

Stigler et al. (1995) investigate the role of journals in scholarly communication and the increasing use of formal methods between 1892 and 1990. Looking at the highest level of technique in five journals,[6] there has not been much change between 1892 and 1922, with roughly 95% of the papers being classified as primarily verbal. This figure drops to 80% in 1932-3, and continuously falls throughout the following decades, such that by 1962-3 one third of the papers were primarily verbal and by 1989-1990 more than 90% employed either algebra, econometrics, calculus, or more advanced techniques. These authors recognize the difficulty in classifying papers according to the technique employed, but argue that "[n]o faults of classification, however, could conceal the enormous movement toward mathematics in recent decades" (ibid., 342).

Fourcade et al. (2015) have also documented "the dramatic rise of economics' engagement

[6]AER, Ecmca, JPE, QJE, and *Review of Economics and Statistics* (REStat)

with mathematics and statistics in the post-World War II period". By looking at the proportion of extra-disciplinary citations in the top five journals, they show that developments taking place outside of economics are also an important explanation of its formalization (ibid., 102). Kosnik (2015), likewise, documents that *model* is the most common word in articles' corpus and highlights the increasing importance of mathematical methods and of the microfoundations literature in seven top journals from 1960 to 2010.[7] Using JEL codes, Kelly and Bruestle (2011) have shown that mathematical and quantitative methods and microeconomics account for approximately 37% of the publications in the eight leading journals,[8] and that this proportion has remained fairly stable between 1970 and 2007, while these two areas represented nearly 17% of the papers in economics as a whole in the 1970s and 13% in 2000-2007. The proportion of papers in mathematical and quantitative methods in the eight leading journals is more than twice the proportion of these papers in economics as a whole, which is further evidence that the main papers in this field tend to published in the top journals.

3.3.2 The Top Journals

A fine example along my line of inquiry is Eagly's (1975) study titled "Economics Journals as a Communications Network". Eagly argues that information exchange can be thought of as an "idea industry" with "vital importance for the progress of the discipline". Instead of focusing on the importance of individuals as "lighting-rods for the discipline, serving as liaison between the gods and mere mortals, or electric eels, serving to maintain the alertness of fellow economists who are swimming in the same waters", one should approach the production of ideas from a sociological standpoint because of the division of labour and interdependence of practitioners: "Information flows at many different levels must be taken into account" (1975, 878). The author analyzes a network of 18 journals in 1961-4 and in 1970-1, highlighting the growing importance of American journals during the 1960s, when AER, JPE, and QJE were the three most central journals in the network. Using a measure to estimate the prestige of journals, he finds that QJE ranks first and AER ranks third in both waves. Moreover, QJE and AER have the higher "sending-receiving ratios" (the number of times a journal is cited compared to how many times it cites other journals), meaning that they are feeders of the network since this is a measure of

[7]She uses the top five journals plus the *Journal of Economic Literature* and the *Journal of Economic Perspectives*

[8]The top five plus REStat, *International Economic Review*, and *Journal of Economic Theory*.

"the journal's innovative role as a wellspring of seminal ideas in the discipline" (1975, 880).

Although "major theoretical contributions appear occasionally in journals outside the central core of the discipline" (Stigler et al. 1995, 334), it has been widely documented in the literature that the top journals reap a significant portion of citations even though they account for a small fraction of all papers. AER, JPE, and QJE "are the three most highly regarded general journals in the profession" and, although there are a number of methodologies to evaluate the prestige of journals, these three invariably figure at the top of the list (Wu 2007, 59). Quandt (1976), likewise, has shown that in 1970 between 16% and 24% of the references in AER, JPE and QJE were to one another and that AER was the most prestigious journal.

While the share of publications from US/Canada-based authors in these three journals has decreased from 92% between 1963 and 1993 to 83% in 2003-2011 (Hamermesh 2013, 164), the "Americanization of the economics literature" is very much alive (Eagly 1975, 884). As an example of "the extent to which the US has become the center of economic research since World War II", among the 27 core journals of economics in 1986 only one was not published in English and roughly half of the citations to the core journals were concentrated in five journals: AER, Ecmca, JPE, RES, and REStat (Diamond 1989, 3-4).[9] After 1990, QJE has grown considerably in importance, becoming the leader both in terms of median number of citations and considering the ratio of citations per paper, followed by AER and JPE (Card and DellaVigna 2013).

The most comprehensive study of concentration is Glötzl and Aigner (2017), who have shown that economics is very concentrated in terms of articles, journals, regions, institutions, authors, and paradigms. They report that the Gini coefficient of citations to articles has increased from 36.5 to 69.2 between 1956 and 2016, and that the Gini coefficient of citations to journals is even more concentrated, increasing from 67.9 to 85 in the same period. Moreover, the top five journals received 27.6% of all citations and published 71% of the top 100 articles between 1956 and 2016. The top five reached their peak in the early 1970s, when they reaped nearly half of all citations, and this proportion has been steadily declining. Yet, it is striking that in 2016, although papers in the top five amounted to 2% of all papers, they still received roughly 22% of all citations. Moreover, while the number of journals has increased from 40 to 675 between 1956 and 2016, the top five journals' share of the top 100 articles has remained around 70% during

[9]The Diamond's list has been criticized by Burton and Phimister (1995) and Hodgson and Rothman (1999), who argue that citations are a crude measure of quality since there are many features that should be accounted for such as percentage of self-citations and their distribution over time. Though there are significant differences between their lists and Diamond's, the predominance of US journals is still observable.

the whole period. On the geographical dimension, 49% of the papers were published in North America and these papers account for 73.5% of all citations between 1980 and 2014, with 18 out of the 20 most cited institutions being located in the US. Furthermore, while the top 10 and top 100 authors have on average, respectively, 114.6 and 74 citations per article, which accounts for 3.6% and 15% of all citations, roughly one third of all papers have zero citations.

Aigner et al. (2018) similarly show that among the 560 most cited papers between 2001 and 2013, 81.1% have their origin in the USA and 63.3% of these citations are to the top five journals. The prestige of these journals can be seen by their high number of citations; their median number of citations was around 200 in the period between 1990 and 2000, and AER, JPE, and QJE were the most cited journals (Card and DellaVigna 2013). Kim et al. (2006) list 146 papers written after 1970 with more than 500 citations, showing that in the 1970s and 1980s the main outlets were Ecmca (21.4%), JPE (12.4%), and AER (14.4%) and in the 1990s they were QJE (21.4%), JPE (15.7%), the *Journal of Finance* (14.3%), Ecmca (12.9%), and AER (8.6%). Overall, roughly 40% of these papers were published in AER, JPE, and QJE. Furthermore, 85% of the papers were written by researchers working in the USA.

Another issue is the concentration of affiliations of authors who published in the top journals. Some institutions feature more often than others. Siegfried (1994), analyzing the same journals as I do, has shown that between 1950 and 1989 there was a decrease in such concentration, i.e. in the share of papers published by authors affiliated with the four institutions that most often appear as affiliation in the pages of AER, JPE, and QJE.[10] However, Wu (2007) has updated these results for the period 2000-2003, showing a reversal in this trend. Hence, not only is there a highly skewed distribution of citations favoring the top journals, but also a high concentration of authors affiliated with the top institutions.

3.3.3 The Role of Editors

A further issue in the formalization of economics concerns the role of editors in favoring particular lines of research. As an example, the editorship of John Davis of the AER between 1911 and 1940 may have delayed the formalization of the journal. Likewise, Keynes' dislike for econometrics may have affected the content of papers published in the *Economic Journal* while he was editor (Stigler et al. 1995, 344).

[10] In the 1980s these were MIT, Princeton, Chicago, and Harvard for AER, Chicago, Stanford, MIT and Harvard for JPE, and Harvard, Princeton, MIT and Stanford for QJE.

Analyzing the content of papers in five journals[11] between 1886 and 1959, Coats 1971 observed that original differences between editorial lines faded through time, with journals becoming more homogeneous regarding the distribution of contents. His results suggest that "editors were subject to forces beyond their control; that far from being dynamic academic entrepreneurs of the Schumpeterian type, they were merely passive recipients of a changing flow of manuscripts over which they exercised little or no editorial influence" (ibid., 32). Yet, he notes that there is not sufficient information available to judge the extent to which editors may be considered "'gate-keepers' of the science in any but a passive sense of that expression. Nor can we readily discover what, if any, have been the effects of changes in the composition and functions of editorial boards" (ibid., 39).[12] Hodgson and Rothman (1999), likewise, note that to evaluate whether or not an editorial bias exists would require a comparison of rejected and accepted papers; since no such data is available, it is difficult to know the extent to which editors influence the content of publications. Nonetheless, they express concern about the high level of institutional concentration of editors and authors.

This issue has been more recently discussed by Colussi (2018), who showed that 43% of the papers published in AER, Ecmca, JPE, and QJE between 2000 and 2006 were written by scholars connected to at least one editor of the journal. This is not to say that there is favoritism, for, he argues, an alternative explanation is that top universities attract more talented and productive students, who in turn tend to become even more productive due to interactions with like-minded researchers. Be it as it may, there is little doubt that the network of economists has become a small world network (Goyal et al. 2006).

Laband and Piette (1994) have argued that although editors may accept papers that would not be published otherwise due to their connections with authors, overall these relationships help editors to choose high quality papers. They thus do not discard that favoritism may happen in some cases, but their main claim is that, due to competition among editors for high quality papers, editors use their network of relationships to gain information about high impact papers, their main goal being to publish such papers rather than favor people from their network.

Even conceding that editors exert some influence on the content of publications, it may

[11] AER, JPE, QJE, *Economica*, and *The Economic Journal*

[12] For a more detailed investigation of Dewey's, Homan's, and Haley's editorships of AER see Coats (1969). The author argues that in spite of Dewey's desire to differentiate the outlet from JPE and QJE, there were no significant differences in these journals during his editorship. With respect to Homan, he notes that it is hard to estimate his accomplishments, given the "rapid post-war growth and rising standards of the economics profession" (1969, 63)

be argued that they are more likely to affect the topics that are chosen and who gets to be published, rather than the methods used. Moreover, it is difficult to disentangle whether editors are key actors in shaping the contents of papers, or if the very choice of who becomes the editor of a journal reflects lines of research that are gaining importance. Hence, the long run trends discussed in the present chapter should be seen as part of a broader phenomenon:

> [T]he pattern of home bias in top economics journals, together with the stability of rankings of top departments, is not just a coincidence of geography and authors, but stems instead from a particular form of social organization and control (Fourcade et al. 2015, 100).

Regardless of the inconclusive evidence on the issue of bias and the influence of editors in the publishing process, it should be clear from the discussion thus far that there is an excessive concentration of power in the top journals.

3.4 Data and Methods

3.4.1 Data

Annual data from JSTOR are used for the period 1940 to 2010. Differently from other databases, JSTOR has the advantage that it covers the 1940s; however it contains a very small amount of papers published after 2010. The sample consists of 230,033 papers, out of which 16,300 were published in *The American Economic Review* (AER), *Journal of Political Economy* (JPE), and *The Quarterly Journal of Economics* (QJE).[13] Reviews, rejoinders, comments and such like were excluded. Table 3.1 presents the number of papers in each journal by decade. Although JSTOR does not list all journals of economics, its database is quite large and taking together all journals other than the top three offers a fairly close description of the economics profession as a whole.

I have chosen to analyze AER, JPE, and QJE, instead of the top five, because Ecmca mostly publishes formal papers since its foundation and because in the 1940s mathematical economists formed a small and isolated community. In this sense, it was the general journals who were mainly responsible for dispersing ideas to wider audiences since they were more accessible and

[13] AER's *Papers and Proceedings* is also included since JSTOR does not distinguish between AER's journal and AER's *Papers and Proceedings*.

Table 3.1: Article Count by Decade

	1940s	1950s	1960s	1970s	1980s	1990s	2000s
American Economic Review	867	782	925	1463	1616	1598	1992
Journal of Political Economy	330	349	549	904	660	528	445
Quarterly Journal of Economics	347	410	500	543	567	464	461
Other Journals	6474	10126	17491	30706	40548	49296	59092

more widely read. As to RES, I opted for not including it on account of the Americanization of the economics profession. It is not clear without further research the extent to which RES communicated with the top three American journals, even though ideas certainly crossed the Atlantic.

To further investigate recent trends in economics, abstracts from fifteen journals were collected from JSTOR, Web of Science, and manually from the journals' websites. Conference editions and special issues were excluded to avoid overestimating the importance of topics discussed in such issues. The criterion to choose the journals was to select the highest ranked journals according to IDEAS for which abstracts were available in 1990 and 2017, listed on Table 3.2. The journals were divided in general and field journals.

Table 3.2: List of Journals, IDEAS Ranking, and Number of Abstracts

Journals	Ranking	Obs. 1990	Obs. 2017
General Journals			
Quarterly Journal of Economics	1	38	40
Journal of Political Economy	2	54	44
American Economic Review	3	55	115
Econometrica	6	52	62
Review of Economic Studies	8	40	51
Field Journals			
Journal of Financial Economics	4	22	121
Journal of Finance	7	70	63
Journal of Monetary Economics	10	40	33
Journal of Econometrics	12	30	83
The Review of Financial Studies	13	17	111
The Review of Economics and Statistics	14	97	72
Journal of International Economics	15	40	86
Journal of Labor Economics	16	22	28
Journal of Public Economics	18	48	119
Journal of Development Economics	21	52	71

3.4.2 Methods

The exercise consists of two steps. First I compute the proportion of papers that use mathematical and quantitative methods from 1940 to 2010 in AER, JPE, QJE, and for the rest of the journals listed on JSTOR combined. Secondly, co-word analysis is employed using data from the journals listed on Table 3.2 for the years 1990 and 2017 in order to assess recent trends in economics.

In the first step, papers are classified as "formal" if they use mathematical and quantitative methods. Formal papers are divided into theoretical and applied papers. A paper is classified as theoretical if it uses mathematics, but not econometrics, and it is classified as applied if it uses econometrics, regardless of the extent to which it draws on economic theory.[14]

To compute the proportion of applied papers, I select all articles that contain at least one of the following expressions: *panel data* , *time series*, or *cross section*, as well as those that use the words *data** and *regress**. Given that several papers use data, but not necessarily econometrics (especially in the 1940s and 1950s, when there was a significant number of empirical works, but econometrics was still unusual), and since the word *regress** can have different connotations, I require that both words be used in order to reduce the number of articles erroneously classified as applied. The symbol " * " is a wildcard operator that allows for truncation. For example, writing *model** in the search engine yields the number of papers that use words such as *models*, *modeling*, *modeled*, etc. As a proxy to estimate the proportion of theoretical papers, I select all articles that use either the word *model** or *equation**, but do not use any of the words selected in the previous step, i.e., I select all papers that use models or equations but do not use econometrics.

A potential objection to this approach is the risk of false drops, i.e., a paper that uses one of the expressions but is not necessary theoretical or applied, however my results are very close to other papers that have investigated similar journals.[15] Furthermore, I have cross-checked

[14]The expression "applied" has multiple meanings and one may argue that an econometrical paper may not necessarily be applied if it is not based on economic theory. Conversely, a theoretical paper (say a paper in game theory) may be defined as applied if it has policy implications. For an introduction to the history of the concept "applied economics" see Backhouse and Biddle (2000). However, I follow the customary distinction between applied and theoretical depending on whether or not a paper uses econometrics.

[15]Although Stigler et al. (1995), Backhouse (1998), and Hamermesh (2013) do not include AER's papers and proceedings, and the former also includes Ecmca and REStat, my results are very close to theirs. Backhouse (1998) reports an increase from 20% to 40% in the proportion of papers that use mathematics (but not econometrics) between 1940 and 1960, which is the same result I have found. Since Hamermesh (2013) classifies papers as empirical (thus it also includes papers that use data, but not econometrics), my results are virtually identical

by adding a number of words to ensure that the choice of expressions is a good proxy for the phenomenon under investigation, and the results only changed marginally. Adding the words *optimiz**, *theorem*, *nash* and *general equilibrium* to the query to capture theoretical papers and adding *structural equation(s)*, *simultaneous equation(s)*, *least squares*, *estimator*, *cointegration*, and *maximum likelihood* to capture applied papers only changes the results by roughly 2 p.p. for the period as a whole.

In the second step I use co-word analysis to assess the centrality of the three journals in the network and to further investigate recent trends in economics by analyzing what words appear more often in abstracts in two waves: 1990 and 2017.[16] Since in the first step all papers using econometrics are classified as applied regardless of the extent to which they draw on economic theory, it is not clear whether the importance of theory has been growing or declining once one takes into account that applied papers also use theory. Therefore, I investigate the abstracts of the fifteen journals listed on Table 3.2.

Using the VOS (visualization of similarities) mapping technique,[17] co-word maps were built for 1990 and 2017. The size of the words is determined by the number of documents in which they appear, and there is a link between two words if they co-occur in a document. Since I use a distance-based map rather than graph-based map, the distance between two words reflects how strong is the link between these two words, where the strength of the link is determined by the frequency of documents in which they co-occur. Furthermore, the position of words is determined by their relatedness with all the other words in the map. In this sense, the centrality of a word is a measure of its importance, since the more often a word appears in conjunction with all the other words, the more central is its position in the map. Hence, both the size and the position of words are measures of their importance and they capture different attributes. The map also uses clustering techniques to group words based on their relatedness.

to his for the years 1993, 2003, and 2011, but lower for the years 1963, 1973, and 1983. This is not surprising considering that in the earlier years there was a high number of empirical (but not econometrical) papers, while in the last three decades it has become less common to find empirical papers that do not use econometrics. One may object that AER's *Papers and Proceedings* should have been excluded; however as a check I have deleted all papers using the expression *papers and proceedings*, which also drops papers published in AER that cite papers from the special edition, and the results only changed by roughly 1 p.p.

[16] Generic words like *paper* and *study* were excluded, and a number of words were replaced such as *equilibria* by *equilibrium*, *empirical framework* by *empirical*, *regressor* by *regression*, *subgame perfect equilibria* by *game*, etc. Furthermore, the words *quasi natural experiment*, *regression discontinuity design* and *natural experiment* were replaced by *experiment*; hence experiment refers both to actual experiments and quasi-experiments, since both methods are equivalent as far as my argument is concerned, i.e., these methods usually do not draw on economic theory to a large extent. The thesaurus file with all modifications is available upon request.

[17] For details about technical implementations of VOSviewer see van Eck and Waltman (2007, 2010)

More specifically, the maps are constructed applying VOS to a similarity matrix, which is a co-occurence matrix normalized by the total number of co-ocorrences of words. The similarity matrix is normalized using the association strength a_{ij} of words i and j $(i \neq j)$ given by

$$a_{ij} = \frac{mc_{ij}}{c_{ii}c_{jj}}$$

where m is the number of documents, c_{ij} is the number of documents in which words i and j co-occur, and c_{ii} and c_{jj} are the number of occurrences of i and j. Considering that the weighted sum of the squared Euclidean distances between all pairs of concepts is given by

$$E(\mathbf{x}_1 ... \mathbf{x}_n) = \sum_{i<j} a_{ij} \|\mathbf{x}_i - \mathbf{x}_j\|^2$$

Where the vector $\mathbf{x}_i = (x_{i1}, x_{i2})$ denotes the location of word i and $\|\cdot\|$ is the Euclidean norm. The position of the words in the map is then determined by minimizing their Euclidean distances subject to the constraint

$$\frac{1}{n(n-1)} \sum_{i<j} \|\mathbf{x}_i - \mathbf{x}_j\| = 1$$

3.5 Results

3.5.1 The Top Journals and the Quest for Formalization

Figure 3.1 shows the proportion of formal papers taking the top three journals together. There is a sharp increase in the proportion of formal papers between 1940 and 1980, such that by 1976 this proportion reached 90%, remaining stable thereafter. Furthermore, the figure also shows that while theoretical papers were much more common than applied papers in the first decades, since 1960 applied papers have been growing faster than theoretical ones, increasing their share among publications. Indeed, in the early 1990s the proportion of papers using econometrics becomes larger than the proportion of theoretical papers, which has been falling since the early 1980s. While in 1940 only 28% of the papers were formal, and there were roughly twice as many theoretical (19%) than applied papers (9%), by 1955 the proportion of theoretical papers more than doubled (55%) while the proportion of applied papers nearly did not change. After 1955, however, the importance of econometrics steadily increases reaching its highest value in 2010 (69%), while a mild growth is observed among theoretical papers between 1955 and 1983, when it reaches its peak (60%), and, after 1983 its importance has been continuously decreasing, with its level in 2010 (30%) returning to its 1948 level. Therefore, theoretical papers follow an inverted-U trajectory while applied research exhibits a positive trend for the whole period.

Figure 3.1: Formalization in the Top 3 Journals by Applied and Theoretical (%)

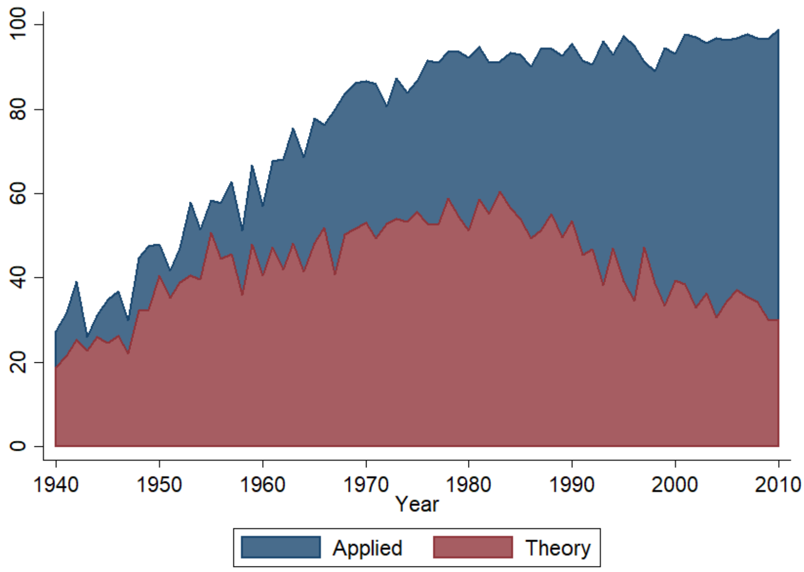


Figure 3.2 shows the level of formalization in each of the three journals, while Figures 3.3 and 3.4 show, respectively, the proportion of theoretical and applied papers in these outlets. The same pattern discussed for the top three journals has been observed in each of the journals. Indeed, the series are remarkably similar and this is an indication that the journals mirror one another, i.e., that they communicate.

The most important differences concern QJE. As can be seen in Figures 3.3 and 3.4, QJE was the journal with the highest proportion of theoretical papers for most of the period under analysis. In fact, by 1983 84% of the material published in this journal made no recourse to econometrics. However, in the 1980s there was a sharp increase in applied research and a pronounced decline in the proportion of theoretical papers. Indeed, QJE lagged behind AER and JPE until the late 1980s, when it became the journal with the highest proportion of applied papers. Interestingly, this observed increase in the trend's slope coincides with its consolidation as the most important journal; between 1985 and 1995 QJE moved from forth to first place in the ranking of journals (Card and DellaVigna 2013).

Figure 3.2: Formal Papers by Journal, (%)

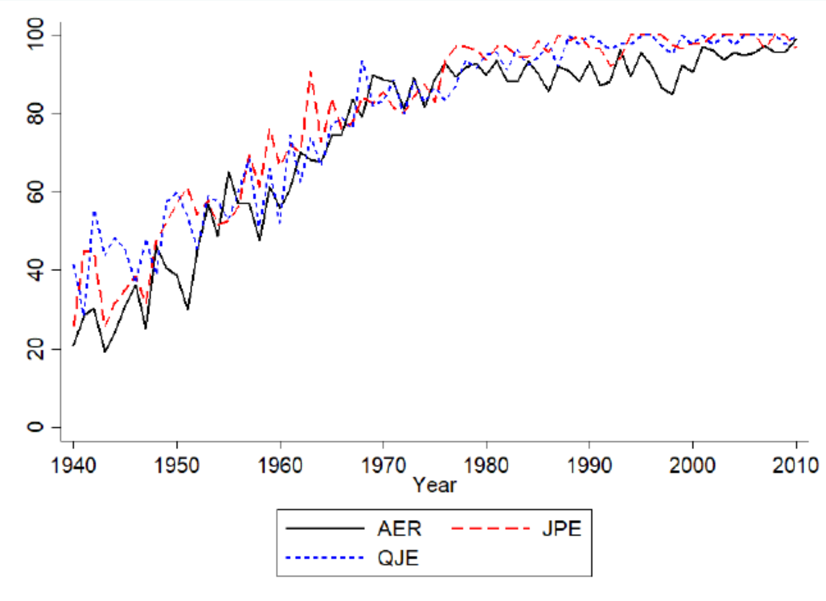
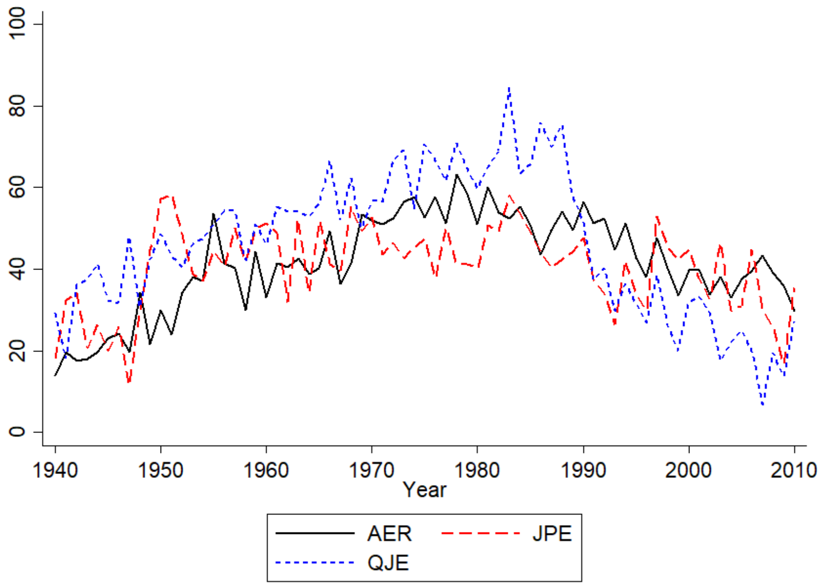
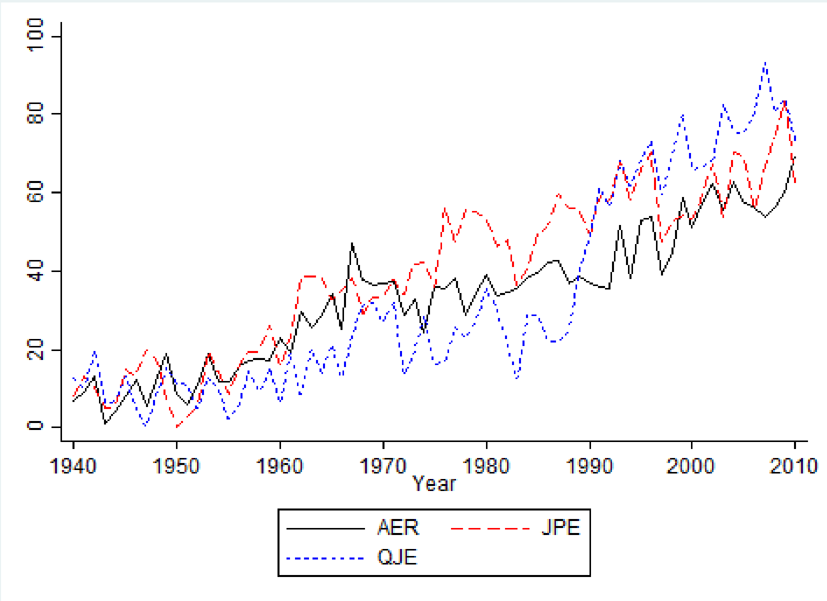


Figure 3.3: Theoretical papers by Journal (%)



Comparing my results with Kim et al. (2006), one sees a clear relationship between the proportion of applied papers and the number of citations received by a journal. These authors have shown that while in the early 1970s AER and JPE led the list of journals that published highly cited papers, reaping respectively 20% and 23.3% of papers with over 500 citations, in the early 1990s the proportions were AER (5.7%), JPE (25.7%), and QJE (20%), and after 1995 QJE assumed the first position. My results help to explain their findings: computations show that while in 1970 the proportion of econometric papers in AER, JPE and QJE was,

Figure 3.4: Applied Papers by Journal (%)



respectively, 37%, 33%, and 27%, by 1990 this figure changed to 37%, 49%, and 48%; moreover, in 1995 QJE became the journal with the highest econometrical content. Therefore, considering that empirical papers are more cited than theoretical ones (Johnston et al. 2013), QJE's rapid ascension in the ranking of journals in the late 1980s is consistent with the sharp increase of its share of applied works.

My analysis also corroborate a common narrative of the history of United States postwar economics in terms of Harvard/MIT versus Chicago, with Cambridge mostly as theoretical leader and Chicago (for whom theory already existed, namely price theory) mostly focused in finding empirical evidence to counter Cambridge ideas. My results can be read along these lines, as they show that the Harvard-based QJE was somewhat more theoretical than the other outlets, while the incidence of applied work was higher in the Chicago-based JPE. Of course alternative narratives are possible, which are still to be written.

The fact that the top journals move in unison indicates that the explanations offered in the literature do not wholly explain the phenomenon under investigation. As important as the Cold War might have been, for example, it leaves unexplained what is the mechanism leading to the quick and virtually identical adoption of formal methods in the top three journals, while the same process happened much slower when considering all journals of economics. As shown in Figures 3.5 and 3.6, the economics profession as a whole lagged behind the top three journals, but, ultimately, followed the same trend. My interpretation is that there are spillovers between the top journals and that ideas quickly spread in the journals at the core of the network of journals; with some delay, these ideas also spread to the network of economics journals as a whole.

For the economics profession as a whole, the share of applied papers only becomes higher than theoretical papers in the early 2000s, roughly ten years later than in the top three journals. Moreover, although the share of theoretical papers is flat since the 1960s and the inverted-U shape observable for the top three is not so clear for economics as a whole, it should be noted that the share of theoretical papers decreased from 39% to 34% between 2006 and 2010. Hence, it seems that the same trend observed for the top journals is happening in economics as a whole, albeit with some delay. It seems plausible to conjecture that the top journals lead the process and the other journals follow them, but that it takes some time for ideas to travel from the core of the network to journals that are not in the core, one possible explanation being that it takes time for people that do not publish in the core journals to learn the new methods.

Figure 3.5: Formal Papers, Top 3 and Other Journals (%)

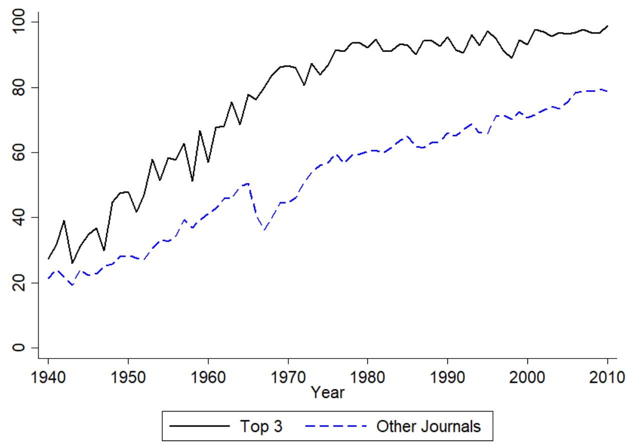
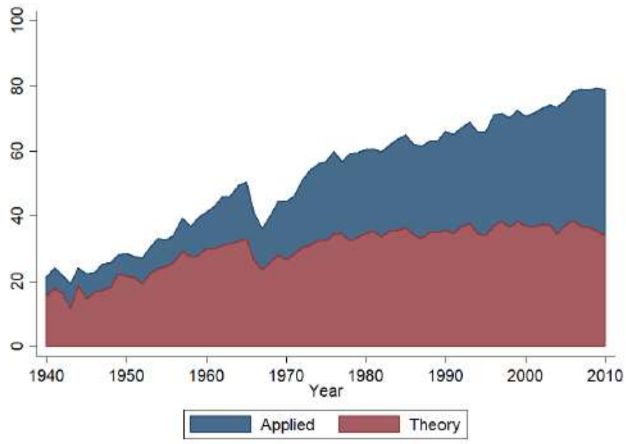


Figure 3.7 shows the citation practices of the 19 journals in 2017.[18] The figure illustrates the centrality that AER, JPE, and QJE occupy in the network. The leaders in citations are AER (4175), *Journal of Finance* (2898), Ecmca (2670), QJE (2349), *Journal of Financial Economics* (2192), and JPE (1908). Though one may counter-argue that AER publishes a higher number of papers than most other outlets and that papers have a tendency to cite more often papers from the outlet where they are published, even excluding AER from the dataset the journal still leads the ranking with 3557 citations. On the other hand, when *Journal of Finance*, *Journal of*

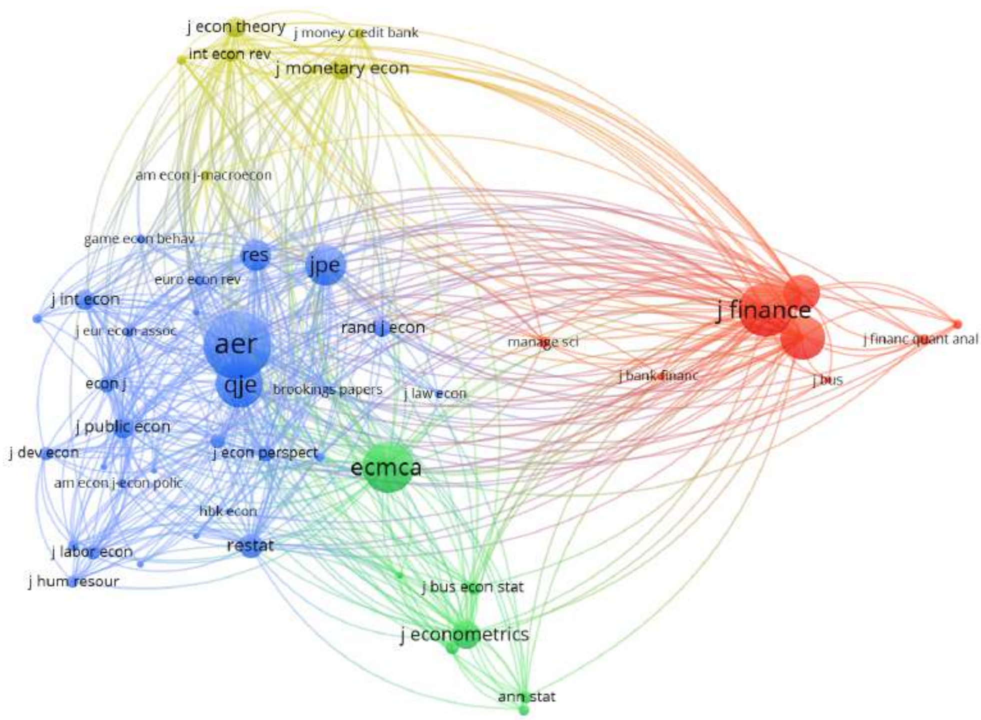
[18]The 15 journals listed in Table 3.2 plus *The Journal of Human Resources*, *Journal of Economic Literature*, *Journal of Applied Econometrics*, and *The RAND Journal of Economics*

Figure 3.6: Formalization in Economics Journals by Applied and Theoretical (%)



Financial Economics, and *The Review of Financial Studies* are excluded from the database, their number of citations reduces to less than 500. Therefore, although financial journals are highly cited, they are mostly cited by like-minded outlets and they are not as influential as the core journals.

Figure 3.7: Citation Network of 19 journals, 2017



Considering that journals constitute a communication network where each journal is a node, and since AER, JPE, and QJE form the core of this network, the increasing use of mathematical and quantitative methods in each of these journals is likely to have had positive externalities on the rest of the network. Therefore, from the perspective of the sociology of the economics profession, the leading role of these three outlets may help to explain why some ideas got accepted by the scientific community and others did not. If knowledge is socially constructed, and the terms of debate are negotiated by researchers, than the top journals are an important forum where conversations take place, if not *the* most important locus. It was through making their appearance in the top three journals, I conjecture, that ideas that once belonged to a small community of mathematical economists (mostly those active in the Econometric Society and its journal *Econometrica*) reached larger audiences.

Likewise, the top journals seem to have played an important role in the recent increase in applied work at the expense of theoretical work, e.g., the rise of quasi-experiments and experiments after 1990. By 1990 the top journals still relied heavily on theory, as will be seen in the next subsection, while field journals made much more extensive use of econometrics. Still, a possible explanation of the rise of "datanomics" after 1990 is that quasi-experimental methods were legitimized as a central tool in the economist's toolkit by the circumstance that some of the would-be seminal papers using such methods were published in the top three journals (e.g., Angrist and Krueger 1991; Card and Krueger 1994; Angrist and Lavy 1999). These editorial choices brought to the fore a new way to approach econometrics that draws much less on economic theory. In this connection, it should be noted that Ashenfelter, who advised David Card and Angrist and was one of the first promoters of quasi-experimental methods, was the editor of AER starting in 1985 (Panhans and Singleton 2017), while Esther Duflo is currently the editor of AER. Moreover, QJE went through changes in its board of editors in the 1980s, which helps to explain the increase in applied work in that outlet.

3.5.2 Recent Trends in Economics (1990-2017)

Table 3.3 summarizes the main findings from comparing the abstracts of the fifteen journals listed in Table 3.2 in 1990 and 2017. The ten concepts included in the table are related to either theoretical or applied works, which is the main focus of this paper, while words that can be used both in theoretical or applied papers (e.g., *cost* and *price*) are not included in the table, but are included in the co-word maps. Overall, one notes that terms such as *data*, *effect*, *estimation*, and *impact* have become much more common, while terms as *model*, *equilibrium*, *theory*, and *behavior* have lost importance between 1990 and 2017. Looking at the general journals, in 1990 six out of the ten terms listed are typical of theoretical research, namely *equilibrium*, *behavior*, *game*, *theory*, *agent*, and *choice*, while in 2017 only *preference*, *agent*, and *equilibrium* are associated to economic theory. Moreover, *equilibrium* has dropped from the second position to penultimate, while *effect* and *data* have increased their importance by roughly 10 p.p.. In 1990, *impact*, *policy*, and *estimation* were not among the list of 10 relevant concepts, while in 2017 they were used in, respectively, 14%, 12%, and 11% of the abstracts. Looking at field journals, although they are much more empirical than the general journals in both waves, one notes a similar trend. The proportion of abstracts using the term *model* has decreased between 1990 and 2017, while terms associated with applied research have gained importance in recent decades. Moreover, the proportion of abstracts using the term *policy* has increased from 8% to 13% between 1990 and 2017.

Table 3.3: 10 Relevant Concepts, 1990 and 2017 (%)

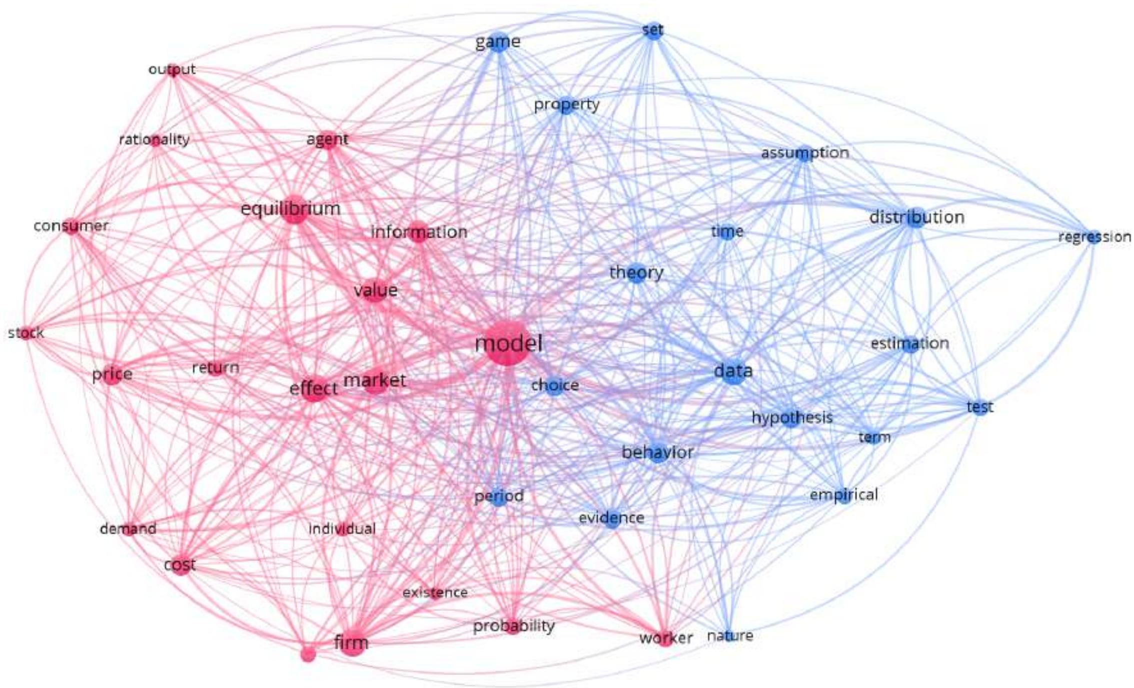
General Journals				Field Journals			
1990		2017		1990		2017	
Model	50	Model	47	Model	43	Model	37
Equilibrium	19	Effect	27	Effect	27	Effect	33
Effect	18	Data	27	Data	24	Data	31
Data	16	Impact	14	Evidence	13	Evidence	22
Behavior	11	Preference	13	Test	11	Country	14
Game	11	Policy	12	Country	10	Impact	14
Theory	11	Agent	12	Estimation	9	Risk	13
Agent	10	Estimation	11	Stock	8	Policy	13
Choice	9	Equilibrium	11	Empirical	8	Estimation	12
Evidence	9	Evidence	11	Hypothesis	8	Shock	12

Figures 3.8, 3.9, 3.10, and 3.11 present the co-word maps with approximately the 40 most common expressions. Apart from the more general remark about the increasing use of words related to applied works relative to words with a theoretical connotation, the position of the words in the map sheds much light on the changing use of data in recent decades. In the four maps, the red cluster contains most of the theoretical terms, while most of the terms associated to applied research are in the blue cluster. Note that in the general journals in 1990 (Figure 3.8), the blue cluster contains words associated both with theoretical works (e.g. *theory* and *behavior*) and with applied research (e.g. *empirical* and *regression*). Moreover, the word *data* is in the center of the blue cluster, and it is quite close to the terms *hypothesis*, *theory*, and *behavior*. Therefore, in 1990 there was a strong connection between applied work and economic

theory in the general journals. Apart from that, the red cluster mostly contains words that reflect economic theory (e.g., *rationality*, *equilibrium*, *agent*).

In 2017 the picture is rather different (Figure 3.9). Now, the blue cluster where *data* is located does not contain any expression related to theoretical research. This does not mean that applied research is completely divorced from economic theory, for there are links between expressions in the blue and the red clusters, and one can observe some terms in the red cluster such as *application*, *empirical*, and *estimation*, but it does indicate a substantial reduction in the relationship between theory and applied research. Since the distance of the terms is determined by their relatedness, the fact that in 2017 the blue cluster does not contain any term that is clearly associated with theory, whereas in 1990 there were a number of theoretical concepts in the blue cluster, indicates that applied papers draw much less on economic theory in 2017 relative to 1990. Moreover, while in 1990 the word *choice* was quite central, in 2017 the word *policy* occupies a central position in the map, and the word *experiment* appears in the 2017 map, but not in the 1990 map.

Figure 3.8: General Journals 1990



Looking at field journals (Figures 3.10 and 3.11), one sees that although theoretical terms are much less prevalent than applied ones, similar changes can be observed. The most striking difference is that while in 1990 the terms *data* and *model* belonged to the same cluster, in 2017

Figure 3.9: General Journals 2017

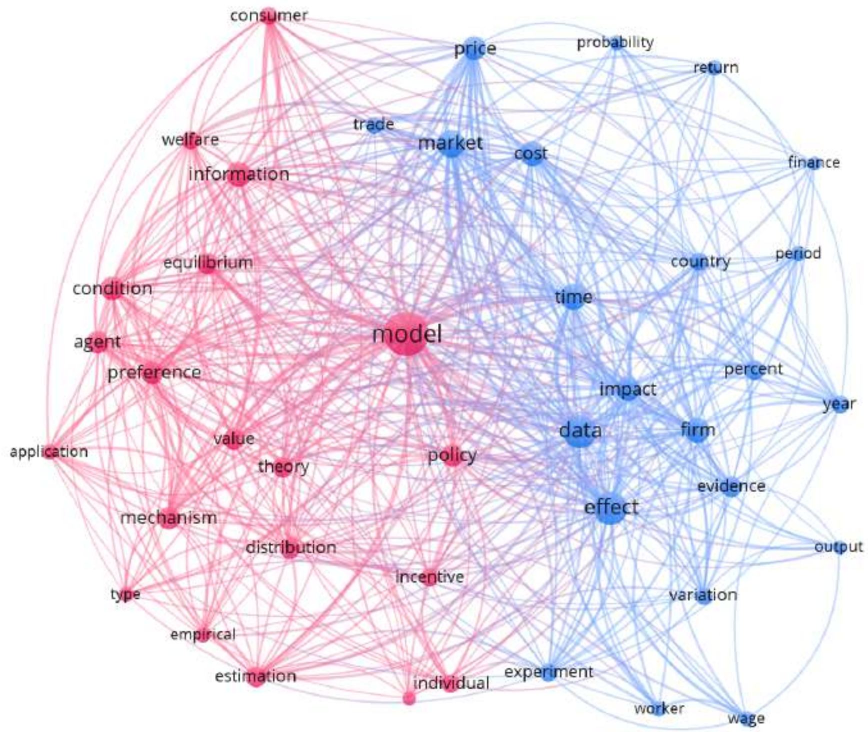
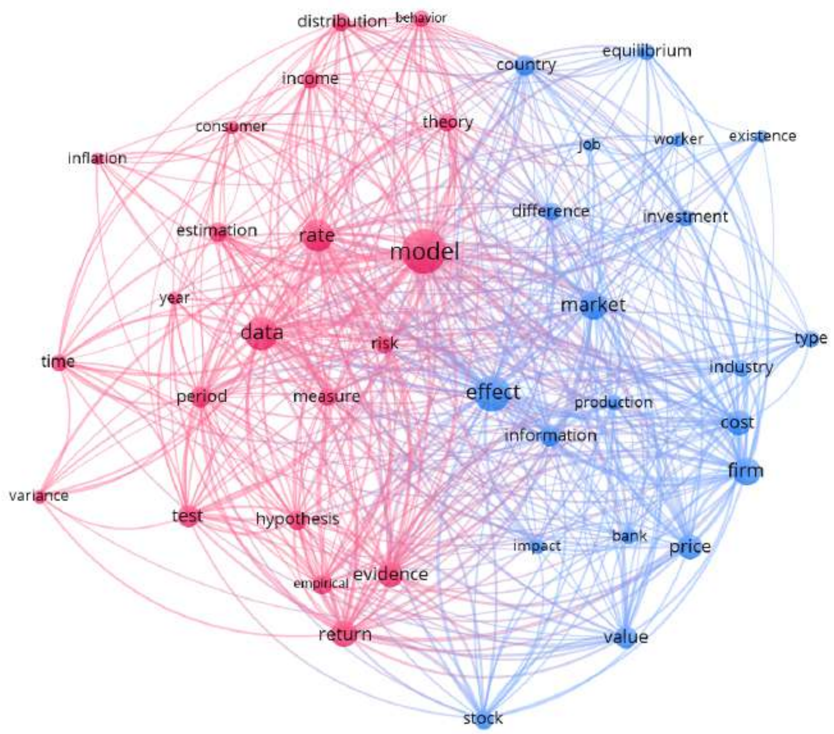
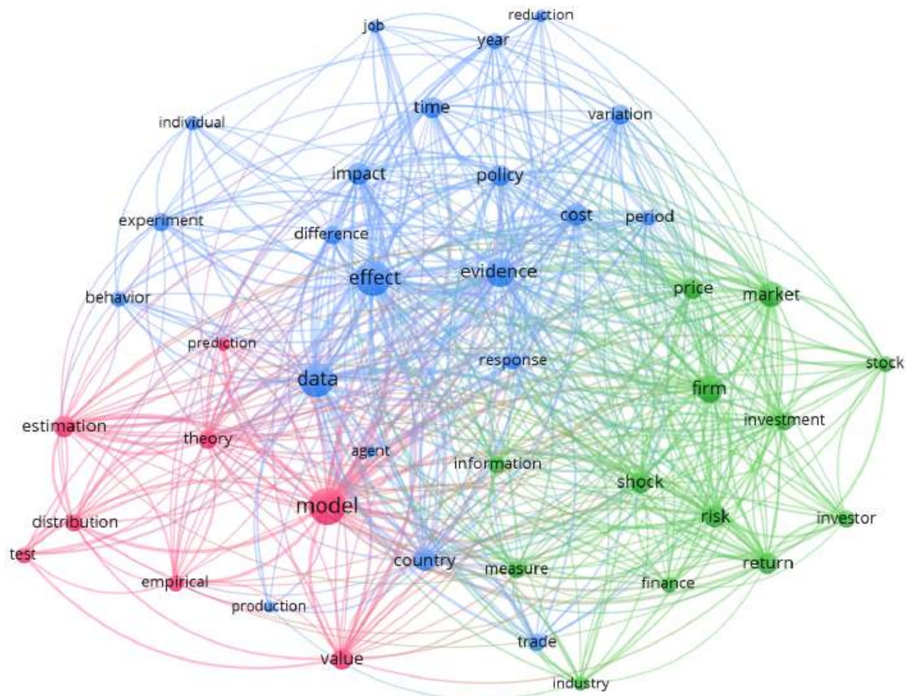


Figure 3.10: Applied Journals 1990



the words are no longer in the same cluster, which clearly points at a decreasing relationship between applied work and theory. Moreover, the word *model* occupies a less central position

Figure 3.11: Applied Journals 2017



in 2017, while the words *evidence* and *data* have grown in importance, both in terms of their frequency and their centrality. The word *equilibrium* appears in the map in 1990, but not in 2017, while the words *policy* and *experiment* can be seen in the latter, but not in the former. Also note that the words *behavior* and *individual*, which are typical of microeconomics parlance, are much closer to the word *experiment* in the 2017 map than to the word *model*, which suggests that individual behavior has been more often studied empirically than analytically. Finally, in 2017 there is also a green cluster, which reflects the increase in the number of papers published by financial journals. Given that papers in finance use a different vocabulary than other journals in economics, words such as *shock*, *risk*, and *return* are grouped in a separate cluster in 2017. These changes indicate that although field journals traditionally relied much less on theory than general journals, they have also drawn much less on economic theory in 2017 relative to 1990. The comparison between general and field journals reveals that both groups are considerably more homogeneous in 2017 than they were in 1990, hence the decline of theory and the rise of applied methods is observable across the journals under investigation.

3.6 Whither Economics?

In the last decade there has been much talk about the so-called "empirical turn" in economics. Like every proclaimed "turn" or "revolution", the expression suggests an unwarranted paradigmatic revolution *à la* Kuhn that is hardly identifiable in any period of the history of economics. As my results suggest, there has been a steady increase in the proportion of econometric papers for many decades rather than an abrupt change. Likewise, there has been a considerable increase in theoretical models in the aftermath of the World War II, but the process was more gradual than the expression "formalist revolution" suggests.

The idea of an empirical turn is problematic, moreover, because it ignores that economics was already quite empirical before the rise of mathematical and quantitative methods. In the 1940s, for instance, nearly half of the papers in the three journals used the word *data*, and in the interwar period there was a wealth of empirical work. Thus, despite the increasing use of econometrics in recent decades, one should keep in mind the changing uses of data throughout the twentieth century. [19]

The changing character of applied economics involves too many elements to be summarized here. Yet, one important point that complements my account is that within econometrics there has been a change from models to methods after 1990 (Panhans and Singleton 2017). As these authors show, there has been a paradigmatic shift from the Cowles econometric approach to quasi-experimental methods, which has grown considerably in importance since 1990. This shift, they explain, has its origins in the disarray of the simulataneous equations approach in the 1980s, with its contested reliance on economic theories based on the optimization of agents or firms. The larger availability of panel data and longitudinal surveys from the 1960s onward was a necessary but not sufficient explanation of this shift, and the changing character of applied economics must account for "a confluence of trends that fed into the stabilization of the 'credibility revolution'", such as the suitability of quasi-experimental methods "to meet the demands of patrons of economic research, particularly policymakers" due to their intelligibility (ibid., 131). In the 1950s, Marshak, Koopmans, and Haavelmo made important contributions in general economic theory and theoretical econometrics, advancing considerably Frisch's and

[19]This issue has been thoroughly debated in a recent issue of *History of Political Economy* edited by Backhouse and Cherrier (2017) entitled *The Age of the Applied Economist: The Transformation of Economics since the 1970s*. Practitioners, likewise, have discussed the limitations and advantages of Randomized Controlled Trials and the changing role of economic theory, see Deaton and Cartwright (2017) and Jackson (Forthcoming)

Tinbergen's program defined by the "subordination of measurement to model building and academic priorities" (ibid., 150). Hence, the early decades of econometrics were marked by a close connection between economic theory and econometrics. In quasi-experimental methods, on the other hand, economic theory takes a much less important role, but still their outcomes have economic implications and may orient public policy:

> [Q]uasi-experimental design represents a subtle but significant reorientation of the role of economic theory in applied work on two levels. Rather than objects belonging in an economic model, the question itself is framed around a specific historical intervention, and the intervention replaces the role of a model in the empirical practice (ibid., 145).

Stafford (1986) has shown that the proportion of empirical papers (in labor economics) with a meaningful theoretical section has nearly doubled between 1965 and 1983, hence not only have theoretical papers grown in importance in the aftermath of the Second World War, but there has also been an increasing use of theoretical models within econometrics. Nonetheless, as shown by Biddle and Hamermesh (2017), the proportion of microeconomic applied papers in the top five journals that draw on economic theory rose from approximately 40% in 1951-1955 to 80% in the period from 1973 to 1977, but in the subsequent period this figure fell to approximately 60% in 2007-2008. Thus, while there is a larger proportion of theoretical models underlying applied papers nowadays than in the early 1950s, the extent to which theoretical models are used in econometrical papers has fallen considerably in recent decades. "If the typical thesis of the eighties was an elaborate piece of price theory estimated by non-linear maximum likelihood on a very small number of observations," remarks Deaton (2007), "the typical thesis of today uses little or no theory, much simpler econometrics, and hundreds of thousands of observations". My results point in the same direction, with applied works becoming increasingly more detached from theory after 1990. Thus, not only theoretical papers have become less common, but also their importance in orienting applied research has decreased after 1990.

If the 1940s and 1950s mark the rise of formalization, with Koopmans (1947) exemplifying the new attitude of economists, so that in the early 1970s Leijonhufvud (1973, 328) rightly (and ironically) pointed out that "status is only to be achieved by making 'modls'", in the 1980s it was measurement which was on a clear ascent.[20] Thus, in a letter to *Science*, Leontief (1982,

[20]AER's 1972 editorial captures the dissatisfaction of readers of the journal as to the prevalence of theoretical

104-107) complained of economists'

> irresistible predilection for deductive reasoning [...] economic journals are filled with mathematical formulas leading the reader from sets of more or less plausible but entirely arbitrary assumptions to precisely stated but irrelevant theoretical conclusions.

To him, not only mathematical economics fell short of practical relevance but also econometrics, which relied heavily on aggregate data that were of little use "to advance, in any perceptible way a systematic understanding of the structure and the operations of a real economic system", while "masses of concrete, detailed information contained in technical journals, reports of engineering firms, and private marketing organizations are neglected". He was critical of the excessive deductivism of economics and skeptical of the possibilities of interdisciplinary research. The "splendid isolation in which academic economics now finds itself", he argued, was likely to continue

> as long as tenured members of leading economics departments continue to exercise tight control over the training, promotion, and research activities of their younger faculty members and, by means of peer review, of the senior members as well. The methods used to maintain intellectual discipline in this country's most influential economics departments can occasionally remind one of those employed by the Marines to maintain discipline on Parris Island.

This brings me back to the main point of the paper. The hegemony of the top journals and their effect on shaping economic discourse is not only an element that helps to explain the formalization of economics from a historical perspective, but it has unfortunate implications for the future of economics. Researchers should not ask themselves what kind of argument and method is more likely to be accepted in the top journals before choosing the topic they deem more relevant or interesting. The pernicious incentives provided by the hierarchy of journals may hinder the emergence of novel ideas. Whether or not one should welcome the shift from theory to application is a matter of debate, and ultimately of how one understands the role of

articles: "Articles on mathematical economics and the finer points of economic theory occupy a much more prominent place than ever before, while articles of a more empirical, policy-oriented, or problem-solving character seem to appear less frequently". The editor argued that this tendency was not due to the journal's preference for such articles (Borts 1972).

the economist. Yet, the most relevant question is not whether the changes in recent decades in economics are beneficial, instead what is at stake is what are the drivers of these changes. In this sense, if my argument that the top journals are gate-keepers of economic discourse is accepted, then there is much to be questioned about why should a small number of journals have so much power in determining what is acceptable as legitimate practices of economists.

3.7 Conclusion

Debreu began his presidential address to the American Economic Association claiming that as "the Second World War was drawing near its resolution, economic theory entered a phase of intensive mathematization that profoundly transformed our profession" (Debreu 1991, 1). To him, economics benefited from this process by becoming "open to an efficient scrutiny for logical errors". This happened by incorporating new tools developed by mathematicians, such as convex analysis, fixed point theory, and the theory of integration and of nonstandard analysis. By becoming more abstract, economists could solve some long-standing riddles, such as the integrability problem and consumer choice under uncertainty. Yet, and perhaps surprisingly, he ended his presidential address by muddying the water as to the desirability of this process: "Ceteris paribus, one cannot prefer less to more rigor, lesser to greater generality, or complexity to simplicity; but other things are not equal, and in the estimate of many members of our Association the cost of that mathematization sometimes outweighs its benefit" (1991, 3-5). Thus, he argues, any evaluation of the pros and cons of the formalization of economics requires understanding how and why it happened.

The answer to such question obviously involves a number of elements and historians of economic thought have widely debated this issue. Hopefully, this chapter has contributed to this investigation by shedding some light on one aspect of this process. As I have tried to show, there is an institutional side of this story that should be accounted for: the formalization of economics benefited from the vehicles that promoted the rise of such ideas. In this sense, the most important journals form the core of a communication network and they influence the diffusion of certain ideas throughout the whole network of the economics profession. The co-evolution of formal content in the three journals I have investigated is remarkably similar, in terms of both theoretical and applied works, which suggests that ideas quickly spread in the core of the network. Comparing the three journals with the other economics journals, one notes that the top journals lead the process, and that the rest of the network follows the same pattern, albeit with some delay. Moreover, using co-word analysis I have shown that between 1990 and 2017 applied research has become more dissociated from economic theory. Thus, not only has theoretical research been losing importance since the early 1980s, but more recently it has become less important in orienting applied research.

Further research would be necessary to better understand the role of the core journals on shaping economic discourse, examining in more detail how ideas spread from the top journals to the economics profession as a whole, how and when these outlets came to occupy a central position in the network of journals, and whether or not their editors favor certain lines of research. My contribution was a first step in showing the role of the top journals in the formalization of economics by acting as vehicles of diffusion of economic ideas. In this sense, there may have been a "standing on the shoulder of giants" effect through which the publication of papers in these journals led to extensions, revisions, applications and discussion in the other journals. Moreover, I have found some evidence of the rise and death of theory and the continuous increase of applied papers between 1940 and 2017. Finally, I have argued that my findings are relevant not only to shed some light on the formalization economics from a historical perspective, but also to assess the contemporary state of economics. What the next chapter of this story will be remains to be seen, but given the hierarchy of the economics profession there seems to be little doubt about who is going to write it.

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